depend upon a change in the substance of the muscles themselves, how does it happen that we can produce a temporary muscular power by agitating and rousing the patient? Again, the example of muco-enteritis proves (as nearly as proof is admissible) that the nervous and not the muscular system is that which is affected. It is well known that any affection of the alimentary canal, a structure so well supplied by, and so nearly related to, the nervous centre in the abdomen, speedily influences the whole system. A violent blow at the epigastrium has produced a speedy death; a few drops of a foreign fluid in the abdomen may immediately produce most severe nervous symptoms—facts which furnish us with abundant evidence against the opinions which have been adduced. Nor can I agree with Mr. Ross, “that the stage of typhus in which the muscles is most decidedly influenced in the condition of the nervous system is that of relaxation.” I believe that in the state of subsultus tendinum the nervous system is more affected, and the muscular more influenced by the nerves, than in the state of relaxation, but that the muscular and nervous systems in typhus always bear a strict “concurrent relation to each other.”

The mechanical cause of the congestion and effusion met with in the brain, supposed to depend on the absorption of fat, is the next point to which I shall refer. Recollecting the various provisions made to prevent the production of disease from the sudden propulsion to, or subtraction of blood from, the brain (a subject which has lately been well discussed by Dr. George Burrows in the Lumleian lectures*), I may fairly ask, how are the experiments of Mr. Ross to prove that there is less fat in the cerebral substance in typhus fever than in health or in other diseases? Again, why does congestion as constantly occur in the lungs and in other organs of the body?—and has it been proved that they have also lost their fat? Chemical investigation would have cleared away many of the difficulties; such examination would not have entailed much labor or difficulty, but no experiments appear to have been made. Moreover, how is it that in all chronic diseases in which, as Liebig states, the fat of the body, the substance of the muscles, the nerves, and the brain, are unavoidably consumed, the space left in the brain (vacuum) is not filled by blood, and congestion produced? Moreover, Mr. Ross states “that the injection of the brain was greatest in those cases in which death ensued quickly.” If the congestion really arose from this cause, it ought to be found most abundant in those who died slowly, or in those in whom existence was prolonged, because in the latter the absorption and “vacuum” would be greatest. The views of our author here, as in other instances, contradict each other—nay, more, they contradict facts. This observation of Liebig is opposite to our experience; one, amongst many other examples, may be quoted in the cure of phthisis, in which disease some of the organs of the body are often found enlarged.

We are next told, concerning the lymphatics and lacteals, that when the glands of the mucous membrane are in a state of suppuration, the minute plexus of vessels and absorbents in the mesentery is more injected than usual. This effect is, then, considered

* Medical Gazette.

of the same nature as that arising from the action of a poison applied to the surface—as, for example, in the case of mercurial inunction, &c. Thus it is, we are informed, that typhus, however introduced, causes inflammation of the absorbent lymphatic system of the abdomen.

The assertions relative to the lymph containing as much fatty matter as blood, I cannot argue upon, because it is well known that in the accurate analysis of our countryman, Dr. Rees, in whom I would place great confidence, the fatty matter present amounts but to a very small quantity. Moreover, I quite agree with the original assertion of Hunter as regards the absorption by the lymphatics, in health, of the waste matter of the system. Of course, in disease we know the lymphatics do absorb the fat, but this has been long known; the hibernating tale is well nigh as old as Adam.

In the commencement of the third paper, we are told that “the removal of the eruption about every fourth day, as stated by Chomel and Louis,” is a stronger evidence than the very existence of the eruption itself in support of the opinion of the eruptive character of typhus. I regard this as an argument of the same class as many of those I have already alluded to, and, as such, may dismiss it, for I have now arrived at the author's doctrine, which is nothing more than the old one of Liebig in a new form. The virus is supposed to act as a ferment, and creates, in the ultimate molecules of the system, the same kind of action as exists in itself. In this manner is explained the action of the variolous virus; and it is worthy of remark, that not one word is said in the paper relative to the mode of action of the virus in the case of typhus; it is altogether disregarded.

The subject of treatment next presents itself, but I hesitate to enter upon the question, inasmuch as this portion of the paper of Mr. Ross appears to me to be still more erroneous and injudicious than are his theories. I have already extended my remarks to a greater length than I had intended, and shall, therefore, defer making any further allusion to the treatment of typhus, at least for the present. In conclusion, I may be permitted to apologise for having thus ventured to occupy attention so long, by discussing theories which, founded in error, seem to carry with them their own refutation; and I should scarcely have intruded these last observations upon the notice of the readers of the Provincial Medical Journal, had I not, in a late number, observed that a very talented contributor had joined in the praise which the ingenuity of Mr. Ross's views may elicit, but which their correctness cannot entitle him to. I make no apology to the author for having thus discussed very freely the opinions he had advanced, believing, as I do, that his object, like my own, has been the endeavour to aid the advancement of medical science.

