While a survey of reports on this topic showed no mention of peripheral nerve paresis during TPN parenteral nutrition, I found two cases of Wernicke encephalopathy after prolonged intravenous therapy that were probably due to thiamine deficiency.² Since the patients mentioned by Dr Ladefoged and Dr Jarum received only 9.7 mg thiamine once or twice a week, I think that a thiamine deficiency could probably explain the paresis of the peroneal nerves and perhaps of the radial nerves too.

These facts should alert the clinician to suspect a "parenteral-nutrition-related beri beri" if polyneuropathy without any evident cause appears in patients fed intravenously (the determination of transketolase levels might prove a reliable laboratory test in this case). They suggest that about 0.3 mg thiamine/kg/day should be given to patients receiving parenteral nutrition as has been recently advocated.³

FEDERICO BOZZETTI

Istituto Nazionale per lo Studio e la Cura dei Tumori, Milan, Italy

¹ Ziporin, Z Z, *et al*, *Journal of Nutrition*, 1965, **85**, 297.
² Nadel, A M, and Burger, P C, *Journal of the American Medical Association*, 1976, **235**, 2403.

³ Wretling, A, *Surgical Clinics of North America*, 1978, **58**, 1055.

How to take a clinical examination

SIR,—I started to read the article by Dr J F Stokes (13 January, p 98) with interest because he has been concerned with the clinical examination of the MRCP for a number of years, but as I read my disappointment and later annoyance grew. Regarding the Adie pupil, he suggests that the examiner will have arranged for the pupil to be dilated if the daylight in the ward is bright and suggests that the candidate should ask the patient if he has had drops in his eye, which would presumably have been homatropine. He does not seem to appreciate that the drops should be 2.5% mecholyl, which constricts the Adie pupil but not the normal pupil.¹ If homatropine had been put into the eye to dilate the pupil, it is doubtful if it would be possible to recognise the Adie pupil anyhow. Nowhere in the article does he suggest that the urine should be tested or a sample asked for or that the blood pressure should be measured.

Towards the end of the article, Dr Stokes remarks that it is better not to appear obviously pregnant, which must be offensive to female candidates. Many of the most distinguished doctors in Britain today are women and it is irrelevant if they took the MRCP when pregnant.

K N V PALMER

Department of Medicine, University of Aberdeen

¹ Beeson, P B, and McDermott, W, *Textbook of Medicine*, p 328. Philadelphia, Saunders, 1971.

* * * We sent a copy of this letter to Dr Stokes, whose reply is printed below.—Ed, *BMJ*.

SIR,—The lines are crossed. I had tried to put myself in the position of a candidate who found a patient with unequal pupils and was uncertain whether one had been dilated to make it easier for him to see the fundus or whether he was faced with a case of Adie's syndrome. The question of whether I am able or unable to distinguish between the uses of homatropine and mecholyl does not arise.

I do not undervalue the importance of the blood pressure or of urine testing. In an article of limited length it seemed best to concentrate on points which might be less generally recognised.

If susceptibilities have been offended by a reference, in what I had hoped might be helpful terms, to the variable reactions I have observed in examiners to a physiological state (which may be met at graduating as well as postgraduate examinations), it is a matter for regret—though I do find it difficult to see how it can be interpreted as being denigratory to our admirable company of medical women.

J F STOKES

Nr Henley-on-Thames, Oxon

Early growth retardation in diabetic pregnancy

SIR,—The observation of Drs Jan Fog Pedersen and Lars Mølsted-Pedersen (6 January, p 18) that in a group of insulin-dependent diabetics 10 of 35 patients whose fetuses were "small for dates," by ultrasound measurement of the crown-rump length between 7 and 14 weeks postmenstrual age, subsequently delivered smaller babies than patients in whom the fetal crown-rump length measurements were appropriate for dates is interesting. However, fuller clinical detail is required before their hypothesis that there is a different pattern of growth retardation in diabetic pregnancies can be accepted.

