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 µg./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 µg./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 µg./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 µg./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

doubt that the "Benchuca" in question was *Triatoma infestans*. (3) It is indisputable that *Triatoma infestans* has been repeatedly proved to be an excellent carrier of *Trypanosoma cruzi*. The natural infection rate with *T. cruzi* in these bugs varies from 20% to 80% in endemic foci. (4) It is therefore obvious that Darwin was exposed to ideal conditions for infection with *T. cruzi*.

An innate neurosis (of which there were no manifestations at the time of his exposure to *Triatoma infestans*) can hardly be expected to act as a prophylactic against *T. cruzi*.

His subsequent symptoms were mainly exhaustion after physical effort (which compelled him to give up field work in geology), a common sign in Chagas's disease, and digestive disturbances, which may be related to the destruction of neurones in Auerbach's plexus, a condition recorded by a number of Brazilian workers in infection with *T. cruzi* in man and animals.¹⁻³

If a person became ill after a visit to an endemic focus of malaria, where he was attacked by mosquito vectors with a high sporozoite rate, it would be reasonable to include malaria in the differential diagnosis. It is equally reasonable to include Chagas's disease in the differential diagnoses of Darwin's illness. It is not intended to propose a rare exotic disease for the diagnosis of Darwin's case. Infections with *T. cruzi* are common in Central and South America, in fact commoner than malaria in many parts. Romana,⁴ in a review of the epidemiology and distribution of infections with *T. cruzi*, has collected the findings of surveys made with the complement-fixation test in various parts of South America. The number of infections in the Argentine is estimated as 1,200,000, in Brazil as 46.8% of the total population of Minas Gerais, 59.9% in Sao Paulo. The majority of the infections occur in rural and suburban populations.

Infections with *T. cruzi* are protean in their manifestations. Many patients are symptomless (but their future course cannot be predicted), others commencing with acute symptoms may recover and subsequently lead a normal life; others without a history of acute illness may develop serious cardiopathies. Pifano⁵ estimates the number of cases in Venezuela at 500,000, of whom 10% show cardiopathies in all stages, from early forms diagnosable only by electrocardiography to advanced crippling disease; the others are symptomless. In other endemic foci 33% of cases develop cardiopathies.

Whether or not Darwin had an innate or acquired neurosis (the latter independent of or in response to organic disease) does not affect the issue with respect to Chagas's disease.—I am, etc.,

S. ADLER.

Department of Parasitology,
Hadassah Medical School,
Hebrew University,
Jerusalem.

REFERENCES

- Koberle, F., *Rev. goiana Med.*, 1957, 3, 155.
- Costa, R. de B., and Lima Filho, E. C. de, *Rev. Inst. Med. trop. S. Paulo*, 1964, 6, 211.
- Alcántara, F. G. de, and Oliveira, J. A. M. de, *ibid.*, 1964, 6, 207.
- Romana, C., *Rev. bras. Med.*, 1962, 14, 549.
- Pifano, C. F., *Gac. méd. Caracas*, 1960, 68, 543.

Occluding the Umbilical Cord

SIR,—The "Better Method for Occluding the Umbilical Cord" described from New Zealand by Dr. J. Crowley (10 April, p. 996)

involves an excellent principle—that whatever is used should be elastic to allow for the fact that the cord shrinks as it dries. And the method he describes for applying his rubber band seems a good one, judging by the fact that it has been hit on independently in so many different parts of the world, including Newcastle upon Tyne¹ and Helsinki.² But it seems wise to draw attention to the fact that a rigorous test of the efficacy of bands cut from latex intravenous tubing, as he describes, has shown them to be ineffective.¹ That Dr. Crowley has encountered no trouble during the first year's use of his method may possibly be due to the fact that the risk of haemorrhage from the cord appears to be only of the order of one in every 1,000 babies.³ This is not a good reason for using an inefficient band, however. Efficient alternatives include the simple stationer's elastic band described by Neligan *et al.*¹—I am, etc.,

G. A. NELIGAN.

Department of Child Health,
University of Newcastle upon Tyne.

