on whom the burden of giving medical certificates to School Board children falls, with a view to a deputation to the School Board on the subject. We shall be glad to receive the names of medical practitioners in the metropolitan area, whether in general or hospital or dispensary practice, who sympathise with us in our desire to make certification effective and to secure due respect to the medical profession. An immediate intimation will oblige.

I am, Sirs, your obedient servant,

JAMES GREY GLOVER, M.D. Edin.

25, Highbury-place, N., Jan. 15th, 1895.

WOMEN AND THE PROFESSION IN INDIA.

To the Editors of THE LANCET.

SIRS,—With reference to a remark made regarding Zenana work in India at an extraordinary Comitia of the Royal College of Physicians of London,¹ held on Oct. 24th last, that "there was no difficulty in the way of medical men entering the most jealously guarded harems when there was need for their services," it is to be observed that such a remark strictly applies only to Bengalees—i.e., Hindoos of Bengal—in Calcutta and Lower Bengal. These same people when in Upper India strictly seclude their women in harems. I submit that in the North Western Provinces, Oudh, the Punjab, and Rajpootana women of the upper and middle classes of all the various nationalities, Mahommedan women especially, are kept in such strict seclusion in the harems or Zenanas that medical men cannot have access to them. A medical man when requested to treat a native woman is expected to judge of all symptoms by feeling the pulse. A screen is made by holding up a cotton sheet across the room; on one side of the screen are the patient and her female friends, on the other the medical man and her male friends. The patient's hand is then passed under the screen and the pulse is felt; if the tongue is to be examined a slit is made in the screen and the patient thrusts her tongue through it for inspection; if the eyes are to be examined the patient places one eye by turns at the slit; if palpation of organs is requisite the medical man passes his hand under the screen. Stethoscopic examination is made only after the patient has been scrupulously covered so that no part of her person is exposed excepting the part to be examined.

In the troubles of parturition a medical man is either not permitted to render manual aid, or, when permission is granted, it is often too late to save life. I have before me a case of arm presentation in the wife of a Hindoo of the shopkeeper caste; permission was delayed for days, and when examination was made the foetus was putrid and the mother in the last stage of exhaustion. Delivery was effected by evisceration of the uterine contents, and the mother succumbed to tetanus. Now, had there been a qualified midwife or lady medical practitioner at hand, the mother at least would have been saved. Another painful case was that of a Mahommedan woman in labour, a primipara, whom I was asked to treat. She had been some days in labour, and the head was said to be presenting. No manual examination could be permitted. I could do nothing except cautiously give a dose or two of ergot. No effect following its administration, I declined to do anything further unless allowed to examine the patient. From the state of the pulse I gathered that she was rapidly sinking. I brought this fact home to her friends and begged that an examination might be allowed, and I said that if the native midwife had correctly reported as to the presentation safe and rapid delivery could be effected. Her father-in-law and husband said that they would be disgraced if they permitted the examination. *The woman died undelivered* while I was still in the house. But, irrespectively of parturition, there are, we know, innumerable conditions where, to afford efficient treatment, it is imperative that the medical man should see and thoroughly examine his patient. Not to speak of surgical treatment, even medicinal treatment often requires examination not practicable under the Zenana system. The well-to-do classes will not allow it, and hence a vast amount of suffering within reach of relief goes unrelieved. The poorer women of those classes who attend at our dispensaries often come when it is too late; or, when they attend at a dispensary in which there is a native female hospital assistant they stipulate that they shall not be

subjected to examination by a male. If there is no difficulty in medical men entering harems the whole of Lady Dufferin's noble philanthropic scheme for affording medical aid to Indian women must be declared uncalled for; but the response made to the call for funds for the scheme by the wealthy natives and princes of India speaks unmistakably of the usefulness of the institution, and Lady Dufferin's name will never fade from the memory of India's daughters.

I am, Sirs, yours obediently,

G. D. McREDDIE, M.D. BRUX.,

Late Civil Surgeon in India (retired).

Stone-court, Greenhithe, Kent, Dec. 26th, 1895.

FRACTURES OF THE LOWER LIMB.

To the Editors of THE LANCET.

SIRS,—In a lecture on Fractures of the Lower Limb by Mr. Christopher Heath, published in THE LANCET of Jan. 4th, 1896, I find the following: "I would remind you how important it is, in fracture of the leg particularly, that the fracture should be set thoroughly and accurately. Of course, I know well there are many difficulties. Immediately after the accident all the muscles of the limb are more or less in a state of spasm, and tend, therefore, to pull the bones into abnormal positions, but that state of spasm passes off in the course of a few hours, and you can generally manage with care and patience to put the limb in a proper position, and unless this is done, and done accurately, the surgeon has not treated the case properly." And again: "I may say that this method of pegging and screwing has been recommended by an enterprising surgeon, not only for compound, but for simple fractures. But I cannot conceive how anyone can believe that it is justifiable to convert a simple fracture into a compound fracture, and of this I am quite certain, that the majority of surgeons for the present will remain content with the usual methods of treatment."

