these matters, on the precise limits of gynaecological practice which they would regard as ethical and therefore justifiable?—I am, etc.,

Aberdeen.

D. B. WATSON.

SIR,—Your leading article on abortion law reform (17 April, p. 1009) refers to two aspects of this problem.

The first concerns the modernization of this law (passed in 1861) to conform with current medical, legal, and social thought.

Although you state that the medical profession need be in no doubt about the legality of most of the accepted reasons for termination of pregnancy, it has been shown over and over again that in fact most medical men, lawyers, and the police are still uncertain of the exact interpretation of the present law. Actually the legality of termination to prevent damage to mental health depends on the precedent of one case (*Rex v. Bourne*) which has not yet been made statutory law. In consequence many doctors still fear to advise termination where it would be justified, and many women who would be entitled to legal medical terminations now seek backstreet abortions because they do not realize that even the existing law would enable them to have legal medical operations.

It is therefore very necessary to clarify the law by amending it on the lines advocated by the Abortion Law Reform Association, which aims to make termination legal when the case attracts relief under one of the following three main clauses: (1) when termination is necessary for preserving the physical or mental health of the mother; (2) when there is serious risk of a defective child being born; (3) when the pregnancy results from a sexual offence (such as rape, incest, or intercourse with a girl under 16).

The A.L.R.A. believes that it is true to state that a majority of the people of Great Britain is in favour of the above amendment of the law.

Secondly, you refer to "termination on demand," only to dismiss this as being quite unacceptable to the medical profession. While most people would agree with your assertion that backstreet abortions would decline when a cheap, reliable (and I would add safe) contraceptive is discovered, this is still not available.

Meanwhile, it is a little difficult to understand the attitude of mind which readily accepts the intent to prevent the birth of a child by contraception, yet will not accept exactly the same intent when contraception has failed.—I am, etc.,

London W.1.

JAMES CAMPBELL.

Tetanus Immunization

SIR,—It is unwise to be dogmatic about tetanus immunization, but the straight answer to the second of Dr. W. H. Crichton's two questions (17 April, p. 1066) is "No." Tetanus antitoxin is not indicated in a properly immunized person. The difficulty, however, is to be certain (because of the usual unavailability of adequate records) that a person is immune. Then there is so much individual variation in the response to tetanus

vaccine that arbitrary definitions have to be made. Textbooks are usually conservative and retain a policy comprising a full, properly spaced, primary course of three injections of vaccine, followed by two or three reinforcing doses at five-year intervals. (There is now a tendency to extend this interval to seven or 10 years.) There is evidence that, after only two doses of tetanus vaccine, given at some time in the past (even up to 20 years), there remains a "shadow-factory" of immunity ready to spring into immediate, effective action when a stimulating dose of vaccine is injected.¹ It would seem, therefore, that for an immunized or partially immunized person a dose of adsorbed tetanus vaccine (in addition to surgery and antibiotics when indicated) almost always suffices as a specific prophylactic measure. Nevertheless, the circumstances dealt with below may occasionally have to be considered.

Dr. Crichton's first question is more difficult to answer briefly or dogmatically. Consideration of the need for tetanus antitoxin involves the bacteriological problems of tetanus bacillus growth, toxin-production, spore-formation, and spore-germination. Any factor which encourages any of these (or for some reason retards and postpones them) is menacing. Thus, delay in treatment (for more than four hours—not "three days," as Drs. Butler and Benson have it); a deep wound which cannot be treated adequately by immediate surgery; a penetrating or puncture wound when surgery may be impossible or unwise; a wound containing a foreign body; a wound showing devitalized tissue which cannot all be removed; a wound so contaminated as to raise suspicions of tetanus infection—in short, any circumstances which may reduce the efficacy of treatment by means of adsorbed tetanus vaccine and rapid, adequate surgery (with or without the aid of antibiotics) must raise the question: is immediate passive immunization with tetanus antitoxin an *additional* requirement?—I am, etc.,

Beckenham, Kent.

D. A. CANNON.

REFERENCE

- ¹ *Proc. roy. Soc. Med.*, 1965, Meeting on Tetanus Prophylaxis, 58, 221.

