Correspondence

Treatment of Traumatic Wounds and Open Fractures

SIR,—That the treatment of traumatic wounds and open fractures is arousing great interest at the present time is evidenced by perusal of the current numbers of the *Lancet* and the *British Medical Journal*. The special articles on war and air raid casualties which have been appearing in your pages have been most instructive and greatly appreciated. A series of lectures upon this subject delivered to the medical profession of this district in July, under the auspices of the British Medical Association, was attended by packed audiences of practitioners and surgeons. My object in writing to you is to submit that the best Continental practice, as influenced by the paramount authority of Böhler, has revealed to many that the system and methods advocated must furnish an entirely new conception of the subject of wounds and fractures. Evidence is not lacking that in not a few quarters in this country advantage is being taken of this new knowledge to improve the treatment of patients. If the report of the Interdepartmental Committee on the Rehabilitation of Persons injured by Accidents receives general acceptance in its reaffirmation of the need for the organization of such treatment, I consider that the standards of treatment and end-results will be markedly improved in Britain.

In the treatment of cases of recent traumatic wounds and of compound fractures many links in the chain of successful therapy have to be forged. None of them can be neglected. They include considerations as to first aid, shock, haemorrhage, the propriety of closure after excision of the wound, methods of preparing the limb for operation, the use or avoidance of tourniquets and antiseptics, prophylactic sera and chemotherapy, the best dressing, bandage, and retentive appliances. Furthermore, there is the great emphasis now laid upon the thorough immobilization of the limb by a method which maintains a good circulation, usually a non-padded plaster. There is the recommendation that the limb must be elevated to prevent chronic venous congestion and oedema; and the great principle also of non-interference with the wound, provided the pulse and temperature charts give no indication otherwise. A visitor to Böhler's clinic who stays to study is favourably impressed by many things, including the standardization of routine methods, and by the very important fact to the British observer that even minor and comparatively trivial cases of traumatic wounding are admitted and not treated as out-patients. This is particularly the case in injuries of the hand. Another striking detail is that the skin suturing is done very accurately and completely. Subcutaneous catgut is avoided and the skin stitches are not removed for fifteen or twenty days. Is it not too often the case that traumatic wounds do all right until the usual sixth or seventh day, when the stitches being removed and the case allowed too much liberty, things begin to go wrong? The articles by A. Tudor Hart (*British Medical Journal*, May 27 and June 3, 1939) and by Josep Trueta, Barcelona (Lancet, June 24, 1939), well repay study by all who may be called upon to deal with civilian or war casualties.

It is true that there are many ways of reaching the goal in surgery, and individuals or groups may feel entitled to think that they have good reason to be content with the results obtained by alternative methods in cases of traumatic wounds and open fractures. There are bound to be divergences of view regarding the propriety of the early closure of traumatic wounds and as to the rival claims of aseptic and antiseptic methods. We will all admit, however, that most of our knowledge is based upon authority. The best Continental practice, supported as it is by the experiences in Spain, must inevitably carry great weight and merit the closest attention. I am confident that he who makes a study of these methods and employs them with skill, care, and good judgment will add greatly to his ability to deal with traumatic surgery. The knowledge, experiment, and experience of many masters have gone to build up this system. I refer to Paré, Lister, Halstead, Sir Robert Jones, the British, American, and French surgeons in the last war, and to Böhler. It was the acute sense of the delicacy of the tissues and their great powers of natural recovery that marked these men out from their contemporaries. To improve our standards requires activity and reform in two directions. The first is administrative—to create special units in our hospitals. The second is technical—to apply improved methods in the treatment of the cases by a system of scrupulous and patient attention to a mass of details.—I am, etc.,

Edinburgh, Aug. 21. W. A. COCHRANE.

Spinal Anaesthesia

SIR,—During the last few months many articles and letters have been written to the *Journal* on spinal anaesthesia in general and the technique of administration in particular. It is a matter of considerable surprise to me that the writers have rarely mentioned the claims of "heavy" percaine as a safe and, what is most important, an easily administered spinal anaesthetic. ("Heavy" percaine (Ciba) is a hyperbaric solution 1:200, with a specific gravity of 1024 and the addition of 6 per cent. glucose. It is put up in sterile ampoules of 3 c.cm.)

