

TOULMIN, who must know that he has committed a great error, which is both degrading to himself and injurious to the character of the whole profession.

We have, however, a word to say to our contemporary, who so blandly suggests to his readers that the Blackheath practitioner's conduct is typical of the behaviour of English medical men generally; and who would "look back to the cause of this deplorable degradation of the medical profession in England, with a view to prevent a similar result in Ireland."

The readers of the *Dublin Medical Press* need not, we trust, be informed, that the surgeons in general practice in this country, as a body, entirely repudiate the conduct of Mr. TOULMIN, and view it with feelings of unmitigated disgust.

Our attention has been directed to the strange medley of names—quacks and regulars—which figure as the medical officers of the London City Mission. This association employs a large number of missionaries, for the purpose of extending religious instruction among the various districts and classes of the metropolis. The Jews, the police, the cabmen, the soldiers, all have their special missionaries devoted to their moral and spiritual improvement; but the system adopted seems very likely to demoralize instead of to regenerate the medical part of the mission. The medical examiners, upon whom we suppose the medical affairs of the association chiefly rest, are Dr. RISDON BENNETT and Dr. BURFORD CARLILL. The honorary medical officers form, however, a very heterogeneous mixture. Among the honorary physicians we have Dr. RISDON BENNETT, Dr. CONQUEST, and Dr. CAMPS, all stout allopaths; but they hold office with Dr. CURL and Dr. MALAN, the homœopaths. Among the surgeons, we find Mr. MARTIN WARE, Mr. LEGGATT, the Messrs. MERRIMAN, and Dr. OGIER WARD, cheek-by-jowl with Mr. BATCHELOUR and Mr. JOHN SAY CLARKE, both of whom are notorious homœopaths. Surely this state of things ought not to continue. To please the eclectic tastes of the missionaries, medical men have no business to mix themselves up with quacks and quackery. As the matter now stands, whenever a missionary is taken ill, there is quite a discussion amongst himself and his friends as to whether he shall send in his adherence to the homœopathic or to the regular portion of the staff. We trust this notice will produce some amendment, or we shall be forced to return to the subject in a different mood.

#### THE ANALYTICAL SANITARY COMMISSION.

THE VINEGARS OF MESSRS. HILL, EVANS, AND CO.

To the Editor of THE LANCET.

SIR,—We accept the declaration of the Analytical Sanitary Commission, that we "*do not use sulphuric acid in the manufacture of our vinegar*," as a withdrawal of the unjust imputation contained in their report, published in your number of August 28; but in the absence of the details of their analysis, we must not be understood to assent to its accuracy, because, in our case, it is opposed to the elaborate reports of several eminent analysts of acknowledged reputation.

The statement of the Commissioners, that the Worcester water contains an *unexampled* amount of combined sulphuric acid, is at variance with the analysis of the Burton water reported in THE LANCET of May 5th last, and the distinction drawn in the vinegar analysis of some samples between *free* and *combined* acid, seemed to us an indication of the Commissioners being fully aware of the necessity of ascertaining that point before declaring the vinegar

operated upon to be adulterated with sulphuric acid; so that by directing their attention to this important consideration, on which indeed depended the whole value of their researches, instead of being thought candid, we might, with apparent justice, have been charged with implying a doubt of their competency to conduct the analysis they had undertaken.

We would further remind you that our letter of January 3 was duly acknowledged by THE LANCET, in the Notices to Correspondents of January 10, so that no blame rests with us that both it and the samples did not reach the Analytical Sanitary Commission. The letter of Sir Charles Hastings to you was not sent to vouch for the purity of our vinegar, but for the credibility of any statements made by us in the communication which accompanied it. We think it must now be conceded that, viewing the object sought—THE PROTECTION OF THE PUBLIC HEALTH, and the grave consequences to parties involved in the investigation—the best means were not adopted by the Commissioners for obtaining authentic samples of vinegar for rigid analytical purposes.

We are, Sir, your obedient servants,

Worcester, Sept. 20, 1852.

HILL, EVANS, AND CO.

While nothing could have been more to the purpose, or more proper under the circumstances, with the single exception to which we referred at the time, than the letter of Messrs. Hill, Evans, and Co., to which we replied in the LANCET of Sept. 18th, we are sorry we cannot say as much of the communication printed above.

What do Messrs. Hill, Evans, and Co., mean by stating that they must not be understood as assenting to the accuracy of our analysis of their vinegar? Do they mean to imply that their vinegars do not ordinarily contain from two to three grains of sulphuric acid in the 1000; that this acid does not exist in a state of combination, and that it is not most probably derived from the water employed in the manufacture of the vinegar? These are the conclusions to which our analyses have led us; and what is there in these results which Messrs. Hill, Evans, and Co., can possibly charge with inaccuracy?

