Dr. Rickman has, in fact, written of the development of psycho-analysis, and nobody is better qualified to do so. But it is a pity that your "Fifty Years of Medicine" number did not include a review of the development of psychiatry. This would include an impressive body of material which would interest (and perhaps convince) the general reader more than an account of the development of one school of psychopathological theory.—I am, etc.,

London, S.W.3.

D. S. MACPHAIL.

*The signed articles in the symposium on "Fifty Years of Medicine" were concerned with the outstanding developments of the past fifty years. Of these, the work of Freud was merely one. Many of the points made by Dr. Macphail were dealt with in the leading article "Progress in Psychiatry."—ED., *B.M.J.*

The Fate of the Foreskin

SIR,—I should like to range my popgun alongside Dr. Douglas Gairdner's cannonade (December 24, 1949, p. 1434) and join in his volley against the respectable bastions of infantile circumcision. Dr. Gairdner refers to 54 boys between 2 months and 3 years referred for circumcision, in all but one of whom the prepuce could readily be retracted after separation with a blunt probe. Records are not immediately available to me, but I can state that during two years as a senior hospital resident I was requested to perform circumcision on infants (children over 1 year being admitted and not all seen by me) at the rate of two or three each week. No infant under the age of 3 months could be seen, on account of the waiting-list. My practice was to carry out gentle separation of the prepuce, as described by Dr. Gairdner, in order to retract it and apply antiseptic to the glans and preputial skin preparatory to operation. In not one of the series of two hundred or so did I find difficulty in retracting the foreskin. This awoke in me the suspicion—since strengthened by fifteen months' specialized experience of paediatrics—that infantile circumcision is an unwarranted surgical procedure.

On the other hand, a comfortably remote acquaintance with troops serving in North Africa suggested to me that ritual circumcision among nomadic tribes in the Middle East, with inferior bathing facilities, was as sound a precaution in the second world war as in the forty years' wandering.—I am, etc.,

Birmingham.

BRIAN WEBBER.

SIR,—We think that Dr. Douglas Gairdner's interesting article on the prepuce (December 24, 1949, p. 1433) neglects one or two points. It is easy in the infant (if one has spent some time in "Casualty" selecting candidates for circumcision) to distinguish two types. In the first, on attempting to retract the prepuce, its margin thins, a little of the glans becomes visible, and one can feel that a little more proximal traction would peel the prepuce back—i.e., it is simply adherent or, as Dr. Gairdner points out, not separated.

In the second, on attempting to retract, the margin of the prepuce does not thin so much, a definite constriction is usually visible just proximal to the margin, which is a little bulbous beyond it, and, in any case, one can feel that further traction meets with a definite resistance, preventing the preputial orifice from enlarging. This is true phimosis. We do not think a probe is necessary to make this distinction.

That the first type should be treated conservatively, it is easy to agree. The point is whether the second type later becomes retractable. Through not distinguishing these types in his Table (Fig. 5) Dr. Gairdner throws no light on this point. If these are the persistent 10%, then, as they are easy to recognize, it would appear simplest to circumcise them in infancy.

It is true that after circumcision one occasionally finds the glans very raw and slow to heal (say two or three weeks), which would be accounted for by Dr. Gairdner's histological facts. But we find this uncommon, perhaps in about one case in six, and not occurring necessarily in the younger victims. Might it not sometimes be the result of leaving the prepuce adherent for too long?

Furthermore, our impression is that older children (of two or three years) are more likely to get post-operative sepsis and delayed healing and also (this is more than an impression) are much more difficult for the mother to manage during the two or three weeks required for healing. For these reasons it is the practice in the hospital in which we work to circumcise infants with true phimosis.—We are, etc.,

H. M. ROSE.

A. M. GOULD.

London, S.E.13.

SIR,—Dr. D. Gairdner's paper (December 24, 1949, p. 1433) has dealt comprehensively with an operation which has become little more than a vogue. It is indeed appropriate that some attention should be drawn to the fact that circumcision is needlessly performed in the vast majority of cases.

The main pretext upon which this operation is performed is undoubtedly the so-called "non-retractable" foreskin. I would add to Dr. Gairdner's figure of 10% of non-retractable foreskins in boys of 3 years of age by saying that at least 25% of foreskins at this age have not yet become completely separated from the glans penis. The older view that the presence of the foreskin initiates and encourages masturbation is still present amongst anxious and ignorant parents; yet their own forcible manipulations of the infant's prepuce are more likely to encourage this habit, even if it was harmful.

I would also add to Dr. Gairdner's sequelae of circumcision by pointing out that in 80–85% of developed meatal ulcers meatal strictures also develop. It is for these strictures that meatotomy is so frequently performed, the vast majority of these patients having been circumcised in infancy. Congenital meatal stricture is a rarity. I remember some years ago presiding over an out-patient clinic to which a young mother brought her 3-year-old son and requested circumcision of the boy. Finding no reason whatever why the perfectly mobile and clean prepuce should be removed, I asked why she wanted the operation performed. She replied, "Oh, it looks so much nicer."