Now, Sirs, I agree most thoroughly with a portion of Mr. Heath's statement as far as I understand it—namely, that *if the ends of the fragments are not put in accurate apposition and the bone restored to its normal shape the surgeon has not treated the case properly*. But I maintain, as I have done over and over again, that in the large proportion of oblique fractures of long bones, especially of the lower extremity, it is impossible to do this by means other than operative. I would quote the following lines from a short paper on the subject of the treatment of fractures published by me:¹ "It is very possible that I may be accused of exaggerating the evil results of the treatment of fractures in the present day, and that many may not be inclined to agree with the mechanical principles upon which my statements are based. It is, however, quite open to anyone to prove the former by publishing an account of a series of fractures sustained at the same period of life which show better results, and the latter are capable of absolute demonstration." Surely, as I pointed out here, the question of the possibility of reconstituting the form of the bones by means of splints is, if true, capable of complete verification. As Mr. Heath asserts that he is able to do this, may I ask him to produce, at a meeting of the Clinical Society of London, a series of half-a-dozen cases of oblique fractures of the tibia and fibula, and a similar number of Pott's fractures, which he has treated in sequence, and give the Fellows of the Society an opportunity of forming an opinion on the success of his treatment and on the wisdom of the statement which he makes—namely: "But I cannot conceive how anyone can believe that it is justifiable to convert a simple fracture into a compound fracture," &c.? We are all anxious to learn the best modes of treating fractures, and Mr. Heath's very clear statements of his views on the subject give us an excellent opportunity of doing so.

I am, Sirs, yours faithfully,

W. ARBUTHNOT LANE.

St. Thomas's-street, S.E., Jan. 14th, 1896.

"A PLEA FOR THE PASTEUR TREATMENT OF HYDROPHOBIA."

To the Editors of THE LANCET.

SIRS,—Dr. H. Howard Murphy's argument in pleading for the Pasteur treatment of hydrophobia would be cogent if the

¹ THE LANCET, Nov. 2nd, 1895.

¹ Brit. Med. Jour., April 20th, 1895.

premises on which it is founded were sound; but I doubt whether this is the case, and I should like to point out where they seem to be at fault. In the first place, I would disclaim all sympathy with the objects and methods of the Anti-vivisection Society, though I admit that their compilation of deaths following Pasteurism is useful, for this has been neglected by everybody else. In the last nine or ten years—i.e., since M. Pasteur's treatment was commenced—very nearly 17,000 persons have been subjected to the treatment at the Paris Institute, and 283 of these have died from hydrophobia. Dr. Murphy assumes that 20 per cent. of these 17,000—i.e., 3400—would have died but for this treatment (I presume that the figures 30 per cent. and 3200 given in his letter are printer's errors). This amounts to an average of 340 per annum. This assumption is derived partly from *à priori* reasoning from principles based on M. Pasteur's experiments, and partly from guesses at the probable frequency of the development of hydrophobia in those persons who have been bitten by presumably rabid animals. There is, as far as I know, no *à posteriori* evidence to confirm it, and in the absence of such evidence it cannot be held to be proved that M. Pasteur has saved a single case. It is quite possible that there may be some fallacy in the experiments, or that the frequency of hydrophobia among bitten cases has been largely over-estimated. Now, of these 17,000 by far the majority (nearly seven-eighths, I believe) come from France alone. It would, therefore, follow (according to Dr. Murphy) that M. Pasteur's treatment has saved nearly 300 natives of France (seven-eighths of 340) from hydrophobia every year. How many other cases of hydrophobia there may be every year in that country which have not undergone preventive inoculation, I do not know. This number seems incredibly large for a population scarcely, if at all, out-numbering that of the British Isles, and, moreover, one has not heard of any great prevalence or extensive outbreak of rabies in that country.

Can it not be shown by official statistics whether or not the mortality from hydrophobia in France has been reduced to a corresponding degree by 300 or by any other number per annum since the introduction of Pasteurism? If it can be shown to be so reduced it would, of course, furnish, in the absence of any other explicable cause, strong, almost conclusive, support to the theory of M. Pasteur's followers, and I should be most happy to know it. Dr. Murphy further says, with regard to fatal cases of hydrophobia, that "till 1880 (1886?) every conceivable plan of treatment had been tried, and none proved in the least effectual." And, again: "Every one of the 283 died in spite of every effort to save them." I demur to this. There are many cases on record which have been successfully treated. I collected a series of them and published an abstract of it in your columns in March, 1890.¹ Since then I have read reports of several fatal cases, and in not one of them has any real effort been made or have any efficacious means been attempted to save them. It is really painful to hear of the utterly ineffectual and futile methods that are the only ones resorted to with the hope of curing or relieving the patients. In such desperate cases history shows that anything but skilful and heroic treatment is seldom of any avail, very free blood-letting being the sheet-anchor. Dr. Murphy says that "the medical profession is not biased in favour of antirabic inoculation." I do not agree with this statement, for it appears to me that an undue amount of faith is almost invariably placed in any novel method of treatment emanating, with a scientific hallmark, from a pathological laboratory—witness the tuberculin fiasco. And, on the other hand, there is still a deplorable prejudice, owing to ignorance and timidity, against blood-letting, the disuse of which, as Sir B. W. Richardson truly said twenty years ago, makes the present age the most feeble and ineffectual, in the matter of treatment, in the whole history of medicine.