Messrs. Hill, Evans, and Co. remark: "The statement of the Commissioners, that the Worcester water contains an *unexampled* amount of combined sulphuric acid, is at variance with the analysis of the Burton water, reported in THE LANCET of May 5th last." By reference to the report alluded to, we find that one sample of Burton water analysed contained about sixteen grains of combined sulphuric acid in the imperial gallon of 70,000 grains, while the second contained about twenty-seven grains of acid; but the water used by Messrs. Hill, Evans, and Co., to account for the amount of combined sulphuric acid present in their vinegars, must hold in solution something like from 140 to 210 grains of combined acid. The comparison of the Worcester with the Burton water does not hold good; therefore we repeat, the former, so far as we are aware, contains an amount of sulphuric acid in combination altogether unexampled.

In all other samples of genuine vinegar which we have analysed, and they are not few in number, we have never found, as already stated, the quantity of combined sulphuric acid in 1000 grains, by measure, to exceed half a grain, it being most frequently even much under this. In the three samples purporting to be the vinegar of Messrs. Hill, Evans, and Co., the combined acid ranged between two and three grains—a quantity altogether unexampled.

This peculiarity in the composition of the vinegars of Messrs. Hill, Evans, and Co. was entirely the cause, and a very sufficient and natural cause, too, of the accidental conclusion at which we originally arrived; and on a perusal of our analyses, it must at once have occurred to Messrs. Hill, Evans, and Co., that the peculiar character of the water used by them had misled us, as it had done others before; and it was in not referring either to the very remarkable CONSTITUTION of the water, or the important fact that it had before led to error, we conceived that they had exhibited some want of candour, an opinion we see no reason to alter.

To the last paragraph of the letter of Messrs. Hill, Evans, and Co. we entirely demur. So far from conceding that, having regard for "THE PROTECTION OF THE PUBLIC HEALTH," the best means were not adopted for obtaining authentic samples of vinegar for analysis, we maintain, on the contrary, that we

adopted not merely the very best means, but we believe the only available means, of securing the vinegars of the several makers in the state in which they were vended, and of guarding the public against deceptions and mistakes.

### Correspondence.

“*Audialteram partem.*”

#### FIBRINOUS CONCRETIONS.

[LETTER FROM DR. GARSTANG.]

To the Editor of THE LANCET.

SIR,—You will perhaps allow me to make a few remarks on the letter written by Dr. Ogle, and published in THE LANCET of September the 4th. In answer to his first inquiry, as to the colour and consistence of the fibrinous concretion to which I referred on a former occasion (THE LANCET, August 28th), I would say, that it was of a dirty white, or rather, perhaps, of a cream colour, and that in consistence it resembled (as near as I can describe), the white part of an egg that has been boiled. To apply the term “leathery consistence” to the concretion would be an exaggeration, but it was very firm, and, as I said before, it had an attachment to the cardiac walls. I could not say that the concretion had undergone contraction. It was modelled to the shape of the cavities in which it lay, and did not quite fill them. Dr. Ogle asks if the concretion was “laminated.” If by this he means, did it exhibit, on section, distinct layers (like those of an onion)? I answer no; neither was it organized, or otherwise changed in structure; it was simply a firm, tough mass of fibrine, stringy or fibrous in appearance, when torn across.

The blood was fluid. The arterial system was not examined minutely, but with sufficient care to show that no deposit or other form of obstruction existed at the roots, or in the first parts of the great vessels. I regret that the heart was not examined microscopically; but I may observe that it did not exhibit those appearances of flaccidity and of softness of structure which are, I think, always met with in cases where death has resulted from mere failure of the muscular power of the central circulating organ. The inspection was made seven hours after death.

And now, having answered Dr. Ogle's inquiries so far, I may perhaps be permitted to ask him, whether, from the “hundreds” of inspections which he has made, he has kept particular notes on the subject of “Fibrinous Concretions” in the heart. I strongly suspect that he has not, and that he speaks from memory merely, not from the *litera scripta*. If such be the case, I can only say that his conclusions are worthless. Like Dr. Ogle, I have seen very many inspections, (having had somewhat favourable opportunities of doing so,) and have also often seen concretions of fibrine removed from the heart; and, dare I judge from memory merely, I am sure that I should arrive at conclusions on this matter quite contrary to his.

But the fact is, that until just lately, I (like the generality of my medical brethren, I believe) have attached no importance to these concretions, and therefore I cannot speak of the cases in which I have seen them with any degree of correctness. At the same time I know that observers who do take particular notice of such formations, hold opinions quite contrary to those held by Dr. Ogle. Grisolle cases of pneumonia afford, I think, striking evidence on this point.