London, May 26, 1843.

CASE OF FEMORAL HERNIA.

TO THE EDITORS OF THE PROVINCIAL MEDICAL JOURNAL.

GENTLEMEN,—The following case so strikingly exemplifies the importance of the practitioner personally examining the usual seats of hernia in all

instances of sudden abdominal attacks, and not resting satisfied with the assurances of his patient, that it may not be undeserving of being recorded.

Notwithstanding every one admits the truth of this surgical axiom, I have reason to believe, from what has occurred within the sphere of my own observation, that life is not unfrequently sacrificed by forgetfulness of it, or by a false delicacy, leading to an insufficient investigation. The case is likewise curious, showing, as it does, how large a hernia may exist without the patient being aware of its presence.

I am, Gentlemen,

Your obedient servant,

W. H. MORGAN,

Surgeon to the Bristol Infirmary.

Bristol, May 25, 1843.

A few evenings ago I was sent for to see an elderly female, who had been suffering from pain in the bowels and vomiting since the early part of the day. I found her laboring under the symptoms that would attend a strangulated hernia in an early stage. They had commenced gradually, and without any obvious cause, and had progressively increased. Some aperient medicine, which she had taken shortly before my visit, was immediately rejected. She stated that she was never the subject of rupture, and was quite positive that nothing of the kind was present. With some reluctance she permitted me to examine the groins, and on the right side I found a femoral hernia, the size of a pullet's egg, overlapping Poupart's ligament, and very prominent. As soon as she saw it she exclaimed, with evident surprise, "Bless me! I had no idea there was any swelling there." It was a little tender, but not otherwise painful, and was reduced by the taxis in a few minutes, with complete relief to all her symptoms.

PROVINCIAL MEDICAL JOURNAL

SATURDAY, JUNE 3, 1843.

In contemplating the advancement of any great public good, it is ever a most gratifying task to be able to connect each onward step with the praiseworthy efforts of its promoters. Rarely does it happen that labors such as these originate with, or are maintained by, a single individual; but in the consideration of the subject to which we shall beg leave to direct the attention of our readers, we are enabled at once to trace back the origin, development, and increasing advocacy of one of the most important questions in relation to public *hygiene* to the labors of one individual, a member of our own profession. It is a practice too frequently adopted both in matters relating to forensic medicine and medical police, to abnegate the necessity for summoning to the assistance of the legislature the knowledge and experience of the profession. Take, as an example of the former, the difficult question of insanity; it is true, medical testimony may be called for; but it is not permitted

to influence the general principles upon which the law still continues to decide the question of personal responsibility, and this, moreover, with the knowledge that such principles were framed at a period when little or nothing was known of the nature or treatment of mental alienation. The recorded opinions of a Hale or an Erskine are still daily called to aid the judge in deciding upon subjects which pathology alone can correctly determine—still is the *dictum* of the lawyer the admitted substitute for more exact knowledge. Medical opinion and medical evidence exert no positive influence in the decision of such cases, however completely they may be within the province of our art, unless, as in a recent example, a long train of medical witnesses happen to give the same evidence as to matters of observation, and when the part they are made to play is the same with that of any non-professional witness.

The same is true with reference to subjects of medical police, in which professional opinion too frequently is either unsolicited or disregarded. With so little encouragement to proceed in a difficult and unpleasing investigation, he deserves peculiar praise who comes forward as the advocate of any important question in relation to either of these branches of medical science. Perhaps no question has arisen in late years possessing so large a share of importance in reference to the health and, to a certain extent, the moral welfare of a people, as that which relates to the interment of the dead in the midst of the living—a custom disgusting in itself and the undoubted source of many of the fatal maladies so constantly occurring amid the poor and densely populated neighbourhoods in our great metropolis.

In Mr. George Alfred Walker we recognise the untiring and successful opponent of a custom so baneful, and, from the limited views of parochial economists' so general, amongst us. The labors of the author of "Gatherings from Graveyards" are too generally and too favorably known both to the profession and the public, to need any recital at our hands; but it is with pleasure that we are enabled to record his unwearied perseverance, as we also hope ere long to record the complete fulfilment of the excellent project whose advocate he is. Those who have perused the works of Mr. Walker must have become fully conversant with the extent to which intra-mural interment is carried, and of the deleterious agency which such practices produce. That the source of much of the disease occurring in the crowded neighbourhoods of London, more especially in those immediately adjacent to large parochial churchyards, is to be found in the noisome effluvia and gaseous exhalations generated by the continued decomposition of animal matter *very near*, and by no means unfrequently *upon*, the surface of the ground, is most incontestably proved; and influenced as the chemical changes taking place