lying conflicts I have begun to see that I have been missing a great, possibly the greatest, therapeutic force we may ever find." These are very remarkable statements, and merit, I feel, a much wider publicity. Dr. Gordon says that he would unhesitatingly support Swaim's contention that this recovery and freedom from relapse depend in large measure on the patient's degree of emotional stability and the satisfactoriness of his philosophy of life. It would be extremely valuable to know to what extent Dr. Gordon and others are using Swaim's method, and whether any comparable results have been produced in this country.—I am, etc.,

Westminster Hospital, S.W.1, June 21. DONALD COURT.

Chemotherapy of Gonorrhoea

SIR,—The recent articles and letters published in the *Journal* have undoubtedly shown the value of M & B 693 in the treatment of gonorrhoea in the male. General practitioners as a rule treat very few of these cases nowadays, but my experience has shown that results from this comparatively simple treatment have been very successful.

A typical example which came under my care was treated with the drug in doses of two tablets four times a day for eight days, followed by two days' rest. The patient was then given a further course of tablets—one tablet four times a day for six days. No irrigations or other local treatment were administered. The discharge cleared up entirely within four days of commencing this therapy, and the patient was free of any symptom after six days. He was advised to drink plenty of bland fluids, wear a suspensory bandage, and avoid alcohol, etc. When the discharge had ceased the second course of tablets was followed by a series of intradermal injections of gonococcal vaccine.

Whatever treatment is adopted, two things should be stressed—namely, rest and local hygiene. As regards the former, this is not always an easy matter for the patient, especially if he is of the artisan class, but my advice to such patients is to go to bed as soon as circumstances permit after their return from work, thus enabling them to obtain ten or twelve hours' rest in bed each day during the acute stage, with perhaps a still longer period at the week-end. I think it is of paramount importance to explain carefully to the patient the need for protecting the rest of the body from the discharge. One often sees a patient attending with his under-garments stained with obvious discharge, even after he had been warned of the dangers of carrying infection to other parts of the body. I suggest that the patient be provided with cotton bags to which tapes are attached. These could be tied round the base of the penis and the bag removed from time to time.—I am, etc.,

London, W.6, June 17. J. STEIN.

Chemotherapy of Pneumonia

SIR,—Some of the writers of articles on M & B 693 have emphasized the difficulties and dangers associated with the use of this drug, and it was reasonable to suppose that for its successful exhibition there might be required constant supervision by a trained hospital staff, with immediate access to laboratory facilities. Conditions in Public Assistance practice, although considerably better than in Poor Law days, entirely rule out the domiciliary use of serum treatment in pneumonia, but I undertook, along with my colleagues, to test the value of M & B 693 in the area in which we work. All our cases were treated in single-apartment or badly overcrowded room-and-kitchen houses. Although from the scientific point of view it is doubtless desirable to type the pneumococcus in all cases, it is well-nigh impossible to do so in a practice such as ours. Treatment was begun, therefore, immediately the diagnosis was made.

The number of cases treated during the period was twenty-two; the youngest was 8 months, the oldest 81 years. Of these, three were cases of acute lobar pneumonia, two were cases of whooping-cough complicated by pneumonia, and seventeen were cases of bronchopneumonia. All showed a remarkably rapid response to the treatment and the results were uniformly good. In lobar pneumonia the temperature fell in twenty-four hours, while the pulse came down to a rate of 80 within three days. The patients appeared to be well, but signs of consolidation persisted till the usual time for crisis. In all the cases of bronchopneumonia the temperature fell to normal in about twenty-four hours, while the pulse also came to normal in the same period, but the respirations took about fifty-six hours to return to normal.

General treatment was along the usual lines adopted for pneumonia, but all foods and drugs with a high sulphur content were avoided. In one case where the temperature suddenly rose again it was found that instructions had been disregarded and the patient had been given an egg-flip. Phenolphthalein emulsion seemed to be the best laxative. Cyanosis was not common. Nausea was present in a number of cases, but it did not seriously interfere with treatment.