REFERENCES

- Neligan, G. A., Parkin, J. M., and Paul, C., *Arch. Dis. Childh.*, 1964, 39, 630.
- Osterlund, K., 1964, personal communication.
- Neligan, G. A., and Smith, M. C., *Arch. Dis. Childh.*, 1963, 38, 471.

Screening for Diabetes

SIR,—We have recently re-examined 134 glucose-tolerance tests performed in the Worthing area during the years 1963 and 1964. Our findings, I think, are of interest and pertinent to the problem of mass screening of a population for diabetes.

In 68 patients diabetic curves were found. Twenty per cent. of these had fasting blood-glucose levels below 120 mg./100 ml. For

instance, the diabetic in the accompanying graph, with a one-hour value of 300 mg./100 ml., had a fasting value of 88 mg./100 ml. None of the two-hour values in this series was below 130 mg./100 ml.

The above observations leave no doubt in my mind as to the superiority of the two-hour blood-glucose level following 50 g. of glucose as opposed to the fasting blood level for detecting diabetes.—I am, etc.,

J. E. TOVEY.

Department of Pathology,
Worthing Hospital,
Worthing, Sussex.

"Medical Assistants Wanted—
No Experience Needed"

SIR,—I lift an evening paper and read the above. It further states: "Qualifications required: you must be 17-25, fit and able to pass a simple entry exam."

"Medical assistant" as a term clearly denotes someone not medically qualified as a doctor but someone who assists a doctor, so who was responsible for applying this inappropriate name to a doctor just below a consultant in status? To one engaged for many years in the practice of psychological medicine it is difficult to believe that any medical man or men would be sufficiently lacking in insight and horse sense to agree to such a term.

The term "medical assistant" for this grade of doctor must be at once rescinded and a common-sense name substituted.—I am, etc.,

Balfon, Stirlingshire. W. W. MACKINLAY.

Sucking Out Splinters

SIR,—Recently an old Dorsetshire hedger taught me the following non-touch technique for the removal of splinters and thorns from any part of the body, by a method which is both painless and efficient.

The procedure is as follows: a narrow-necked bottle is filled with hot water, and emptied again when the glass is as hot as possible. The neck of the bottle is then placed over the splinter, and as the bottle cools down a partial vacuum is created, thus drawing the splinter towards the surface of the skin. This procedure may be repeated several times as necessary. Obviously the hotter the water, the larger the capacity of the bottle, and the narrower its neck, the greater will be the negative pressure created. In over 50 years of constant use, this old countryman claims that the method has never once failed him.

This technique, which is simple, safe, and sure, ought certainly to find a place in every textbook of first aid, and be in every casualty officer's handbook.—I am, etc.,

Blandford, Dorset. DAVID C. LANGLEY.

Testing for Phenylketonuria After
Infancy

SIR,—I was interested in the last paragraph of Sir Alan Moncrieff's letter particularly (17 April, p. 1065), emphasizing the need to test older children. The following case might be of interest:

A boy aged 8½ attending a country primary school was brought forward for examination by the school medical officer as being backward and so "difficult" that there was the question of exclusion from school. On examination he was found to be exceedingly euphoric, erratic in behaviour, completely lacking in concentration, and with an I.Q. of 68. He was diagnosed by this medical officer (not the writer) as being a suspicious case of phenylketonuria, this being then confirmed by the consultant paediatrician. He was put on a strict diet and also on the waiting list for schools for the educationally subnormal. The parents then sent him to a small private school in the meantime. He was seen regularly, however, both in hospital and by the school medical officer. The parents were scrupulous regarding his diet and when he was seen at 9½ years his I.Q. had risen to 74, although his behaviour was still abnormal. At 10 years the picture was the same, but by 12 years he had improved sufficiently to be admitted to the small local secondary school, where he made definite progress in his work and behaviour and had every encouragement from the staff. At 13 years his I.Q. had risen to 85 and he was considered suitable for transfer to a large modern secondary school along with the rest of his small school, which was closed. He became well