This attitude towards sexual cosmetics is more primitive. It is generally the circumcised fathers who desire a similar mutilation of their sons. If 84% of public-school boys are circumcised it is time that doctors enlightened them on the anatomical functions of the prepuce and the importance of hygiene rather than subject their male offspring to this fashionable mutilation.—I am, etc.,

Newcastle-upon-Tyne.

J. W. PERROTT.

Insufficient Anaesthesia

SIR,—At the present time anaesthesia for prolonged abdominal cases commonly consists of heavy curarization with minimal narcosis. Like many other anaesthetists I am an enthusiastic advocate of this technique, but as the following case will show one must be reasonably certain that the level of narcosis does not become too light.

The patient, an intelligent and co-operative woman aged 46, underwent laparotomy for suspected carcinoma of the head of the pancreas. Premedication was morphine 1/6 gr. (11 mg.), atropine 1/100 gr. (0.65 mg.), and she was slightly more drowsy than the average when she arrived in the anaesthetic room. Induction was with "tubarine" 15 mg. (after 5-mg. test dose) and 0.4 g. of thiopentone. A No. 9 cuffed tube was easily inserted and directly connected to a Coxeter-Mushin absorber. Nitrous oxide and oxygen 500 ml. of each per minute were run into the circuit and respiration was aided throughout the operation. Small doses of thiopentone and tubarine were given into the drip as required, to the total of 0.8 g. and 65 mg. respectively. Subtotal pancreatectomy was carried out by Professor Charles Wells. During the first three hours the gas mixture was unaltered, but during the fourth hour oxygen alone was given. During the last half-hour (before and during wound closure) 20% cyclopropane was administered. Prostigmin 5 mg. and atropine 1/50 gr. (1.3 mg.) were given during skin suture. The patient's post-operative course was good.

I saw the patient on the third day after operation and during conversation casually inquired about when she came round. Her reply was somewhat shattering—"As a matter of fact, doctor, I woke up in the theatre! I remember going to sleep after your injection in my arm, and some time later I was awakened by the most excruciating pain in my tummy. It felt as if my whole inside were

being pulled out: I wanted to cry out or otherwise indicate my suffering, but I couldn't move any part of me. I heard the doctors talking about the gall-bladder and about doing something with it to the small intestine. Then I went to sleep again and later on woke up back here in bed." She said she had never before experienced such short-lived agony, and it was obvious that the pain must have been severe, because she appeared to have very little post-operative pain or discomfort—i.e., she was a woman with a high pain threshold. It is reasonably clear from the foregoing that her period of consciousness coincided with the absence of nitrous oxide from the mixture, and that the cyclopropane was adequate to induce further sleep for the conclusion of the operation. At no time during operation was her respiratory excursion less than about 20% of normal, and during the fourth hour she twitched her eyebrows twice: on each occasion 1 ml. of 5% thiopentone was given. By itself it was obviously insufficient; if the nitrous oxide had been maintained she would still have twitched her eyebrows (as patients often do with this anaesthetic technique) but she would not actually have wakened nor would she have had any recollection afterwards. As she had diabetes the use of even a slight trace of ether was precluded, and long-continued cyclopropane, even in a low percentage, was not considered owing to the possible nuisance of increased oozing.

If faced with a similar type of case again, I should perhaps be a little more generous with the thiopentone, but the main requirement is to maintain the nitrous oxide unless the patient is obviously and grossly anaemic. I should be most interested to learn if any other anaesthetists have had the similar misfortune of having a curarized patient very definitely conscious at some stage of an operative procedure.—I am, etc.,

Liverpool.

E. H. WINTERBOTTOM.

Second Thoughts on Streptomycin

SIR,—Having just returned from Canada and the U.S.A., where I studied the chemotherapy of tuberculosis, I was extremely interested to see a note under "Medical News" headed "Second Thoughts on Streptomycin" in the *Journal* of December 3, 1949 (p. 1303). Perhaps readers would be interested to hear that the Eighth Streptomycin Conference held on November 10–13, 1949, in Atlanta, Georgia, was a most important meeting, at which the whole field of chemotherapy of tuberculous diseases was discussed, and the views of this conference are now being extensively quoted in the U.S.A. Your quotation, "... Results now reported appear to indicate that it (dihydrostreptomycin) has no important advantage over streptomycin, and that ... its use in preference to streptomycin should not be encouraged," seems to be the general consensus of opinion regarding these substances in both Canada and America, as it seems to be in this country also. It would appear to be more than worth while to record, however, that at the Mayo Clinic the views of the chest physicians are in direct contrast to the statement quoted, and it is dihydrostreptomycin which is by far the most widely used. The workers at the Mayo Clinic maintain that it produces so much less toxic effects that its use would seem to be indicated and encouraged.—I am, etc.,

London, S.E.12.