In Glasgow last winter pneumonia and bronchopneumonia tended to be of a rather mild sort, and in any event the number of cases in our series is too small to discuss mortality rates. Our experience, however, seems to suggest that the chemotherapy of pneumonia with M & B 693 need not be restricted to hospital cases and may be regarded as a justification for the undertaking, in the same type of practice as ours, of a trial on a much more extensive scale.—I am, etc.,

Glasgow, June 16. EDWARD N. THOMSON, M.B.

Xenopus Test for Pregnancy

SIR,—In your issue of June 17 (p. 1258) Professor J. W. C. Gunn of the Pharmacology Department at Capetown takes exception to the fact that in a recent publication Professor Crew associates the xenopus pregnancy test with my name. No one sympathizes more strongly than I do with Professor Gunn's desire to see that the University of Capetown should get all the credit due to it. Nevertheless the significant omissions which completely distort the truth about the history of the xenopus test force me to supply relevant information.

1. After I discovered the suitability of xenopus as a test animal for the anterior lobe gonadotrophic secretion, my laboratory undertook an extensive investigation on its relation to metabolic processes. The results (Hogben, Charles, and Slome, 1931) were published as from Capetown after my return to London. While this work was in progress Dr. Zwarenstein expressed a desire to extend our inquiries into the relation of the pituitary to metabolism, and learned my technique of hypophysectomy from me in my laboratory and *not* in the physiological department. Meanwhile tests on pregnancy urine were carried out in my laboratory at Capetown, and when I left it I entrusted, with Dr. Zwarenstein's full knowledge, further inquiry on the practical application of my work to pregnancy diagnosis to two workers then associated with me—namely, Dr. Ariel Goldberg and Dr. David Slome.

2. After returning to London I imported large stocks of xenopus for a renewed attack on the whole problem, and invited Dr. Bellerby to join me in the work. Bellerby started by investigating the reliability of the test for pituitary extracts while collecting material to explore the

pregnancy test throughout the early stages. Meanwhile I had heard that Dr. Goldberg and Dr. Slome were not in a position to carry on with this part of the programme.

3. At that time Dr. Zwarenstein and one of his pupils, Mr. Shapiro, sent to me for publication in this country the first of a series of communications in which they claimed that xenopus was unsuitable as a test animal for gonadotrophic substances because it loses reproductive activity in the laboratory. Although I informed them that our own results were in total disagreement with this finding and that their conclusions were based on defective animal husbandry, I agreed to forward the first of this series for publication; and since no serious claim for the xenopus test could be sustained until the so-called "captivity effect" which Zwarenstein and Shapiro described had been disposed of, Dr. Bellerby began a series of investigations, supplemented later by the work of Mr. Landgrebe, also in my department, with a view to prescribing the precise conditions of diet, density, pollution, temperature, and illumination to maintain xenopus at maximal reproductive activity in the laboratory as a test animal.

4. While these studies were in progress Dr. Zwarenstein came to work for a year in my laboratory and learned during that period how the captivity effect can be avoided by proper management of the animal. He was therefore fully aware of the tests which Bellerby had long ago carried out, as he had been fully aware that Dr. Slome and others of my associates had made preliminary studies on the matter during the period when he had been previously associated with me.

5. Dr. Bellerby's experiments which appeared from my laboratory after Zwarenstein's somewhat precipitate announcement (in the *Trans roy. Soc. S. Afr.*) of his own results on *freshly caught* toads would have been completed at least a year earlier if Zwarenstein had not chosen to endow an exploit in defective animal husbandry with the status of a necessary physiological process. Since Dr. Zwarenstein had not, and never has since, taken the opportunity of withdrawing what he previously wrote about the captivity effect, the claim that he discovered the xenopus test for pregnancy would not be justified by reference to the published record of the work up to that point. Even if it were not true (*a*) that Dr. Zwarenstein became aware that the problem was being worked out in my laboratory while he was associated with its work, and (*b*) that he was kept in touch with what we were doing by correspondence which passed between us and culminated in his own desire to continue his work in my laboratory in London, the fact remains that everything which Dr. Zwarenstein had written about the captivity effect indicated, if it were true, that xenopus could not be used as a test animal.