Africa's Need

SIR,—Whereas I think it is an excellent move for the Faculty of Medicine at Glasgow University to spare a number of its staff to assist with the medical training of undergraduates in Nairobi, as reported in your leading article (10 April, p. 939), I feel that one should not forget those pioneers who laid the foundations of medical training in Kenya. People like the late Sir Albert Cook, Drs. C. J. Wilson, W. H. Kauntz, and Mr. C. V. Briambridge, whose work remains a lasting memorial to them. Also Mr. N. P. Jewell, who did so much to promote and teach surgery, and Drs. C. R. Philip, D. Plum, and H. C. Trowell, who created, conducted, maintained, and improved the Medical Training Depot in Nairobi.

I sincerely trust that the high standard of British medicine will be maintained in this new venture, which will prove to be beneficial to the future doctors and their patients in East Africa.—I am, etc.,

Peldon,
Near Colchester, Essex.

P. G. PRESTON.

Herpes Zoster and Varicella

SIR,—I was interested by the letter from Drs. B. K. Muller and W. J. Gomes (24 April, p. 1127) on concurrent herpes zoster and varicella, and particularly by their reference to "only five cases" reported in the literature.

On looking through your correspondence columns for the past fifteen years I have found many scores of records of such cases, too numerous to specify. Perhaps I might be allowed to refer to a letter (*Brit. med. J.*, 1964, 1, 1325) in which I analysed the records of all patients with herpes zoster in the wards of this hospital during 1947–53. Of 35 cases, 11 developed a chicken-pox rash, usually 3–5, but in one case as long as 12, days after the onset of herpes. There are good reasons for supposing that sparse varicella vesicles were not looked for, or that if present their significance was not appreciated, in some of those cases in which the dual phenomenon was not recorded.

It is my impression that, in the elderly, varicella lesions are seen in about half of all cases of zoster. This phenomenon is, of course, strong evidence of the unitary origin of the two conditions.—I am, etc.,

Brook General Hospital,
London S.E.18.

C. F. L. HILL.

SIR,—The five cases quoted by Drs. B. K. Muller and W. J. Gomes in their letter of 24 April (p. 1127) do not appear to include any recorded in the *British Medical Journal*. There was correspondence on this subject in the *B.M.J.* in 1960 to which I contributed my own case.¹—I am, etc.,

JOHN S. HAPPEL.

Ropley,
Alresford, Hants.

REFERENCE

- ¹ Happel, J. S., *Brit. med. J.*, 1960, 2, 69.

Mediastinal Obstruction by Radiotherapy

SIR,—Further to your Preliminary Communication by Mr. M. Sutton (20 February, p. 495) two male Royal Free Hospital patients with superior mediastinal obstruction due to lung cancer (Case 1 and Case 2) were treated with the recommended dose of 5 mg. of demecolcine followed four hours later by deep x-ray therapy. Three days after starting treatment with a low daily maximum dose of 50 r at 230 kV. Case 1 had a prolonged epistaxis and was found to have thrombocytopenia, his platelets being 20,000 and 49,000 on consecutive days. His white blood count was within normal limits, the figure being 6,600. He died four days later, the cause of death being carcinomatosis and carcinoma of right lung. Case 2 showed no evidence of bleeding, but he was found to have a platelet count of 26,000 and 52,000 on consecutive days after daily maximum doses of 100 r at 230 kV. for 15 days. The platelet count subsequently rose to 85,000, but he developed intracranial metastases and treatment was stopped. His white blood count was 7,600. Thrombocytopenia in cases treated with demecolcine and radiotherapy has been described¹ but in these cases it is not known whether the platelet count was already depressed prior to starting treatment

or had become depressed as a result of the demecolcine or the radiotherapy, or a combination of both. Further patients with this condition on treatment here by radiotherapy are having platelet counts undertaken, though formerly this was not routine practice during palliative radiotherapy at low daily doses.

We would be interested in any comments on these findings, but would suggest that if demecolcine plus radiotherapy is undertaken for superior mediastinal obstruction careful check should be kept on platelets as well as white blood count.

