

recess it should be the business of every medical officer to see the M.P. of his own locality, and lay our case fairly before him.

The Poor-law Board, if report speaks true, has admitted the payment to be inadequate to our services, but adds, whenever a vacancy occurs, there are plenty of applicants, and therefore feels a difficulty in adding to the poor rates by augmenting our salaries. This argument, however specious, I contend is not a fair one. Our profession, like all others, is overstocked, hence men are anxious to take any appointment they hope may lead to something better; but this is no reason why places of trust should be reduced to the lowest possible minimum; and *ours is a place of trust*, since the lives of millions of the poorer classes are committed to our charge. Were the Poor-law Board itself thrown open to public competition, there would be plenty of candidates to supply the place of the officers at half or even a quarter of their present salaries; but that does not prove that it would be right to underpay them, as the Union medical officers now are. Let the duties of every officer be ascertained, a fair remuneration fixed, and, as vacancies occur, the best men chosen to fill them.

There are upwards of 3000 Union medical officers, half of whom at least have petitioned Parliament and memorialized the Poor-law Board for an amelioration of their present position; had the others done the same, it is probable that Parliament would have passed some resolution in our favour last session. Time that is passed cannot be retrieved, but henceforth let it be the duty of everyone to examine the list of Unions, and ascertain those that have not joined the Association, and endeavour to enlist them into our ranks. The officers of each Union should be called together, and, when convenient, meetings of several Unions should take place in each county, and the proceedings reported in the journals, which have nobly and liberally advocated our cause.

One thousand medical men have subscribed amongst them £327 to defray necessary expenses: of this, there is a balance of £150 in the bankers' hands. But that will not suffice to carry on an affair of this magnitude: it ought to be quadrupled at least. With ample funds at command, your Committee will be enabled, through the medium of the daily press, to lay before the public a true statement of your grievances, and prove to the most determinately blind that an average payment of 3s. per case, as reported to the select committee of the House of Commons, is not sufficient to cover the expenses of keeping horses, providing medicines, &c. &c., (as each case takes on an average, as far as my experience goes, from three to four weeks before it is off the books,) and that it does not afford any remuneration for our time and skill. Further, you will be in a position again to petition the House of Commons, and forward letters to its members individually, requesting them to pass such resolutions or laws as will make it compulsory on the Poor-law Board, if they render us no redress during the recess, to carry out the recommendation of the Select Committee of the House, on a uniform system throughout the country, and not to leave us to the caprice of boards of guardians, which, as in the case of the Weymouth Union, most unjustly accords to one officer 1s. 2½d. per case, whilst it gives to another, under precisely similar circumstances, 14s.; and though repeated complaints have been made to the Poor-law Board on the subject, it has hitherto contented itself with merely promising "to take it into consideration."

We have equity on our side, and must in the end be victorious; but to ensure success requires unanimity of action and proof that our payment is inadequate to our services. Returns connected with the Union Medical Officers have been moved for in the House of Commons; but as the questions do not sufficiently elucidate our position, I have submitted to your Committee certain others, which have been revised, and I now send them to you in their amended state, and shall feel obliged by every local secretary forwarding a copy to each of his colleagues; and where there is no secretary, I must trouble the gentleman into whose hands the papers may fall to do the same with his, deducting from any subscriptions that may reach him all expenses attending the execution of the commission, or, failing to receive any, to inform me the cost, and I will send the money, but on no account to omit forwarding them with the list of Unions, all of which, if tied with twine or bound with a slip of paper, and left open at the ends, and having no writing on them excepting the address, will go by post for one penny. I hope no one will object to answer the questions during this month, and to remember it is no idle curiosity that prompts them, but a sincere desire to be armed at all points for the forthcoming struggle, which I trust will be final. I begrudge not the hours I spend daily in the cause, therefore hope the little I now ask may be complied with by all, even should some

go no further, in order that during the recess a correct summary may be made upon which the Committee may shape its course of action.

I am, dear Sirs, faithfully yours,  
The Poor-law Medical Officers of England and Wales. RICHARD GRIFFIN,  
Chairman of the Association.

The following correspondence has taken place between Mr. Griffin and the Poor-law Board:—

12, Royal-terrace, Weymouth, Sept. 26th, 1856.

