though in form participles, are in fact nouns, many people who commit the error of using fused participles would not do so if they were using pronouns rather than proper names or titles for the people concerned. Indeed Dr Bradshaw used the gerund correctly when he hoped that "you will put your editorial heads together and so avoid . . . their being banged together later."

It should be noted that where the two editors are concerned compound possessives are required, since the possessive must immediately precede the gerund—that is, "the possibility of the editor of *World Medicine's* having delusions of grandeur and that of the editor of the *British Medical Journal's* ignoring conventions. . . ." When this construction becomes awkward (usually because of the excessive length of the compound possessive) the construction should be changed, since the possessive case *must* be retained with a gerund.

I now await with interest the exposure of my own solecisms.

PATRICK BYE

Piltown, Sussex

¹ Fowler, H W, *A Dictionary of Modern English Usage*, 2nd edn, revised E Gowers, reprinted with corrections, p 215. London, Oxford University Press, 1970.

Insulin U40, U80, or U100?

SIR,—I am sure that everybody agrees with Dr Joanna Sheldon and her colleagues (27 November, p 1319) that the present system is rather confusing when insulin of different strengths is administered with a syringe whose graduation was devised at a time when insulin was available only in a strength of 20 U/ml. I also agree that it is an excellent idea to use only one strength of insulin with a syringe which is graduated accordingly.

However, if this cannot be arranged in the very near future, I believe that the second best remedy would be to cease forthwith manufacturing these completely outdated "insulin syringes" and to supply diabetics with syringes which are graduated in millilitres, since it is much less confusing to tell a patient who, for example, uses insulin U40 to inject "20 units, which is $\frac{1}{2}$ ml" than to tell him to inject "20 units which is 10 units" (on the syringe).

B GALANDAUER

London E5

Percutaneous transhepatic cholangiography

SIR,—We write in support of Dr G M Fraser and his colleagues from Edinburgh (27 November, p 1321). Like them we feel that Dr Iain M Murray-Lyon and Mr Keith Reynolds (16 October, p 923) overstress the complications associated with the Chiba needle. A similar point of view has been put forward in another recent review from the Royal Free Hospital¹ suggesting that the complication rate is not significantly different from that for the older sheathed needle technique.

We have performed slim-needle percutaneous cholangiography on 45 consecutive patients with a clinical diagnosis of obstructive jaundice. In 44 the biliary ducts were outlined, including six patients with intrahepatic cholestasis and undilated ducts. No serious complications were encountered and cholangitis was not seen. Laparotomy was delayed for one

to seven days in those patients with extrahepatic obstruction and only one was found to have developed a biliary leak, which may have resulted from a fistulous communication between a carcinoma of the gall bladder and the colon.

We now feel that this technique is the procedure of choice in patients thought to have extrahepatic obstruction and has, in our unit, replaced endoscopic retrograde cholangiography.

G DE B HINDE
P M SMITH

Department of Radiodiagnosis
and Medicine,
Llandough Hospital,
Penarth, S Glam

¹ Elias, E, *Gut*, 1976, 17, 801.

Ultrasonic measurement of liver size

SIR,—Dr S Sullivan and his colleagues (30 October, p 1042) have drawn attention to the inaccuracies in clinical estimation of liver size. They conclude that more reliable methods should be used in studies in which liver size is of diagnostic, prognostic, or therapeutic importance. They draw particular attention to the problem of assessing the level of the upper border of the liver. We have compared this level as obtained clinically by percussion (two observers) to that found from an immediate longitudinal ultrasonic B-scan in the mid-clavicular line. The level assessed clinically in 11 healthy subjects was between 2 and 6 cm lower than that obtained by ultrasound (see figure). Thus change in percussion note, usually taken as the level of upper border of the liver, is consistently lower than the true level.

Longitudinal ultrasonic B-scan in the midclavicular line. A—clinically determined level of upper border of liver; B—ultrasonically determined level.

Ultrasound provides a safe, non-invasive method for measuring liver size.¹ Although transverse ultrasonic B-scanning can outline the liver in its lower part, the upper dome is obscured by overlying echogenic lung tissue. We have overcome this problem by using a combination of transverse and longitudinal scans. With the patient supine and breathing quietly serial transverse scans at 1-cm intervals are recorded from the inferior limit of the liver of the upper limit at which liver can be outlined. Serial longitudinal scans in the sagittal plane delineate the liver edge above this level. All scans are photographed and the

liver outline transcribed on paper. The whole areas of liver on the transverse scans and the appropriate areas on the longitudinal scans are measured using a computer fitted with a graphic tracing device. Liver volume is derived as the sum of all areas measured multiplied by the magnification factor used. This method has proved useful for clinical research.² It yields reproducible results (coefficient of variation 6%). In 20 normal subjects the liver volumes obtained correlated with body weight ($r + 0.81$) and were consistent with allometric predictions of liver volume. In two patients who died within a week of ultrasound estimation the values obtained differed by less than 1% from post-mortem liver volumes.