I am grateful to Dr. Wade for permission to publish this information about her patients.—I am, etc.,

MYRA G. ROBERTSON.

Radiation Department,
Royal Free Hospital.

REFERENCE

- ¹ Sheehy, T. W., and Ransone, J. W., *Arch intern. Med.*, 1957, **100**, 85.

Treatment of the Climacteric Syndrome

SIR,—Your article in "To-day's Drugs" (10 April, p. 976) dealing with treatment of the climacteric syndrome led one to wonder whether the date was 10 April 1965 or 1945. We agree that the statement that "... the object of treatment is to convert an abrupt fall in oestrogen level into a more slowly declining one ..." but why should "... the dose used ... be insufficient to abolish all the symptoms ..." and why is "... the aim to stop treatment altogether as soon as possible"? One can only sense the suggestion that exogenous oestrogens are carcinogenic, although it is now well known there is no evidence to support this theory.¹⁻³

The degree of distress suffered by the menopausal patient is extremely variable, and this in itself appears to have led to the conclusion that the symptoms must, in the main, be psychological. No other endocrine function is based on such an assumption and we think it strange that no attempt is made to measure the oestrogenic activity. The most simple and accurate method of assessing this is by endocrine vaginal cytology, which also serves as a useful therapeutic guide. We have always found a good correlation between the severity of menopausal symptoms and the degree of oestrogen deficiency.

You state that chlorotrianisene (Tace) is not a very potent oestrogen but is of long duration, due to storage in the body fat. During our investigation of the original Tace capsules we found it to be both a rapid and potent oestrogen which gave excellent clinical results, including a marked sense of "well-being, which is rare following the majority of oral oestrogens. We did not, however, find any evidence of its prolonged action in the majority of patients.

In our experience the homoeopathic dose of oestrogen recommended is only suitable for a very small minority of the patients who seek medical help. Indeed, it is difficult to understand why the clinician's chief aim should be to withhold or curtail hormone substitution to the post-menopausal hypogonadal woman. Hormone therapy excites no comment in young hypogonadal women, nor is there any age barrier in thyroid, pancreatic, or adrenal deficiencies.

To-day, when the average woman can expect to reach her threescore years and ten,

it would seem unrealistic to deny hormone treatment, which not only alleviates all the troublesome and antisocial symptoms of the menopausal and post-menopausal phases but is also a prophylactic measure against osteoporosis, senile vaginitis, hypertension, and coronary heart disease, to mention but a few hazards of oestrogen depletion.^{4,5} Why, then, should this treatment be rather grudgingly limited to "months"?

It seems a little paradoxical that the same reasoning is not applied to the male patient following bilateral orchidectomy. Here the rationale is immediate and prolonged hormonal replacement, in spite of a possible deleterious effect on the cardiovascular system. Obviously what is sauce for the gander, is not sauce for the goose!—We are, etc.,

F. OSMOND-CLARKE.

M. MURRAY.

Endocrine Cytological Unit,
Chelsea Hospital for Women,
London S.W.3.

REFERENCES

- ¹ Bishop, P. M. F., and Murray, M., *A.R. Brit. Emp. Cancer Campgn.*, 1957, **35**, 404.
² Larson, J. A., *Obstet. and Gynec.*, 1954, **3**, 551.
³ Wallach, S., and Henneman, P. H., *J. Amer. med. Ass.*, 1959, **171**, 1637.
⁴ Wilson, R. A., and Wilson, T. A., *J. Amer. Geriatr. Soc.*, 1963, **11**, 347.
⁵ Greenblatt, R. B., *New Engl. J. Med.*, 1965, **272**, 305.

Ischaemia and Anaesthesia

SIR,—We were interested in the work of Dr. Charles Sorbie and Dr. Pesi Chacha (10 April, p. 957). Their demonstration of slowing of conduction in the ulnar nerve after stimulation at the elbow by recording from the first dorsal interosseus agrees with our findings¹ after stimulating the ulnar nerve at wrist and elbow. We showed that the effect of lignocaine was on the peripheral part of the neurone and the neuromuscular junction and was probably partly preganglionic and partly post-ganglionic.