I have never taken advantage of the mistake which held up the final introduction of the xenopus test for routine work to criticize in detail Dr. Zwarenstein's technique. On the contrary, I welcomed it as a stimulus to inquiries which have made it possible to prescribe more precisely the necessary conditions for using the test, and gave him every encouragement to pursue his own valuable and parallel inquiries, if only because I naturally regard them as an integral part of the programme initiated in my laboratory in Capetown. Since my relations with Dr. Zwarenstein have been friendly throughout, and since I have the highest opinion of Zwarenstein as a research worker, I regret that the intervention of Professor Gunn has forced me to state these facts, which might wrongly suggest that I regard Dr. Zwarenstein's South African note of 1933 as an unprofessional attempt to jump a claim. He did not claim priority in the joint note (*Nature*, 1934, **133**, 762) which appeared after Bellerby's note from my laboratory (p. 494) under his own name and that of his pupil. He specifically refers to the collaboration of Dr. Goldberg and to correspondence which passed between him and my own laboratory.

With regard to the fact that Professor Crew has chosen to use my name in this connexion, I may further add that I have not discussed with him the personal details which Professor Gunn's letter forces me to record. What Professor Crew did know, and Professor Gunn does not know, is that on hearing of the pioneering work on pregnancy diagnosis in Professor Crew's institute I wrote to him immediately after my return to London in 1930, telling him that my own experiments might be interrupted by my new appointment, and sending him a few specimens of xenopus to test. To my certain knowledge preliminary experiments on xenopus were actually carried out in Edinburgh before Dr. Zwarenstein began any work of the kind; and extensive research in Professor Crew's department was restarted only when the very thorough inquiries of Dr. Bellerby had finally disposed of Dr. Zwarenstein's so-called captivity effect.—I am, etc.,

University of Aberdeen, June 20. LANCELOT HOGBEN.

Spinal Anaesthesia

SIR,—Mr. J. Hughes (June 17, p. 1224) has conferred a boon by his masterly report of observations on nearly 1,000 light percaine spinal anaesthetics.

Perusal prompts a few remarks on what is still an "emerging" procedure. First, is it fair to claim for the Etherington Wilson time-diffusion technique that it involves the minimum manipulation? Surely the simplest in this respect is the following method. Puncture the patient in the lateral position, side of incision being uppermost, and with the bridge raised under the waist until the buttock almost lifts from the table. Enter at the L. 2–3 interspace. After two minutes with the spine tilted at 5 degrees head up, the table is lowered to 10 degrees Trendelenburg. The patient is never moved if it is a kidney operation and only once for a laparotomy, when, after the bridge is lowered to within two inches of the table, he is gently rolled over on his back. Simplicity is important, for it aids safety. This gives a perfect abdominal paraplegia, and never is more than 12 c.cm. needed. Secondly, regarding dosage, 10 c.cm. is often adequate for the abdomen and in poor risks; after this amount is injected I leave the needle *in situ*. If in two minutes the level of anaesthesia is high enough the needle is withdrawn. Otherwise 2 c.cm. more is administered. Thirdly, the spinal route is beneficial even in infancy, and I always use it for emergencies like acute appendicitis if there is any concomitant "cold" or pulmonary affection. Most little ones, down to four or five years, respond surprisingly well to gentle persuasion and prefer a prick to the more memorable suffocative sensations of inhalation. Fourthly, his occasional complete failure is an old spinal bogey which I prefer to explain by imperfection in thecal puncture. When this occurred recently I gave a second 12 c.cm. ten minutes later and normal paraplegia followed. That varying degrees of susceptibility occur is in accord with response to drugs in general, but as we "never" meet with such complete percaine resistance in local anaesthesia why should 0.5 per cent. fail intraspinally? The only obvious difference between spinal nerve roots and peripheral nerves is difference of accessibility to the injection. —I am, etc.,

Bristol, June 25. A. WILFRID ADAMS.