We are indebted to Dr F Ross and Dr R Wells for use of equipment and facilities.

M HOMEIDA
C J C ROBERTS
M HALLIWELL
LYN JACKSON
A E READ

University Department of
Medicine,
Bristol Royal Infirmary,
Bristol

¹ Rasmussen, S N, *British Journal of Radiology*, 1972, 45, 579.

² Roberts, C J C, et al, *British Journal of Clinical Pharmacology*, 1976, 3, 907.

Road safety: BMA comments

SIR,—The publication of the BMA's comments on the Government's consultative document dealing with road safety (23 October, p 1000) is of considerable interest. It emphasises that road accidents are a major cause of permanent disability and one therefore in which the medical profession should have a preventive role.

There are two interlinked factors which bear consideration. The first relates to speed limits. At the present time there is much debate as to whether these should be increased, since they have had little effect in terms of fuel saving. This discussion continues about raising the limits in spite of the clear evidence from the United States that both mortality and morbidity have been considerably reduced as a result of speed restrictions that were enforced in order to conserve energy reserves. Secondly, the higher the speed, the greater is the chance that head injury as a result of an accident will be severe and the likelihood of subsequent epilepsy increased. Indeed, Caveness¹ puts forward the view that road traffic accidents in the USA are the main cause of acquired epilepsy in adults. For this reason it is important that any pressure on the Government by motoring organisations to increase speed limits should be tempered by the knowledge that such an increase will not only lead to greater mortality but also to morbidity of many kinds, including epilepsy.

Clearly environmental factors such as road surfaces, weather conditions, and the state of the vehicle influence the incidence of accidents and obviously affect mortality and morbidity. These in their turn involve the Health Service in monetary cost for medical treatment. In spite of this there is no way of assessing the amount of grief and suffering caused to families as a result of these often serious mishaps. For all these reasons the suggestion in the BMA's comments that some funds be

devoted to the human aspects of road research, perhaps through the Medical Research Council, is not merely timely but long overdue.

D F SCOTT

EEG Department,
The London Hospital (Whitechapel),
London E1

¹ Caveness, W F, *Epilepsia*, 1976, **17**, 207.

Sodium valproate in chorea

SIR,—The report by Dr J A R Lenman and others (6 November, p 1107) that no improvement in patients with Huntington's chorea after sodium valproate therapy could be demonstrated implies that gamma-aminobutyric acid (GABA) may not play a role in the pathogenesis of this disease. A more likely explanation is that sodium valproate does not affect brain GABA concentrations to any substantial degree.

The authors state categorically that sodium valproate inhibits GABA transaminase. This point is not established.^{1 2} Doses as high as 400 mg/kg intraperitoneally in mice failed to influence brain GABA concentrations.² Even in those cases in which the brain GABA concentration was found to be elevated rises in brain GABA were small (maximum of 34% increase following 400 mg/kg intraperitoneally³) and short-lasting (return to control values by 180 min³). The clinical doses used by Dr Lenman and his colleagues were fractions of those which equivocally increase brain GABA in mice. Further, no data are presented to verify biochemically any change in brain GABA (for example, increased cerebrospinal fluid GABA).

One can therefore anticipate that correction of the decreased brain GABA concentrations in patients with Huntington's chorea by more potent inhibitors of GABA transaminase will prove efficacious in treatment.

PAUL J SCHECHTER

Section of Experimental Therapeutics,
Centre de Recherche Merrell International,
Strasbourg, France

¹ Harvey, P K P, Bradford, H F, and Davison, A N, *FEBS Letters*, 1975, **52**, 251.

² Anlezark, G, et al, *Biochemical Pharmacology*, 1976, **25**, 413.

³ Simler, S, et al, *Biochemical Pharmacology*, 1973, **22**, 1701.

SIR,—Dr J A R Lenman and his colleagues (6 November, p 1107) administered sodium valproate to patients with Huntington's chorea in the expectation that the resulting increase of brain gamma-aminobutyric acid (GABA) levels would reduce choreiform movements. No benefit was observed.

I wish to report my own experience of sodium valproate in the treatment of six patients with choreiform movements. The first, a patient with Huntington's chorea was

treated with 1200 mg of sodium valproate daily. A great reduction of the chorea was obtained, and this benefit persists after six months' therapy. In view of this striking result five other patients were treated. The clinical details, dosage, and results of treatment are shown in the table below.

It is hard to account for the results. The efficacy of control of the chorea was so good that it seemed to justify further therapeutic trial. Patient 2, who did not benefit was in many respects a very similar case to patient 1, who responded excellently. It seems most unlikely that the small dose of diazepam used in addition to the sodium valproate in the treatment of the first patient would account for the difference as diazepam alone is of no benefit in chorea. Possibly in the other cases there was a difference in the nature of the underlying biochemical complaint. Unfortunately increases in the dosage of sodium valproate were precluded by the development of drowsiness. The predominantly negative results prevent legitimate comment on the relevance or otherwise of GABA transmitter systems in chorea. Clearly the current therapy for chorea, tetrabenazine, remains the first choice of treatment.