Studies of motor-nerve function, such as those of Sorbie and Chacha, do not demonstrate that ischaemia does not contribute to anaesthesia. This can be done only by studies of sensory nerve conduction. Our work showed that sensory conduction changed similarly under the influence of ischaemia alone and ischaemia plus lignocaine. Ring electrodes applied to the fingers seem, therefore, to stimulate sensory nerve trunks, and we draw the inference that lignocaine acts on sensory nerve endings. Experimental work by Diamond *et al.*² came to a similar conclusion.—We are, etc.,

D. W. MILES.

J. L. JAMES.

D. E. CLARK.

Department of Medicine,
The General Infirmary, Leeds.

REFERENCES

- ¹ Miles, D. W., James, J. L., Clark, D. E., and Whitam, J. G., *J. Neurol. Neurosurg. Psychiat.*, 1964, **27**, 574.
² Diamond, J., Gray, J. A. B., and Inman, D. R., *J. Physiol. (Lond.)*, 1958, **142**, 382.

Intravenous Regional Analgesia

SIR,—The high incidence of toxic phenomena associated with the technique of intravenous regional analgesia reported by Kennedy *et al.*¹ has not been observed in clinical practice with this method at the Westminster Hospital.

We are at present engaged in a study of the peak blood levels reached in volunteers after release of the cuff. Following the injection of a 0.5% solution of lignocaine hydrochloride (155–275 mg.) blood samples were analysed by a sensitive specific gas chromatographic technique. In order to simulate the worst conditions with regard to general blood levels likely to be met clinically, the cuff was deflated after only five minutes. Peak blood levels of 1 to 3.9 $\mu\text{g./ml.}$ were reached within seven minutes of releasing the cuff and they fell in such a way that the half-life was approximately 20 minutes.

The 76.2-kg. patient of Kennedy *et al.*, who suffered cardiac arrest, is unlikely to have had a peak level as high as the above as he received only 190 mg. lignocaine hydrochloride and the cuff remained in place for 30 minutes. We found peak levels of 3.5 $\mu\text{g./ml.}$ five minutes after direct intravenous injection of lignocaine (50 mg.) in two volunteers; no untoward reactions were observed.

Furthermore, blood levels of 1.1 to 3.6 $\mu\text{g./ml.}$ were found after epidural injection of lignocaine (470 to 1,130 mg. during a period of up to five hours) to alleviate labour pains.

It is reported by Bromage and Robson,² using the methyl-orange method of determining lignocaine levels, that the minimum toxic concentration in the blood is about 10 $\mu\text{g./ml.}$ Thus it seems improbable that the correct use of intravenous regional analgesia could lead, after the release of the cuff, to a sufficiently high blood concentration of lignocaine to cause untoward effects and certainly not to cause cardiac arrest. It would be unrealistic to abandon this excellent technique on the result of one report in which blood levels of the drug were not determined.—We are, etc.,

R. L. HARGROVE.

J. R. HOYLE.

J. B. R. PARKER.

Department of Anaesthetics,
Westminster Hospital, London.

A. H. BECKETT.

R. N. BOYES.

Chelsea School of Pharmacy,
Chelsea College of Science and Technology,
London.

REFERENCES

- ¹ Kennedy, B. R., Duthie, A. M., Parbrook, G. D., and Carr, T. L., *Brit. med. J.*, 1965, **1**, 954.
² Bromage, P. R., Robson, J. G., *Anaesthesia*, 1961, **16**, 461.

Darwin's Illness

SIR,—The diagnosis of Darwin's illness (20 March, p. 745) has been and apparently will remain a controversial subject. It is obviously now impossible to make a diagnosis with absolute certainty. All that one can do is to examine relevant facts, particularly events prior to his illness, and suggest a diagnosis based on a reasonable degree of probability.

The facts are as follows: (1) It is indisputable that during the voyage of *H.M.S. Beagle* Darwin displayed a degree of endurance well above the average. (2) It is indisputable that on 25 March 1835 he "experienced an attack (for it deserves no less a name) of the Benchuca, a species of *Reduvius* the great black bug of the Pampas." The known distribution and bionomics of triatomid bugs in South America leaves no