MY LORDS AND GENTLEMEN,—I addressed a letter to your Hon. Board on the 23rd of July last, enclosing certain forms for an improved Weekly Return Sheet; to this letter I have not yet received a reply—possibly it may have been overlooked; but should it, however, not be your intention to adopt any of the forms, I shall feel obliged by their being returned, as it may probably be necessary to submit them, with other documents, to Parliament after the recess.

I have the honour to be, &c.

RICHARD GRIFFIN.

Poor-law Board, Whitehall, Oct. 1st, 1856.

SIR,—I am directed by the Poor-law Board to acknowledge the receipt of your letter of the 26th ultimo, and in reference to your letter of the 23rd of July last, to inform you, that the Forms which you then transmitted to this Board, have not been overlooked by them, but that they wish to retain them for a short time longer, if they should occasion you no inconvenience by doing so.

I am, Sir, your obedient servant,

R. N. GREY, Secretary.

12, Royal-terrace, Weymouth, Oct. 2nd, 1856.

MY LORDS AND GENTLEMEN,—I beg to acknowledge the receipt of your letter of the 1st instant, and am happy to hear that the Forms have not been overlooked, and that you wish to retain them a short time longer, which, I trust, implies there is a chance of one of them being adopted, thus affording a slight relief to the Union Medical Officers. Their prayer for a uniform and equitable rate of payment throughout the country, I hope, will also meet with your favourable consideration, and that there will be no need of their again being compelled to ask the aid of the legislature to enforce the recommendation of the Select Committee of the House of Commons.

I have the honour to be, &c.,

The Poor-law Board.

RICHARD GRIFFIN.

## Correspondence.

"Audi alteram partem."

## EXPERIMENTS ON POSTURAL RESPIRATION.

COMMUNICATED BY

MARSHALL HALL, M.D., F.R.S., &c.

To the Editor of THE LANCET.

SIR,—I am indebted for the following able and careful experiments to Mr. R. L. Bowles, of St. George's Hospital. I think they render any further repetition of them unnecessary, fully confirming as they do the first very accurate investigations of Mr. Fox.

I am, Sir, your obedient servant,

London, Oct. 1856.

MARSHALL HALL.

DEAR DR. MARSHALL HALL,—The enclosed experiments, to each of which I have appended a few remarks, were made by Mr. Hunter and myself, the object being to confirm or correct those previously made for you by Mr. Fox and ourselves, feeling that, from our former experience, we could now guard against several fallacies likely to arise during the experiments. The mode of procedure was the following:—

An india-rubber tube was inserted into one nostril, the opposite one, with the mouth, being closed, and rendered air-tight by plasters, bandages, &c.; the free end of the tube was now attached to a glass syphon, containing a small quantity of water in its bend, and to the opposite end of this syphon was fastened an oiled-silk bag, made in the form of an intestine, to which it was similar in appearance and calibre, when inflated.

This bag, when full, contained forty cubic inches. The water in the syphon acted as an index of the direction of the air in its passage to or from the bag, or, as we have called it in the experiments, one oiled-silk tube; it was the amount of air expired into this tube on pronation, &c., which enabled us to compare it with the natural expiration of thirty inches; for when the tube was half filled, which was easily proved by pressing the air towards its closed extremity, we inferred that we had twenty cubic inches in the tube, and so on. We did not pretend to perfect accuracy as to the amount, but it must be an exceedingly near approximation to the truth.

I think you will also see in these experiments, proof that though the supine position does not in every case prevent the passage of air, it does so in a *large proportion* of cases; and if it do so in one case only, that appears to me quite reason enough why that position should be avoided.

Believe me faithfully yours,

Eaton-place South, Eaton-square, 1856.

R. LEAMON BOWLES.

#### SUBJECT I.

A male, much emaciated, about fifty years of age, and six feet in height. The body was not quite cold, and cadaveric rigidity had but partially come on. He had been dead ten hours. The apex of the tongue reached the incisors, and the body of the tongue was at the floor of the mouth, so that a considerable space existed between its surface and the roof of the mouth. Some fluid was seen at the back of the fauces, and on laying the body on its face, a large quantity of fluids and solids from the stomach made their escape.

*Experiment 1.*—Semi-rotation, prone and lateral. In the former position, the tube was quite half filled, and in the latter it was instantly emptied.

*Experiment 2.*—By alternate pressure and relaxation on the thorax, (the subject supine,) the same result was obtained as in Experiment 1.

*Experiment 3.*—Semi-rotation, prone and lateral, was again had recourse to, and with the same result as before.

