Retrodisplaced Gravid Uterus

SIR,—Through your columns I should be grateful for the opportunity of making some observations on the letters referring to my paper (Feb. 1, p. 169) written by my two friends, Dr. M. Hamilton (March 15, p. 350) and Dr. J. A. Chalmers (March 22, p. 392).

Polak's figures quoted by me and those obtained at U.C.H. show that sterility and retrodisplacement of the uterus are associated. This point was made in my opening paragraph and has been kindly enlarged upon by Dr. Hamilton in his letter. What is not certain, however, is that the displacement in itself is the cause of the sterility. It is possible that there are other factors associated with the retrodisplacement which are responsible for the sterility and which are at present imperfectly understood. It is because of this that I have suggested that correction of the displacement in such cases should be left as the last method of treatment after all other treatments have failed.

I stated "that the incidence of abortion in patients with retrodisplaced gravid uteri is at least no higher than that for all pregnancies." The importance of the displacement as a cause of abortion has been overemphasized and is only likely to be a factor when it causes incarceration. These deductions were made on the figures obtained at U.C.H. and on those of Danforth and Galloway quoted by me. Dr. Hamilton, in his more logical manner, prefers to state that my figures "do not disprove the hypothesis that retrodisplacement (without incarceration) does not significantly predispose towards abortion." If the incidence of abortion (without incarceration) is not influenced by the position of the uterus then correction of the displacement is necessary only to avoid incarceration. I believe this applies to the primigravida and to the multipara, who may have had a previous abortion from a displaced uterus. Dr. Chalmers disagrees with me on this last point and attempts to show that two of the three cases quoted by me prove his point.

From the history of patient No. 39, Dr. Chalmers makes the deduction that the ventrosuspension operation was unsuccessful and that the first two unsuccessful pregnancies occurred in a retrodisplaced uterus. Is this not putting the cart before the horse? The ventrosuspension operation was carried out at a reputable London teaching hospital (not U.C.H.), and I am sure they would be as indignant at Dr. Chalmers's "reasonable" deduction as he would be in similar circumstances. In any event, supposing the operation was unsuccessful, does not the case show that conservative treatment is possible and that continuity of the abdominal wall can be maintained? I would prefer to regard this patient as an habitual aborter, with the uterus accommodating the pregnancy for a longer period on each successive occasion.

In case 42 abortion occurred 11 weeks after anteversion had been attained (not 9 weeks as stated by Dr. Chalmers). If circulatory factors dependent on the position of the uterus were the cause of the abortion as suggested, would the latent period be so long and would not abortion be much commoner than it is with incarceration when such factors are more likely to be active? Dr. Chalmers has misread me when he says that I point out that circulatory abnormalities may interfere with the imbedding of the ovum in a retrodisplaced uterus. I only quoted Barnett's observation on the subject. Indeed, Barnett says that abortion due to such factors usually occurs at 2½ months, not 23 weeks. Personally I prefer to regard and treat such patients as cases of habitual abortion, and I admit that I do not know the cause of the abortion in the majority of such cases.—I am, etc.,

London, W.1.

H. H. FOURACRE BARNS.

Marrow Biopsy

SIR,—Prof. D. F. Cappell, Dr. H. E. Hutchison, and Dr. G. Harvey Smith (March 29, p. 403) call attention to the value of examination of specimens obtained from marrow by paraffin sections in addition to the usual smears. But their advocacy of a rather complicated and inevitably messy method of fixing the "mush" from the sternal puncture needle is surely out of date.

The Turkel trephine is little larger than the Salah needle, and it permits of the obtaining of a "sausage" of marrow as well as of the usual juice for making films. Essentially, it consists of an outer needle (with stylet), and a narrower inner one with a serrated end. The outer needle (with stylet in place) is pushed in until its tip sticks in the anterior lamella of the sternum. The stylet is then withdrawn and replaced by the inner (trephine-ended) needle (also with stylet in place). When the end of the inner needle is in contact with the bone, the stylet is withdrawn, and the inner needle is turned to and fro while slight pressure is exerted, until a sudden cessation of resistance indicates that the medullary cavity has been entered; then a few more half-turns will fill the end of the inner needle with marrow. Next, leaving the inner (trephine) needle in position, the outer needle is pushed onwards in the marrow cavity. The inner needle is withdrawn and its contents expelled into fixative. A syringe is attached to the outer needle and marrow juice obtained by suction.

In this way marrow tissue is obtained while films are not diluted by aspiration of as much marrow as Cappell, Hutchison, and Smith find necessary. In fact there is "at last the answer to the haematologist's prayer for a means of combining the advantages of sternal trephining with those of sternal puncture" (annotation, p. 419).—I am, etc.,

London, W.1.

