

because when the depth of anæsthesia is kept moderate (i.e., conjunctival reflex gone, corneal reflex present but sluggish, light reflex brisk), very little of the surgeon's time is wasted if, in a laparotomy for instance, firm but gentle continuous retraction is made upon each layer of muscle as divided, whereby the muscle is paralysed for the time being by local fatigue. Given by an anæsthetist chloroform should be no more dangerous than ether. It is a little more toxic to the tissues, but it does not so frequently produce prolonged after sickness, which is, in my opinion, the cause of the delayed fall of blood pressure which Dr. Fairlie deprecates. This after-sickness is not usually the *direct* result of ether overdosage, but is derived from the sequence or vicious circle: (1) disagreeable taste of ether, (2) consequent dislike of, and abstention from, food, (3) until starvation causes (4) mild acidosis with renewed vomiting. There is to my mind no doubt that the outstanding advantage of the N₂O-oxygen + spinal method of analgesia is not so much its relative (and absolute) safety as the surety of a comfortable recovery.

Mr. G. McCall Smith's opinion that chloroform is always dangerous when deep anæsthesia is required is perfectly sound, but my experience of the ether-chloroform sequence differs widely from his. Probably the explanation of this difference is to be found in the common failure to distinguish between anæsthesia and analgesia. If anæsthetists, by exercising constant watchful care and observation, would make a practice of maintaining a depth sufficient, and only sufficient, for the purposes of each operation—and it is not desirable or necessary that the patient should have to be strapped or held down—then we should not be shocked by reports of fatalities nearly so frequently as is unfortunately the case at present.

In midwifery, too, chloroform is perfectly safe so long as it is never pushed further than "the obstetric degree," or, in other words, never beyond analgesia; but when deeper sleep is imperative, as for the application of the forceps or other operation, then chloroform is peculiarly dangerous, since the patient diverges from the normal in three important respects—(1) the deep but irregular breathing, (2) the temporarily impaired "vital capacity," and (3) the presence in the blood of unusual and frequently toxic bodies derived from the placental circulation. Therefore, in operative midwifery Mr. McCall Smith's decision to use only ether is worthy of wide imitation.

I am, Sir, yours faithfully,

DONALD J. MUNRO.

Upper Tulse Hill, S.W., March 16th, 1914.

METHODS OF INCREASING THE ACCURACY AND DELICACY OF THE WASSERMANN REACTION.

To the Editor of THE LANCET.

SIR,—After our first perusal of Dr. Thiele and Dr. Embleton's paper in THE LANCET of Feb. 21st we must confess that we debated whether we should treat it seriously, and it was only our fear that some of your readers might attach undue importance to it which induced us to record our absolute dissent.

If Dr. Thiele and Dr. Embleton's statements were to stand unchallenged it would obviously appear that for three years we have not only wasted our time, but have also disseminated incorrect diagnoses in a serious percentage of some 7000 cases. The point we wished to make was, not that

the observations of these authors were incorrect under the somewhat abnormal conditions they had selected, but that their wide generalisations, embracing entirely different conditions, were totally unfounded. We still cannot understand whether they profess to uphold their assertions as logical conclusions.

In their replies to Dr. Browning and ourselves they appear to have wandered from the point. With interesting dexterity they seek to confound us by garbling the meaning of a passage in one of our papers, and thus demonstrating that Dr. Browning and we have stultified ourselves. They also quote an alleged statement by Fildes, which was not published over the signature of Fildes and which is misleading. Finally, they assert that we have never investigated the subject, although how they have derived their knowledge of this is beyond our comprehension. It must be remembered that in the course of one's work one makes many observations and verifies a number of fundamental experiments of others without troubling the medical press about them. They represent the controversy as raging round the relative values of their lecithin antigen and cholesterin antigens, and continually acclaim the fact that their antigen will "fix" in a quantity as small as three-millionths of a gramme. To the uninitiated this may appear to be an extremely potent reagent, but on examination we found that the figure for the despised "crude heart extract" without cholesterin could be represented as being almost identical.

If we regard the point at issue to be the relative value of certain antigens rather than the justification for their generalisations, we must point out that on one side stand the observations of Dr. Thiele and Dr. Embleton, while on the other are ranged the investigations of workers in Great Britain, Germany, America, and France. The point therefore resolves itself largely into an inquiry into the relative credibility of these persons.

In this situation we are content to leave the discussion.—We are, Sir, yours faithfully,

JAMES MCINTOSH,

London Hospital, E., March 17th, 1914. PAUL FILDES.

THE SOCIETY OF APOTHECARIES OF LONDON AND UNQUALIFIED MEDICAL PRACTICE.

To the Editor of THE LANCET.

SIR,—I am desired to inform you that at a recent meeting of the Court of Assistants of the Society of Apothecaries of London the following resolution was passed:—

That it is desirable that the attention of the medical profession should be drawn by means of notices in the medical papers to the prevalence of unqualified medical practice and the advisability of reporting cases of such practice to the society, to be dealt with by the society under the penal powers contained in the Apothecaries Act, 1815.

Under the powers referred to in the resolution the society is enabled to recover from an unqualified practitioner a sum of £20, and it has been found in many cases that this penalty has been very effective in putting down quack practitioners. In recent years very few cases have been brought to the notice of the society, but it is not to be supposed that the society has become in any way unwilling to take up such cases, and, in fact, is very anxious to do so both in the interests of the public and the medical profession.—I am, Sir, yours faithfully,

A. MOWBRAY UPTON,
Clerk to the Society.

London, E.C., March 16th, 1914.