*Experiment 4.*—Alternate pressure and relaxation on the thorax in the supine position. This time, not the slightest in- or ex-piration could be obtained.

On removing the coverings of the mouth, the tongue was seen to be in the same position as when we commenced, but there was again fluid in the pharynx.

*Remarks.*—This case demonstrates how (in the supine position) fluids in the stomach might interfere with respiration, for, in Experiment 2, a good result was obtained, no fluid being in the pharynx, but after the further movements of Experiment 3, more fluid &c. had been ejected from the stomach, thereby filling up the pharynx, and totally preventing the passage of air into or out of the trachea. This case also shows that the epiglottis does not in all cases cover the glottis in the supine position, though in a large majority it would appear to do so.

#### SUBJECT II.

A middle-aged man, very much emaciated, having suffered for a long time from abscess of the brain. Rigor mortis still present. The brain had been removed.

*Experiment 1.*—Alternate pressure and relaxation on the thorax (body supine): no effect.

*Experiment 2.*—On pronation, about one-third of the tube, on applying pressure, nearly the whole tube was filled; on removing the pressure the tube was emptied to one-third, and on resuming the lateral position it was quite emptied. These movements were several times repeated, and invariably with the like series of results.

*Experiment 3.*—Experiment 1 was repeated. No effect was at first produced; but on pressure being applied by a sudden jerk, some obstacle seemed to be removed, and expiration was the result to such a degree that the tube was nearly filled, as in Experiment 2; and on removing the pressure, the corresponding amount was inspired. After this, alternate pressure and relaxation, in the supine position, produced inspiration and expiration with ease. On removing the tube, no obstacle was to be seen in the pharynx.

*Remarks.*—Experiment 1 proves that in the supine position *something* does at times prevent respiration in the dead subject; and although in Experiment 3 respiration was produced in this position, it could not be done at the commencement; some obstacle had first to be overcome, probably adhesion of the epiglottis to the back of the pharynx by viscid mucus, as the following observation would tend to prove. On cutting down and removing the right side of the pharynx, with the corresponding halves of the hyoid bone and thyroid cartilage,

in another subject, a tolerable view of the position of the parts was obtained. The epiglottis was in direct apposition, by its laryngeal surface, with the posterior wall of the pharynx, so as quite to preclude the possibility of the passage of air; when, however, the head, was allowed to hang backwards over the edge of the table, the bending of the cervical vertebræ caused the posterior wall of the pharynx to recede from the epiglottis, so allowing free passage of air. If the tongue had been drawn forwards, would the epiglottis have been removed from the pharynx? or would the prone position cause it to fall forwards? Experiment 3 differs from any we have yet had, in so small an amount being expired in this prone position without pressure: the only reason we could see for this was, the removal of the brain; for in all previous experiments we had allowed the head to hang over the edge of the table in the prone position, so that the weight of the head, dragging on the thorax, must have had considerable power in expelling air from the lungs; whereas, in the case before us, the weight of the head was lost (the brain having been removed): as a further proof of this, when weight was added to the head, the amount of expiration on pronation, without pressure, was very considerably increased.

#### SUBJECT III.

A middle-aged man, very short and emaciated; had suffered from extravasation of urine. There was some dulness on percussion on the left side of the chest; rigor mortis present; tongue very stiff, and moved about with difficulty. Whilst the body was being shifted, we could distinctly hear the rushing in and out of the air.

*Experiment 1.*—Very good result (more than half the tube) from pressure in the supine position.

*Experiment 2.*—The arms were folded beneath the forehead and fastened there by bandage. On pronating the body, nearly one-third the tube was filled, and on applying pressure a little more than one-third.

*Experiment 3.*—As a considerable portion of the chest was (by the arms being folded beneath the head) raised from the table, a block was so placed that, on pronation, the thorax should rest upon it. No better result, however, followed this than in Experiment 2, except that by pressure the tube was now nearly half filled. On resuming the lateral position, the tube was of course emptied as usual.

*Experiment 4.*—The head was allowed to hang over the edge of the table, as in all our previous experiments: and now the tube was more than half filled (as in Experiment 1) on pronation with the addition of a little pressure.

*Experiment 5.*—Pressure in the supine position produced the same result as in Experiment 1. On removing the tube, the fauces were quite clear, and free from fluid.