A. PINEY.

Intrathecal Penicillin in Cerebrospinal Fever

SIR,—In his article on "Medical Aspects of Penicillin Treatment" (March 8, p. 290) Dr. B. A. Young expresses a doubt as to the necessity for intrathecal penicillin in meningococcal meningitis and also a desire to know the practice of fever hospitals.

Speaking for the Park Hospital, I can say that the results obtained for many years with sulphonamides alone, especially sulphathiazole and sulphadiazine, have been so good that we consider intrathecal penicillin as an unnecessary complication in the routine treatment of these cases. Those who have had experience in the treatment of cerebrospinal fever in the past with meningococcal antitoxin have not forgotten the condition of patients receiving daily intrathecal injections of antitoxic serum, neither have they forgotten that often better results were obtained with intravenous administration alone. When they see the ease with which sulphonamide-treated patients now recover, they cannot help but view all return to unessential intrathecal medication as a retrograde step.

Moreover it is not sufficiently realized that the majority of the deaths from cerebrospinal fever at the present time occur in fulminant septicaemic cases, with or without adrenal haemorrhage and often with little or no evidence of suppurative meningitis. Intrathecal penicillin cannot help these patients. Intensive sulphathiazole treatment, combined with systemic penicillin for 24 hours, can save them. It is indeed a curious fact that since penicillin has become generally available the case mortality for cerebrospinal fever should have gone up. One can read in the introduction to the *Medical Annual* for 1946: "The limitations of penicillin are being recognized, but it is surprising to find that sulphonamides may be superior to penicillin in the treatment of meningococcal meningitis."

It has become the practice apparently in many hospitals to administer penicillin intrathecally at the first lumbar puncture to all patients suffering from a purulent meningitis, whether primary or secondary, and before a bacteriological examination. As at least 80% of all cases of primary suppurative meningitis are due to the meningococcus, this means that a large number of cases of cerebrospinal fever are receiving unnecessary intrathecal penicillin. This would not matter much if the injection was without danger. But even the purest penicillin, given with all precautions intrathecally, is often followed in infants and children by various reactions—twitchings, convulsions, apnoea, vomiting, and collapse. In very ill cases with concomitant septicaemia it is easy to understand that the reaction may turn the scales against the patient.

During the years 1944, 1945, and 1946 136 cases of cerebrospinal fever have been treated at the Park Hospital, and the case mortality for all ages, all degrees of severity, late and moribund cases included, has been 8.8%.

The value of intrathecal penicillin in suppurative meningitis, secondary to otitis media, infected sinuses, cranial injuries, etc.,

and also in primary suppurative meningitis due to the pneumococcus and many due to *H. influenzae* (Pfeiffer) is not questioned, but in meningococcal cases its superiority to oral and systemic sulphonamides has not been established, and its use is attended with unjustifiable risks.—I am, etc.,

London, S.E.13.

L. J. M. LAURENT.

Epidemic Kerato-conjunctivitis

SIR,—The annotation on epidemic kerato-conjunctivitis (Jan. 25, p. 145) prompts me to report the common occurrence of K.P. in an outbreak among British personnel in Calcutta in the rainy season of 1945.

The usual history was of a sore eye 10 days after trauma occasioned by Rugby, soccer, or motor-cycling without goggles. Swelling of the upper lid and of the pre-auricular gland was present. Staining of the cornea with fluorescein developed a few days later, followed by K.P. in 20 out of 30 cases, sometimes with folds of Descemet's membrane and sometimes after the ciliary flush had subsided. The corneal picture bore a marked resemblance to the eye lesions of onchocerciasis, which, of course, was not involved. The K.P. was as often associated with the type of fine corneal lesion as with the disciform type, and was quite a feature in this epidemic.—I am, etc.,

Johannesburg.

J. GRAHAM SCOTT.

Endocrine Receptors in Relation to Cancer

SIR,—There can be little doubt that a living cell is ready and willing to receive an endocrine stimulus just as in plants and animals the female is ready and willing to receive the male element. But I fail to see why Mr. T. C. Clare (March 22, p. 390) labels a cancer cell as degenerate and therefore lacking in an endocrine receptor. A cancer cell is not degenerate by a very long way—it is, unfortunately, young, active, and vigorous.

For many years there has been an idea that cancer arises from some fault in the endocrines, the cell receptors, or both, but prolonged observation and clinical experiment have failed to substantiate this theory, promising though it appeared to be.

We know that cellular division is the result of nuclear activity—the greater the nuclear activity, the more rapid the cell division. And here we must hark back to the very beginnings of the organism. An organism was originally a small bit of our planet cast off when the latter began to cool. We are of the earth, earthy. In the course of long years of evolution it adapted itself to surrounding influences, and ultimately man appeared. But just as in our parent, the earth, there are some atoms, such as uranium, which are not too stable, so man may have some cells of a like character—liable to burst into exaggerated nuclear activity at slight provocation. Lucky are the people who have not inherited these unstable cells!