As Dr. Ogle supposes, *I do believe* that in some cases, the “sinking state” may arise from the mere formation of a fibrinous concretion in the heart; although I am also quite willing to admit, that in other cases such concretions may only affect life secondarily—acting, i. e., as the “final stroke”—an opinion which never has been, and never can be, doubted by those who will admit that concretions of the kind in question form in the heart during life. I cannot for one moment entertain the idea that in the case which I have related, the adhesive state arose from the presence of erysipelas poison in the blood. The erysipelatos inflammation had, as I remarked before, passed quite away, a month previous to the death of the patient, and during this time the tongue was clean, and the secretions were natural—conditions which surely could not co-exist with erysipelas in the blood. I do not believe either, for reasons already given, that fatty degeneration of the heart was the cause of death. Borrowing the theory of a “fibrinous diathesis,” I can also say, that if Dr. Ogle can establish such a theory on facts, his labour will prove of great service to the profession.

The absence of pulmonary and cerebral symptoms in the case which I related is less strange in particular than it is in a general

point of view. It may seem curious, I admit, that the heart should be choked with a clot of fibrine, and death result without the occurrence of signs indicative of pulmonary or cerebral derangement. But equally hard is it to suppose the same symptoms absent in cases where death should result from fatty degeneration of the heart, or from erysipelas poison.

I could, moreover, refer to instances in which formations other than fibrine have existed in the heart during life, and have given rise to death; pulmonary and cerebral disturbance being (as in my case) absent.

In conclusion, I have pleasure in saying, that I greatly admire the courteous manner in which Dr. Ogle's letter is written, and that I trust he will receive this reply with all friendly kindness.

Yours very truly,

W. GARSTANG, M.D., &c. &c.

Dobcross, near Manchester, Sept., 1852.

#### INADEQUATE PAY OF MILITIA SURGEONS.

To the Editor of THE LANCET.

SIR,—I observed with great pleasure an article in THE LANCET of Sept. 18th, on the inadequate pay of Surgeons of the Militia; for which, allow me to offer you my sincere thanks, and to add my testimony to the truth of your observations, as being one practically acquainted with the miserable and illiberal treatment by the War Office of our profession.

I will adduce, as an exemplification of your statement, that I have been engaged for three days as follows:—On the first day I examined more than fifty recruits, which occupied between four and five hours (the best part of the day); the second day I was engaged about three hours; and the third, I travelled forty miles by rail, (the railway expenses *only* being allowed,) which occupied seven hours, and for which, as you are aware, the pay is 11s. 4d. per day; with an allowance of 5s. for my expenses at the inn on the last day. Now, having a very fair private practice, you, Sir, can easily imagine to what a sacrifice I am subjected for the paltry pay above mentioned. I really can only account for the above regulations of the War Office as having been hastily formed, without the slightest knowledge as to how a militia surgeon, entirely dependent on his pay, could possibly exist even for so short a time, or how he could survive the remaining months of the year, after the enrolment and twenty-one days' training had concluded, (the pay then ceasing,) or how a private surgeon could afford to neglect his practice for so paltry an inducement. I do trust, Sir, that through such able advocacy as your own, the Secretary at War may be induced to see the error of his way, and speedily remedy such gross injustice.

Trusting that you will be good enough to insert this in your valuable journal,

I am, Sir, yours obediently,

September, 1852.

A MILITIA SURGEON.

#### DEATH PRODUCED BY EXTRAVASATION OF BLOOD IN THE BRAIN.

WHAT WAS THE CAUSE OF THE EXCESSIVE VOMITING IN CONNECTION WITH PROFOUND COMA?

To the Editor of THE LANCET.

SIR,—Under this head, in THE LANCET of Sept. 11th, there appeared a communication from Dr. Robert Molloy, who at the end of the report states that, “although the examination has very satisfactorily revealed the cause of death, it has by no means explained to my mind the singular and unusual persistence of this symptom.” Now, on carefully perusing his communication, it seemed to me that the post-mortem revelations did most satisfactorily explain the existence and “persistence” of the vomiting during life, and that this very interesting case affords another example of the value of pathological observations in confirming physiological and anatomical researches. A large clot of blood was here discovered in the left ventricle; “this clot was estimated to weigh three or four ounces, and was traced through Monro's foramen into the right ventricle, from thence into the third ventricle, and it eventually extended into the fourth.” According to the investigations of Stilling, the small triangular eminences in the floor of the fourth ventricle are special deposits, or nuclei, of grey matter, connected with the roots of the eighth and ninth pairs of nerves; and from Dr. J. Reid's experiments, the gastric branches of the vagi nerves appear chiefly to influence the muscular movements of the stomach, and, consequently, the phenomenon of vomiting; in fact, Dr. Todd, in “The Physiological Anatomy and Physio-