masseur with the theoretical knowledge of an intelligent educated physician, says: "Not a few of the profession applaud Dr. Mitchell and Professor Playfair in a manner that would lead us to infer that they are to be considered the original inventors of massage, and give their whole sole and exclusive attention to it and the cases in which it is useful. With equal propriety might they be regarded as experts in chemistry and materia medica because they use medicine as well as massage, or specialists in electricity because they have called largely on this medical agent to assist them." Shampooing or medical rubbing, which is very useful in its way, can readily be acquired by anyone "in a few lessons," but the true massage, as practised by Professor Von Mosengeil and others on the Continent, depending as it does on a knowledge of anatomy and physiology, certainly cannot be mastered in less than two years.

I am, Sir, your obedient servant,
Weymouth-street, W., Oct. 11th, 1886. WM. MURRELL, M.D.

To the Editor of THE LANCET.

SIR,—Having had some considerable experience in the practice of massage and manual treatment, I beg to protest against the statement by Dr. Sturges in last week's edition that the system can be acquired in a few lessons. The argument used by Dr. Sturges is curious. If it takes "two years" to learn, "why not ten"? he says. So one may ask, if it takes four years to make a surgeon, why not forty? Again, he says: "It may be confidently asserted that there is no digital use whatever needing two years for its acquirement that does not go on advancing towards perfection so long as the mind which informs the fingers retains its vigour." Grant all this. The masseur does become more perfect through years of work; but the point is, how soon is he able to practise the art with benefit to the patient? This question he answers by saying it is a knack, and is acquired, like lawn-tennis or cricket, "in a week." Now, doubtless many are quicker at acquiring it than others; but the benefit the patient can derive must be in proportion to the skill shown, and it seems utterly unreasonable that a method of treatment which requires delicate manipulations of different kinds, and a considerable knowledge of physiology and anatomy, can be sufficiently acquired in a week. Much harm undoubtedly is often done by inexperienced operators. I do not refer especially to the method employed by Dr. Weir Mitchell, and described in his little book entitled "Fat and Blood," but to the whole practice of mechanical treatment, which, though it has existed in rude form for ages, yet really came into prominence and assumed scientific form at the beginning of the present century, when Ling introduced his system at the University of Lund. The methods of Drs. Weir Mitchell and Playfair apply only to a limited range of cases, and consequently the manipulations are limited; yet, even in cases where those limited manipulations were to be applied, I, for my part, should prefer a nurse who had much more experience and practice than could be obtained in a week, precisely on the same ground as when Dr. Sturges administers a drug he would prefer a good one.

The whole system of massage or manual treatment is of very much wider area than that covered by Dr. Weir Mitchell's method. In Sweden, the home of the treatment, it has been employed for many years and for many diseases with admirable results. The Central-Gymnastik Institut (founded by Ling) still flourishes in Stockholm, and furnishes a thorough education in all the manipulations wrought out by Ling and improved upon by his successors. In addition to the practice of the manipulations, physiology and anatomy are carefully taught, and at the end of a two-years' course a diploma is granted by the Institut. Men from that institution are now practising all over Europe, and it is certain that in the struggle between the trained and the untrained there can be only one issue. Why should we not have a similar institute in London, where a system could be taught and properly equipped manipulators sent into the field? If the treatment is to be carried out at all, why not be thorough? If, as Dr. Sturges admits, it is a treatment which "confessedly needs skill, dexterity, and practice," can anyone believe that a few lessons can do this? No; if the "settling day" has to come "when its precise value will be finally determined," that can only be when we have careful observers and trained manipulators. Without these

the settling day will be deferred for ages, or the practice itself will fall into the region of empiricism and quackery.

I am, Sir, yours truly,
Stranraer, N.B., Oct. 1886. THOMAS EASTON, M.A., M.B.

INDUCTION OF PREMATURE LABOUR IN CERTAIN SEVERE CASES OF ALBUMINURIA.

To the Editor of THE LANCET.

SIR,—The publication of Professor Cullingworth's case in last week's issue of the "successful induction of premature labour in an extreme case of albuminuria" induces me to place on record a similar case with somewhat similar treatment.

Mrs. S—, primipara, aged twenty-six, came under my care on Nov. 5th of last year. The case on examination proved to be one of pregnancy complicated with albuminuria. The confinement was expected about the middle of January of the present year, thus making her period of pregnancy somewhat less than seven months. The lower extremities were extremely anasarcaous, the hands and face slightly so. The quantity of urine passed was much less than natural, was of high specific gravity, and highly albuminous, containing a large quantity of casts—hyaline and blood casts. The treatment consisted of diaphoretics, the hot-air bath, free purgation, and free leeching of the loins. Notwithstanding this, the urine gradually diminished and became more albuminous, until, on the 27th instant, the quantity passed in the previous twenty-four hours only reached between three and four ounces. Now came intense headache, impaired vision, vomiting, and a condition highly suggestive of uræmia. Under these circumstances, with no labour imminent, I considered the best treatment would be to induce premature labour. This I did in the usual manner with a gum-elastic catheter, and with the best results, as my patient was delivered within twenty-four hours from the introduction of the catheter of a living female child, apparently about seven months' gestation. The after progress was all that could be desired; the urine was passed in large quantities, and within one week was quite free from albumen.

I am Sir, yours truly,
Bolton, Sept. 1886. ROBERT PATRICK, M.D.

To the Editor of THE LANCET.

SIR,—It seems to me that Dr. Cullingworth's judicious treatment of his patient by hydragogue cathartics and hot-air baths played a more important part in the prevention of convulsions than did the cheerful disposition of the patient, and he has every reason to be satisfied with so favourable a termination of such a serious case; but I would submit that it is not fair to judge of the effect of pilocarpine in the albuminuria of pregnancy from the result of this case, for, to quote the words of the report, "it produced no result whatever," and therefore as the characteristic diaphoresis did not ensue, it is evident the pilocarpine was inert, and as it was given in a sufficiently large dose—a third of a grain—it is fair to assume it was the fault of the particular specimen used.

I am, Sir, yours &c.,
Sunderland, Sept. 18th, 1886. JAMES MURPHY.

"INFANT FEEDING."

To the Editor of THE LANCET.

SIR,—I hope space may be granted me for a short letter in answer to that of Dr. Thurstan, published in your issue of Oct. 9th.

I am quite unable to accept his interpretation of the value of a physiological basis for our actions in dietetics or, by inference, in any branch of medical treatment. Such knowledge has governed me in coming to the conclusions I have, and has made, I trust, my experience of ten years children's work of more value than it would have been without such help. The question that we are discussing is, I gather, that of *hand-rearing* of infants from *birth*, and not when affected by diarrhoea or some such ailment. My remarks, I would say, must be accepted as within this belief. My experience is entirely opposed to Dr. Thurstan's assertions, having been built up after duly testing methods such as he advocates. I am prepared to state that in conjunction with the general measures referred to in my