I am, Sirs, yours faithfully,

F. LUCAS BENHAM, M.D. Lond.

Elizabeth-street, S.W., Dec. 23rd, 1895.

P.S.—The number of deaths from hydrophobia among persons who have undergone M. Pasteur's treatment is given in the *Annales de l'Institut Pasteur* as eighty-four up to the end of June, 1895. Why is this number alone given, instead of the much larger figure of the Anti-Vivisectionist Society's statistics? Does not the Pasteur Institute follow up the subsequent history of its cases?

¹ THE LANCET, March 1st, 8th, and 15th, 1890.

"ANTI-STOOP" v. SHOULDER-STRAPS.

To the Editors of THE LANCET.

SIRS,—I regret that Mr. Bernard Roth in his letter to THE LANCET of Dec. 14th last should have so erroneously described my "anti-stoop" as to apply to it the term "useless straps." If he will examine the appliance himself at Messrs. Arnold and Sons, or even the diagrams which appeared with your notice of it on Nov. 23rd, 1895, he will see that the only straps about it are the belt round the waist and the back strap connecting the appliance to the belt, neither of which are in the slightest degree essential to its efficacy. With regard to Mr. Roth's objection to all mechanical aids to correct roundness of the shoulders I have nothing to say. His quotation of an eminent authority in support of this view has been sufficiently dealt with by Mr. Heather Bigg in THE LANCET of Dec. 21st, 1895. But I might observe that his quotation had reference to an appliance very different from my "anti-stoop." The circumstances which caused me to invent my "anti-stoop" did not have their rise in a desire to reap riches from its sale. Having always been round-shouldered, I had endeavoured in vain to obtain a cure by shoulder-straps and had suffered much pain from their use. It then occurred to me that a pair of steel arms coming up the back and branching out below the neck, so as to pass over and to the front of the shoulders and there to exert a "springy" elasticity to press the shoulders backwards, would not only be painless if the pressure were transmitted by means of padded blocks, but would probably be effective also. I accordingly had a pair made and wore them for a little over two months, by which time I had received incalculable benefit. I was fully two inches broader across the chest after than I had been before, and narrower to about the same extent across the back. In fact, the coats which I possessed before donning my "anti-stoop" were at the end of these two to three months quite unwearable.

Just a few words regarding the evanescent character, as Mr. Roth declared it must necessarily be, of any such improvement brought about by mechanical means. I have not once worn my "anti-stoop" during the last nine or ten months, but, so far as my friends or I myself have been able to notice, there has been no reversion to the round-shoulderedness which before had been so conspicuous. I attribute this result to the fact that my "anti-stoop" does not splint the shoulders back and keep them so—thus superseding the muscles of the back, as Mr. Roth seems to assume—but that, by acting as a continuous and slightly assistant reminder to the wearer to hold himself or herself up, it stimulates the use of those muscles and thus strengthens and invigorates them.

I am, Sirs, yours truly,

W. A. CARSON,

Eccles, Dec., 1895.

Inventor of the Carson "Anti-stoop."

THE PROTECTION OF THOSE NURSING PATIENTS SUFFERING FROM DIPHTHERIA AND OTHER SPECIFIC INFECTIOUS DISEASES.

To the Editors of THE LANCET.

SIRS,—Some few years ago, when in correspondence with my friend, the late Professor Tyndall, on the above subject, I asked him whether he thought it probable that a suitable covering of antiseptic cotton wool to the nose and mouth of those engaged in nursing sufferers from the above diseases would act as a filtering agent to the micro-organisms. His reply was in the affirmative. I am anxious to know whether your readers have had experience of this method, and more especially so in cases of typhus fever and diphtheria.

I am, Sirs, yours truly,

St. Leonards, Jan. 15th, 1896.

SAMUEL LEE.

THE MEDICAL EDUCATION OF WOMEN.

To the Editors of THE LANCET.

SIRS,—Now that most of the portals of medicine are open to women, and that it remains but for time to speak as to the viability and vitality of the movement, it may not be out of place to suggest that the deans of the schools of medicine for women should recommend some age limit under which