*Remarks.*—As in Case I. and II. of the present series, Experiments 1 and 5 were exceptions to the general rule. Even though the tongue had been moved about, so as to relax it as much as possible, it did not drop back into the fauces, on account of its excessive stiffness. The reason of the but partial success in Experiment 2 was clearly from the large portion of the chest which was prevented from coming in contact with the table; and the result of Experiment 3 being no better, even though a block was placed beneath the chest, would doubtless depend upon the weight of the upper part of the body, which is necessary for the proper expulsion of the air, being conducted away through the medium of the spine to the lumbar region. In Experiment 4, when the chest came well in contact with the table, the result was as usual.

#### SUBJECT IV.

A middle-aged man, had suffered some time from a purulent discharge from the side of the chest, with which cavity the opening was supposed to communicate; and there was considerable dulness on percussion over the same side of the chest. Rigor mortis less marked than usual; the tongue had dropped back into the pharynx, but it could not be accurately ascertained whether it quite stopped the way, or not.

*Experiment 1.*—Pressure on thorax, (supine;) no result whatever.

*Experiment 2.*—Prono-lateral movements, with the head on the table; a little more than one-third the tube, but increased by pressure to nearly one-half.

*Experiment 3.*—Prono-lateral movements, with head hanging over the edge of the table; nearly half the tube full, which took place very readily at first, but afterwards more slowly; on taking out the tube, grumous fluid from the stomach was found in it. After this was emptied out, the experiment was repeated, and the air now passed in and out as easily as at first; so that the tube was nearly half filled.

*Experiment 4.*—Pressure on the thorax (the body supine;) no result, after repeated trials. It being observed that the abdomen was very lax, and so interfering with the amount expired from allowing the diaphragm to descend, a binder was applied, but a large quantity of the same fluid as before was ejected from the stomach into the tube. The subject was pronated, and the fluid allowed to run out from the tube and pharynx.

*Experiment 5.*—The arms were folded beneath the chest, and the pronolateral movements employed, but with very little result.

*Experiment 6.*—Pressure in supine position; no result. On removing the tube, the body of the tongue was far back in the fauces, but no fluid could be seen.

*Remarks.*—It was difficult to say whether it was the tongue or fluid in the pharynx that prevented the passage of air when the subject was supine; but the tongue was much more movable than in previous cases, and therefore farther back. It was evident in Experiment 3, that the fluid was the cause of the difficulty. Experiments 2 and 5 were much inferior to Experiment 3. The amount of air expired was in all these experiments less than usual, probably from the disease of the chest.

### UTERINE PATHOLOGY.

[DR. SCOTT'S REPLY TO DR. TYLER SMITH.]

To the Editor of THE LANCET.

SIR,—My attention has been drawn to an article written by Dr. Tyler Smith, which appeared in your issue of April 26th, at the close of which my name is prominently mentioned by that gentleman in terms which, if left unnoticed, are calculated to do me serious professional injury. Whether Dr. Tyler Smith is justified in dragging into the discussion between him and Dr. Bennet one who has taken no part in the dispute, and who, living at a great distance, was unable to reply at once to the gross charge preferred against him, I leave to the profession to decide. It seems to me, however, that if such a principle is sanctioned, no man's character is safe in future, and the frequency of such attacks will only be in proportion to their impunity.

The lady alluded to by Dr. T. Smith had been under the care of two medical gentlemen in large practice before she consulted me, and had been treated both locally and generally for uterine disease without relief. From one of her medical attendants, I learned that she had had leeches more than once to the os uteri, and that the nitrate of silver had been applied to the ulceration which existed, without benefit.

The patient, on coming under my care, presented the most formidable array of symptoms, from which she had suffered for very many years. The countenance was extremely sallow, debility extreme, pulse feeble, great irritability of stomach, purging, alternating with constipation, sleeplessness, loss of appetite, pain in the back and ovarian region, and a sense of weight and dragging about the loins, with leucorrhœa of a profuse and watery character.

On examination, I found the os, cervix, and body of the uterus extremely indurated; the os hypertrophied, and the seat of an unhealthy ulceration; the cervical canal so obstructed, that it was with the greatest difficulty I could pass the smallest sound.