M. M. NAGLEY.

Streptomycin in Tuberculous Meningitis

SIR,—All writers seem agreed that intrathecal streptomycin is absolutely essential in the early stages of the treatment of tuberculous meningitis. The injection of a substance into the spinal canal is attended by a certain risk at any time and leaves a doubt in the mind if death should occur within a short time of administration. It is my experience with intrathecal streptomycin that deaths—when they occur—take place at an earlier stage than was the rule in untreated cases. These difficulties would be largely overcome if intrathecal injections could be avoided. The following case is not put forward as a reason for abandoning intrathecal streptomycin—not by any means. It is given as an example of the necessity for early diagnosis in this condition and for the early removal to hospital of suspected cases, for in certain cases, in the initial stages, it may yet be found safe to rely on intramuscular injections alone.

A schoolgirl aged 9 years was admitted to hospital on September 23, 1949, suffering from queried meningitis. She was then three days

ill, complaining of headache, lassitude, and loss of appetite. A sister had died some years previously from meningitis, probably tuberculous. On examination she was well nourished, but was somewhat confused. There was ptosis of the left eyelid; neck rigidity was present and Kernig's sign was positive. Temperature was 100.4° F. (38° C.), and the pulse rate was 90. Lumbar puncture revealed a clear fluid under pressure. Pending the bacteriologist's report this was regarded as a case of tuberculous meningitis. Treatment was commenced with intramuscular injections of dihydrostreptomycin—1 g. in 24 hours in divided doses at 8-hourly intervals. Gradually the patient became more acutely ill. On the third day in hospital she was vomiting continually and temperature was 103° F. (39.4° C.). She was now very confused. The bacteriologist's report on the C.S.F. was: fluid clear, cells 10, chlorides 700, globulin slight trace, sugar normal, acid-fast bacilli present. There was no change in her condition until the sixth day of treatment, when her temperature fell to 99.4° F. (37.4° C.). As a result of this improvement the decision to give intrathecal streptomycin was deferred until the following day, when, her condition being much better, it was abandoned. On this day temperature was normal, vomiting ceased, and she was less confused. Thereafter she rapidly regained normal health, and there has been no set-back up to the time of writing. Eight-hourly injections were continued for six weeks, for the remaining eight weeks 0.5 g. was given every 12 hours intramuscularly. The report on the C.S.F. on October 19 was: cells 2, globulin trace, sugar normal, chlorides 740, no gross pleocytosis, no tubercle or other organisms. A similar report has been more recently received. The patient was allowed out of bed on December 16. She was able to walk quite well in a few days, and had put on weight. Clinical examination revealed nothing abnormal in central nervous or other systems. Report on x-ray examination of lungs: enlarged left hilar shadow—both lung fields clear. There was no local or general reaction to injections at any time.

This case is considered remarkable, as there appears to be a complete recovery from a condition proved clinically and bacteriologically to be tuberculous meningitis, and especially as treatment was confined to intramuscular injections. There can be no doubt that intrathecal therapy is essential in established cases. Is there a possibility that it can be avoided in the very early stages?—I am, etc.,

Swinford, Co. Mayo, Ireland.

PHILIP F. CAWLEY.

Anticoagulant Therapy

SIR,—There are one or two points in the paper on anticoagulant therapy by Drs. H. W. Fullerton and G. Anastasopoulos (December 31, 1949, p. 1492) which concern my original article in the *Journal of Clinical Pathology* (1949, 2, 45) and to which I should like to draw your attention. They state that haemolysis shortens the accelerated clotting time with the venom method, probably because of lipid set free from the cell envelope. They state that added lipid (as in alimentary lipaemia) produces little or no shortening of the accelerated clotting time when the effects of dicoumarol are marked. They then say that the results obtained by me with the venom test in patients on dicoumarol should be discounted because I "was obviously producing varying degrees of haemolysis." As the differences which I reported between the venom and the brain-extract tests were in patients fully under the influence of dicoumarol and not in normal individuals there seems, Sir, to be some inconsistency here.

As to the haemolysis which Drs. Fullerton and Anastasopoulos suppose to have been present, Wintrobe's anticoagulant (which I employed) is widely used in blood work and does not cause haemolysis. Further, the word "shaken," used in my original paper, does not necessarily imply violent agitation, and the blood was actually mixed by inversion. Lastly, the results obtained by me—namely, less lengthening of the accelerated clotting time with the venom test than with the brain-extract test, and sluggish response of the venom test to the effects of dicoumarol—are actually confirmed by Drs. Fullerton and Anastasopoulos in this paper, as will be seen by reference to their conclusions and to Chart III respectively. Consequently it is with pleasure (having in mind the safety of patients on dicoumarol) that I note from this paper that Dr. Fullerton does not now use the venom test alone in the control of dicoumarol therapy.—I am, etc.,

London, W.9.

G. A. JAMES.