I submit, then, that cancer is brought about by irritation whipping up the electrical phenomena known as nuclear activity in cells which happen to be unstable. If this be so, what then? The Ministry of Agriculture has forbidden the propagation of plants which are liable to disease—for instance, some varieties of potato. Breeders of animals and birds choose healthy parents. As this naturally is impossible in the human race, the predisposition to cancer will continue to be handed down.—I am, etc.,

Wigan.

J. THOMSON SHIRLAW.

Amoebic Hepatitis

SIR,—During the last few months there have been admitted to my wards four cases of hitherto unsuspected amoebic hepatitis with abscess, two of them fatal. One gave a history of treatment with emetine in 1930 in India, and two others admitted to fleeting attacks of diarrhoea in the Far East, while the fourth assured me that he was the only soldier in the Middle East who had never had an attack of diarrhoea. A fifth case sent in for diagnosis for axillary glands was found to have a tender liver, and the sigmoidoscope revealed advanced amoebic ulceration of the bowel. This man had been in the Navy and had had attacks of diarrhoea on and off, but never enough to report sick.

As such a large part of the British Army served in "amoebic dysentery countries" in the late war, and as the original infection by the *Entamoeba histolytica* can be such a mild disease, it seems to me that such cases may become common during

the next few years, and practitioners should keep a careful watch for them. Admittedly the signs and symptoms of early amoebiasis of the liver are indefinite and are not confirmable by laboratory tests. In a doubtful case, however, the improvement with emetine is dramatic if one is on the right track. Few diseases are more treatable in their early stages, but if not diagnosed in time disaster may easily follow.—I am, etc.,

Bristol.

RICHARD C. CLARKE.

Arsenical Encephalopathy

SIR,—The case report by Drs. G. Hipps and R. Goldberg (March 8, p. 296) on a patient dying with arsenical encephalopathy calls for some comment. Their treatment was that advocated by Ransome *et al.* (1945)—i.e., the patient is sat up in a vertical position, receives calcium gluconate, calcium thiosulphate, vitamin C, intravenous glucose-saline, and is repeatedly lumbar-punctured. Stress is laid on the postural part of therapy in order to decompress the brain.

Whereas it is obviously difficult to be dogmatic in a condition which is rare and where spontaneous recoveries occur (mortality is estimated at 75%), the promise of British Anti-Lewisite (dubbed BAL by the Americans) appears to be considerable in reducing the mortality in arsenical encephalopathy. In a recent survey by the American Council on Pharmacy (1946) 44 out of 55 patients suffering from arsenical encephalopathy recovered after receiving BAL; of the 11 cases who died 5 were started on this treatment some 9–72 hours after coma had set in. Thus with early administration of BAL one may hope to reduce the mortality to less than 15%. The American Council recommends 3 mg./kg. four-hourly intramuscularly for two days, then four injections on the third day and two injections on the following days until the patient has completely recovered. They emphasize that BAL must be given early and in adequate concentration to be of use.

My own experience is confined to two patients, both of whom recovered with BAL as the principal therapeutic agent, although both were comatose when this treatment was begun. Incidentally the first patient was nursed flat on his back, the second sitting up. In view of the published results of the American Council it is considered that BAL should be the primary agent in treating patients with arsenical encephalopathy.—I am, etc.,

G. W. CSOKA.

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Electric Convulsion Therapy

SIR,—During the past nine months a new technique for the administration of oxygen and CO₂ throughout the convulsion has been used in the out-patient E.C.T. clinic at St. George's Hospital in an attempt to reduce the excessive cyanosis produced by the convulsion, with the resulting stress upon the cardiovascular system..

It has been our practice over many years to administer oxygen through a Hewett N₂O and O₂ mask at the conclusion of the convulsion, and in this way to cut short the period of cyanosis as much as possible. However, it was felt in the case of old patients (a number being over 70 years of age), and in plethoric and hyperpictic subjects and in those suffering a degree of cardiac insufficiency, that the less strain that could be placed upon the cardiovascular system the better. With this object in view, it was decided to administer oxygen throughout the convulsion. This at first sight may appear illogical in view of the supposed laryngeal spasm occurring at that time; in actual fact, however, this spasm seems to take on the characteristics of the general clonic contractions and in reality is only intermittent—this can be observed by watching the bag of the apparatus when oxygen is administered throughout the convulsion; it will be noticed that very shallow and rapid respirations are taking place in time with the convulsions.

That gaseous exchange is also taking place is demonstrated by the fact that at the end of the clonic phase, and before normal deep respirations are resumed, the patient will be seen to be a good colour with little or no evidence of cyanosis—this being a marked contrast to those cases which do not receive oxygen during the clonic stage.