The treatment consisted in the administration of alteratives and mild aperients, rest, and a mild, nutritious diet. Locally, leeches to the os uteri, and nitrate of silver, with emollient injections. This treatment was pursued for some time without any benefit generally or locally, and suspecting the case to be one of a syphilitic character, I endeavoured to bring the system under the influence of mercury, while I at the same time made a cautious application of the potassa cum calce to the ulcerated surface. The attempt to induce mercurial action signally failed, though I tried the different preparations of the drug, cautiously guarded, in succession. Vomiting and purging invariably came on a short time after its administration had begun, and I was obliged to relinquish its use altogether. The potassa cum calce was applied, and repeated after a proper interval, and I had the satisfaction of seeing the ulceration heal under its influence, and a temporary improvement of the general health take place as the consequence. The induration, however, still remained, and the menstrual discharge was as scanty and irregular as before. Under these circumstances, and finding my patient's health likely to suffer from the heat of Madras, I advised a return to Europe, in the hope that the sea voyage and a complete change of climate would accomplish what treatment had failed to effect. Some time elapsed before

my recommendation could be acted on, and during this period all local treatment ceased; but prior to embarkation, I made, at the patient's request, another examination, when I found the state of the parts unaltered. I noticed neither adhesion nor "mutilation," and had such resulted as the effects of treatment, I could not have failed to notice it. I had come to the conclusion that "scirrhus" existed, and that its existence fully explained the continuance of the extreme induration, and the want of success attending treatment. This opinion I stated in a sketch of the case forwarded by me to England, and on this supposition it is easy to conceive the alteration of structure likely to take place from the progress of the disease. That I would not have instituted local treatment of any but a soothing nature, had I previously suspected the existence of scirrhus, I freely admit. I originally thought the affection syphilitic, and believing it capable of removal by means which have succeeded in cases apparently similar, I thought myself justified in the attempt. Dr. T. Smith may state that no malignant disease existed, but the profession will, I think, be slow to receive with confidence the dictum of one who was, as I shall show, actuated by personal motives in the attack he made, and who, whatever his other attainments, is not considered to have hitherto evinced much practical acquaintance with uterine pathology.

I have been nearly fifteen years in practice, a considerable portion of which has been devoted to investigation of uterine therapeutics, and I can honestly say that this is the first case of "mutilation" brought against me, and that, too, after the lapse of nearly *six* years; though my patients have been scattered over Europe and India, and I had no reason to expect that any mercy would have been shown to my misdeeds, had they been committed. I most emphatically assert, (and I am sure my assertion will be borne out by large numbers of my brethren,) that the *proper* and *cautious* application of the stronger escharotics to the os uteri is unattended with risk, and leaves neither contraction nor "mutilation" behind. A want of caution in failing to isolate within the speculum the part to which the application is made, or the neglecting to saturate the parts, immediately the application is made, with dilute acetic acid, may undoubtedly give rise to deplorable results, from the diffusion of the escharotic over the neighbouring parts; but use and abuse are two different things, and to the latter I certainly cannot plead guilty in practice. Till I am convicted of the latter, no solitary case of long date, however supported by strong assertions, can affect my reputation; and were it not that so extensively-circulated a periodical as THE LANCET had been chosen as the medium of an attack on my character, I should certainly have allowed it to remain unnoticed.

Dr. T. Smith is pleased to disclaim all personal motives in what he has done, and states as his reason for alluding to me by name that he does so because "a great principle is involved, which renders reserve under the circumstances impossible." Surely it was not necessary, for the vindication of "a great principle," that he should gratuitously furnish the details of the husband's fierce indignation and threat of personal chastisement from one in England towards another in Madras! The fact is that Dr. T. Smith was guilty of unprofessional conduct in stating to the patient, before communicating with me on the subject, or receiving her case, that I had grossly maltreated her; and I wrote to protest against it. Hence the animus; hence the attack on an absent man, when he well knew that men whose names ennoble the profession they adorn are in the daily habit of using the escharotics he so strongly condemns, but who are able to defend themselves by an appeal to facts, which even Dr. T. Smith would find it difficult to resist. To my certain knowledge, Professor Simpson has been in the habit of rubbing down half an inch of the potassa on an hypertrophied os without any evil results; and I trust the gravity of the question at issue will induce him to speak out, and for ever silence the extravagant assertions of those who confess they have had no practical experience of the subject.

So much for what is personal. As one who has had the largest opportunities of studying uterine disease as it exists amongst the natives of India, and in private practice, I hope I may be permitted to offer a few remarks as the result of my experience. The question at issue is one of such grave importance that I conceive it to be the bounden duty of the profession to contribute to its solution as speedily as possible by the record of their own experience, and thereby withdraw it from the limited field of single cases of long standing and uncertain history within which it is at present confined. For five years I had the charge of a large female hospital in Madras,