In considering such a small number of cases the mean birth weight can readily be biased by an uneven distribution of the fetal sex, birth rank, maternal size, cigarette use, and the presence of complications such as pre-eclampsia. For example, the average effect of the first three factors at term would be as follows: male *v* female, +150 g; first pregnancy *v* subsequent pregnancies, -150 g; 175 cm/70 kg mothers *v* 150 cm/45 kg mothers, +600 g.¹ Also babies with congenital abnormalities should be excluded in view of their common association with intrauterine growth retardation. If after these factors are allowed for a significant difference in the mean birth weights remains, the hypothesis that the fetal age is less than the postmenstrual age in the "small" group cannot simply be dismissed as unlikely because the menstrual history was certain. There is considerable evidence to suggest that ovulation is not nearly as constant as implied in Naegle's rule. Boyce *et al*² studied 317 pregnancies with reliable basal body temperature charts and found that in 30% conception occurred more than two weeks beyond the usual day of ovulation calculated by Naegle's rule. The gestational age and birth-weight limits used in compiling the control series would be expected to exclude such cases and could account for the different distributions of crown-rump length measurements in the control and diabetic series.

Ultrasonic fetal measurement in early pregnancy has repeatedly been shown to predict the onset of spontaneous labour resulting in the delivery of a mature fetus more precisely than an impeccable menstrual history.³ Experience shows that when a fetal measurement falls below the normal range for the gestation age because of growth retardation the growth rate usually remains below normal. But a small measurement associated with a normal growth rate (as in Pederson and Mølsted-Pederson's cases)

indicates incorrect assignment of the gestational age.³

If our hypothesis is correct that the fetal age of the "small" group was on average 12 days less than that of the "normal-size" group, then a simple calculation from normal birth-weight data¹ suggests that this could account for a 300-500 g weight difference.

We are particularly concerned that obstetricians may be persuaded to disregard the possibility of an error in the expected date of delivery predicted by the menstrual history and deliver such fetuses earlier than would be their normal practice in diabetic pregnancy.

D J LITTLE

S M STUBBS

MICHAEL BRUDENELL

STUART CAMPBELL

Department of Obstetrics and Gynaecology, King's College Hospital, London SE5

¹ Thomson, A M, *et al*, *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1968, **75**, 903.

² Boyce, A, *et al*, *American Journal of Obstetrics and Gynecology*, 1976, **125**, 911.

³ Campbell, S, in *Fetal Physiology and Medicine*, ed R W Beard and P Nathanielsz. London, W B Saunders Co. Ltd, 1976.

Ergotamine tartrate overdose

SIR,—It is interesting to note that the difficulties of giving an effective treatment with ergotamine tartrate in migraine are given publicity. I refer to a letter from some well-known physicians with an interest in migraine whose signatures are headed by Dr J N Blau (27 January, p 265).

Most surprisingly, hypnosis and autohypnosis are seldom mentioned in connection with this distressing complaint—it is without doubt the treatment of choice when simple analgesics fail. Much of the poor reputation of hypnosis of past years has been due to poor patient selection, setting of unrealistic goals, and uncertainty of application; but none of these apply where the treatment of migraine is concerned. Impressive results have been reported by Harding,¹ Cedercreutz,² and Perkins.³

I was involved in a multicentre trial to compare hypnosis and autohypnosis with prophylactic prochlorperazine mesylate (Stemetil) and therapeutic ergotamine tartrate.⁴ Independent assessment was applied by a "blind" observer so far as possible after a 12-month trial period. It was found that those having hypnosis or autohypnosis had significantly fewer attacks in the first six months of the trial than the six months preceding the trial, and in the last six months than the first six months. The chemotherapy group showed no significant change for either the period prior to the trial or the two six-month trial periods.

There were 13 patients having grade IV attacks prior to the trial in the hypnosis group. This was reduced to five in the last three months of the trial. Ten patients were having grade IV attacks in the chemotherapy group, which went up to 14 after chemotherapy. Finally, out of 23 patients having hypnotherapy, 10 patients had complete remission with no attacks in the last three months, while only three of the 24 chemotherapy patients showed similar remission.

While it is perfectly true that there is a tremendous variability in hypnotic susceptibility, the majority of patients can be