I suggest that the ideal spinal anaesthetic should be:

1. Easily administered so that the premedicated patient is disturbed as little as possible.
2. Quick in action so that the start of the operation is not unduly delayed.
3. Of prolonged action when required so that long operations can be performed without hurry.
4. Free from a marked tendency to depress the blood pressure.
5. Infrequently followed by headache and other post-anaesthetic complications.

I submit that "heavy" percaine fulfills these criteria to a greater extent than any other agent in common use. Referring to the five points laid down as ideal requirements:

1. When using this preparation no elaborate movement of the patient is either necessary or desirable. He or she is merely placed on the table in the left lateral position with knees and head approximating as nearly as possible. An injection of from a minimum dose of 1 c.cm. to a maximum dose of 3 c.cm. is made in the usual way into the spinal theca. The patient is then turned on to the back and the table lowered towards the head according to the height of anaesthesia required. Throughout, the head is supported on a single firm and well-filled pillow.
2. Full anaesthesia to the desired level is achieved in from 3 to 10 minutes, the average time being 5 minutes. After 15 minutes the patient can be moved into a full Trendelenburg position with complete safety.
3. With the possible exception of "light" percaine, "heavy" percaine has the longest action of any spinal anaesthetic. I have frequently had perfect anaesthesia of two and a quarter hours, while once, with 3 c.cm., an operation lasting two and three-quarter hours was performed with comfort both to the patient and to the surgeon.

4. Fall in blood pressure in high level anaesthesia averages about 20 mm. Hg, but to a certain extent is proportional to the height of the pre-anaesthetic systolic pressure. It may be said in general that the higher the original systolic pressure the greater the subsequent drop.

5. Post-anaesthetic headache is rarely encountered so long as the foot of the bed is well raised for eight hours after operation and the patient is not allowed to get up too soon. For instance, it is unwise after the most minor operation or the smallest dose of spinal anaesthetic (of whatever type) to allow the patient to go home the next day.

In conclusion, I would draw attention again to the simplicity of technique in administering "heavy" percaine. There seems to me something rather comic in a technique which requires a semi-conscious patient to sit on the operating table and lean like a dummy over the shoulders of a staggering nurse while the anaesthetist holds a stop watch, waiting to give the signal for him to be thrown on to his abdomen, only a second or two later to be turned over on his back, so that before the operation has started any premedication has been rendered all but useless. Perhaps we are on the eve of great events. If this be so, then any methods of anaesthesia which are simple to carry out and quick in action merit attention. The data and opinions expressed are the results of experience gained in more than 400 administrations of "heavy" percaine by my partner, Mr. G. E. P. Markley, and myself during the last three years.—I am, etc.,

Bournemouth, Aug. 22. C. E. GAUTIER-SMITH.

Xenopus Test for Pregnancy

SIR,—In your issue of July 1 (p. 38) Professor L. Hogben replies to my letter of June 17 (p. 1258), in which I drew the attention of Professor F. A. E. Crew to the claims of Drs. H. A. Shapiro and H. J. Zwarenstein as the originators of the xenopus test for pregnancy. What may have seemed uncalled-for intervention on my part was due to the fact that these authors published their paper from the department of physiology of the University of Capetown. As I am at present the acting head of this department I thought it better that I should put forward their claims than the authors themselves. I am at this disadvantage, however, that apart from the published work my information is necessarily second-hand, depending on the statements of others. It would be extremely painful for me to have to discriminate between the word of Professor Hogben and Drs. Shapiro and Zwarenstein, three men for whom I have the highest regard and whose friendship I value. Since I am the one responsible for opening this discussion I should be glad to be given the opportunity of stating the position as it appears to me, and to leave any further debate to those more intimately concerned.