G M YUILL

Department of Neurology,
Crumpsall Hospital,
Manchester

Treatment of idiopathic ascites of haemodialysis

SIR,—I read with interest the report by Dr B F Jones and others (10 April, p 877) of the successful treatment of the idiopathic ascites of haemodialysis by paracentesis followed by instillation of triamcinolone acetonide. This is the first confirmatory communication I have received regarding the usefulness of this treatment. I have not had an opportunity to apply the therapy in our population since our first report¹ because no other patient has developed the complex here since that time. It is of interest to me that the authors used a very small dosage of triamcinolone acetonide (200 mg) with good results. On a purely empirical basis we used much higher dosages (500 mg every 4 h) in the two cases of ascites we reported. We also used a long period of drainage (24-72 h), instilling the triamcinolone through an intermittently clamped drainage catheter until there was no more evidence of recurrent ascites as shown by lack of drainage.

There is no question that catheter drainage and local instillation of triamcinolone acetonide is effective therapy for intractable uraemic pericardial effusion²; however, very few cases of idiopathic ascites of haemodialysis have been treated in this manner. I would be interested to hear of the experience anyone else has had with this treatment in cases of idiopathic ascites of dialysis. I am particularly anxious to know if others are using this procedure, as in

this case report, by the method of a single drainage and instillation.

T J BUSELMEIER

University of Minnesota Hospitals,
Box 281,
Minneapolis, Minnesota 55455

¹ Buselmeier, T J, et al, *Proceedings of the Clinical Dialysis and Transplant Forum*, 1975, **5**, 9.

² Buselmeier, T J, et al, *Nephron*, 1976, **16**, 371.

Postcoital contraception

SIR,—It is unfortunate, though understandable, that your leading article on this subject (23 October, p 961) should have made no mention of the legal and ethical problems involved that ought to be faced but are usually avoided.

There is a great difference between contraception (preventing fertilisation of the ovum) and the use of an abortifacient after fertilisation has occurred. In the former situation the ovum and spermatozoa are independent organisms, but in the latter their nature is profoundly changed and they are united as an early pregnancy with quite different potential. The term "postcoital contraception" implies the use of an agent after coitus but before fertilisation has occurred, but is clearly being used for agents that produce a very early abortion.

Apart from the ethics of this practice, which cause concern, it has been pointed out by Tunkel¹ and emphasised more recently by Brewer² that the provisions of section 1 of the 1967 Abortion Act permit an abortion only when two doctors agree that specified conditions exist, and section 5 makes the intent to procure a miscarriage illegal unless section 1 is adhered to. As the use of diethylstilboestrol, ethinyloestradiol, prostaglandins, or their synthetic analogues, intrauterine devices, copper coils, menstrual extraction, and any other postcoital techniques have the intention of procuring an early abortion they would appear to be in contravention of section 58 of the Offences Against the Person Act 1861 unless section 1 of the Abortion Act 1967 has been observed.

DAVID J HILL

Addenbrooke's Hospital,
Cambridge

¹ Tunkel, V, *Criminal Law Review*, 1974, August, p 461.

² Brewer, C, *World Medicine*, 1976, **11**, No 17, p 33.

Leprosy in Northern Nigeria

SIR,—I was very surprised to read in "Personal View" (20 November, p 1250) the statement by Professor Harold Scarborough, dean of the faculty of medicine at Ahmadu Bello University, Zaria, Nigeria, that "no one knows the prevalence of leprosy in this part of the world." Ross, who pioneered the outpatient treatment of leprosy with dapsone in Northern Nigeria, carried out numerous surveys throughout the region in the early 1950s. In particular, in the area adjacent to the present medical school he measured the prevalence by population survey in the villages of Igabi and Giwa and found figures of 67 per 1000 and 39 per 1000 respectively. In Zaria Province it was 46 per 1000.¹ In Igabi, after seven years' administration of dapsone, it had declined to 50 per 1000. In 1967 I carried out further surveys in these villages and found that the prevalence had declined to 2 per 1000 and 2.5 per 1000 respectively. The prevalence in Northern Zaria, based on outpatient attendance, was 1.6 per 1000.² The results

Effects of sodium valproate on six patients with choreiform movements

Patient ¹	Age (years)	Diagnosis	Duration of chorea (years)	Daily dosage of sodium valproate	Daily dosage of other drugs	Effect on chorea
1	44	Huntington's chorea	2	1200 mg	Diazepam 15 mg	Virtually abolished
2	54	Huntington's chorea	12	600 mg	—	No benefit
3	61	Hereditary chorea	7	600 mg	—	No benefit
4	67	Arteriosclerosis	2	600 mg	—	No benefit
5	67	Arteriosclerosis	2	600 mg	—	No benefit
6	75	Arteriosclerosis	6/12	600 mg	—	No benefit