(1) Professor Hogben's note to the Royal Society of South Africa in March, 1930, contained the extremely important observation that ovulation was induced in the female *Xenopus laevis* by injection of the active extracts of the anterior pituitary of the ox. From this the inference might be drawn that the anterior-pituitary-like substance in pregnancy urine would probably have the same action. If this was verified, and xenopus was found sufficiently sensitive, the reaction might be used as a practical test for pregnancy.

(2) Professor Hogben refers to pregnancy tests which, on leaving Capetown, he had entrusted to Drs. Ariel Goldberg and David Slome. I am informed by Dr. Goldberg and by Dr. Zwarenstein, who actively assisted in these experiments, that numerous tests were carried out during January and February, 1931, in the physiology department. The observations were made, however, not on ovulation in xenopus but on colour changes in the skin. They were discontinued as they were found of no value as a test for pregnancy.

(3) Drs. Zwarenstein and Shapiro at a later date published the statement that xenopus loses reproductive activity when kept in the laboratory for some time. This observation was correct for the conditions under which they were keeping these animals. If true for all conditions, xenopus would be useless as a test for pregnancy except in South Africa, where an abundant supply of fresh frogs is always available. Dr. Bellerby, in Professor Hogben's laboratory, showed that xenopus, if kept under other conditions which he determined, did not lose reproductive activity in captivity. Drs. Zwarenstein and Shapiro's observation at least served the purpose of drawing attention to the necessity of special care in keeping xenopus in captivity.

(4) Later, experiments with pregnancy urine on the ovulation of xenopus were undertaken by Dr. Bellerby in London and by Drs. Shapiro and Zwarenstein in Capetown. Drs. Shapiro and Zwarenstein had previously worked in Professor Hogben's laboratory, but they affirm that this work was quite independent. There is no doubt that their publication of the first practical test appeared about five months before Dr. Bellerby's. Owing to priority of publication, the usual course would be to call this the "Shapiro-Zwarenstein test."

These authors, regarding this as a modification of the Aschheim-Zondek test, have refrained from attaching their names to it. Following their lead, might I suggest that a little credit might be given to the humble batrachian, which seems to give an invariably correct diagnosis, by calling this the "xenopus test."—I am, etc.,

Capetown, Aug. 7. J. W. C. GUNN.

Treatment of Nocturnal Incontinence

SIR,—Nocturnal incontinence is a very troublesome complaint in boys living at home, but in the public school boy it may be positively disastrous. Other boys sleeping in the dormitory are sure to find out about the weakness, and the unfortunate sufferer will be regarded with a mixture of derision, disgust, and scorn that are likely to be shattering to his morals and may induce a permanent defect of character. Many cases cure themselves before long; others are greatly helped by various well-known methods of treatment, such as giving no fluids late in the day, waking at midnight, altering the diet and sleep habits, scolding or reassuring, or giving belladonna in full doses, or thyroid extract. Most practitioners have some private recipe of their own which sometimes succeeds. Or the boy may be circumcised, or sounded for stone, or handed over to a psychotherapist. It is not until these measures have been tried that the surgeon is likely to be called in. By this time the situation is beginning to be serious; the boy soaks his bed nearly every night, and the habit seems to be ingrained. It is for cases like this that the treatments mentioned in this letter are recommended. No claim is made for originality, but they do not appear to be widely known, and about a dozen textbooks or special articles consulted do not mention them.

It is essential as a first step to remove the boy from the school dormitory to the sanatorium or to a nursing home to set his mind at rest and to observe the case. If there is day frequency or if the intelligence is of a low order the case is going to be difficult. Examination must be made to exclude irritation from stone in the bladder, from phimosis, or from worms. If there is no day frequency, no source of irritation, and the intelligence is fair, the most promising line of treatment is to inject silver nitrate into the deep urethra to reduce the sensitiveness of the nerve endings. If the patient is very frightened a general anaesthetic can be given, but normally the urethra can be rendered quite insensitive by the prior injection of decicaine.

The treatment is undertaken when there is urine in the bladder. Two c.cm. of 0.2 per cent. decicaine is injected, though a stronger solution will do no harm. It is milked back along