electroencephalogram the morning after hypnotics or placebo were taken. However, blood samples were taken from the tubing of a catheter and could often be withdrawn without waking the subject. Furthermore, the study was double-blind and thus similar blood sampling took place after placebo, in comparison with which temazepam and chlormethiazole were shown to be effective hypnotics.

Drs Oswald and Adam would also have preferred that a battery of psychomotor tests had been used. The "e"-deletion test was used because it is a simple psychomotor test; there was no suggestion that performance in this test correlates with piloting an aircraft or driving down a motorway. The test was used, in conjunction with other measures, as a means of comparing drug and placebo effects. The "e"-deletion test was previously sufficiently sensitive to detect hangover effects up to 36 hours after a single dose of nitrazepam in elderly subjects.¹ In the present study there was a significant difference (Wilcoxon signed ranks test) between the performances of the old and the young ($p < 0.01$), and within each group between those found 4 and 11 hours after dosing ($p < 0.005$). We have found (unpublished observations) that "e"-deletion correlates well with more complicated tests such as the paced auditory serial addition test² in young subjects, but that older subjects found the latter test too difficult to perform and gave up before completing it. Other workers have shown that elderly women with a history of falls on rising from bed had a significantly greater degree of sway measured by ataximeter than their peers.³ Thus it is relevant that sway was not increased by hypnotics in our subjects. Again, the measurement was sufficiently sensitive to detect a significant difference in sway between the age groups ($p < 0.01$). Finally, Drs Oswald and Adam criticise our failure to use analysis of variance and accuse us of ignoring the element of chance in our analysis of pharmacodynamic tests. Non-parametric tests were deliberately used to avoid making assumptions about the distribution of the data, even though levels of statistical significance are less easily achieved with such tests. In the table in our paper 44 measurements after drugs and placebo were compared, of which 15 (34%) showed significant differences. It is a strange probability theory that can attribute this to chance.

With regard to the comments of Dr A F Macklon and others (22 March, p 861), we have again checked the elimination half life of temazepam from the logarithmic plot of the mean plasma concentration time curve; this cannot be interpreted as showing an elimination half life as long as 26 hours for either group. As we stated, there was marked variability in plasma concentrations between subjects, and insufficient estimations were made in many to allow individual beta elimination half lives to be calculated. This variability would almost certainly be increased in those suffering from illness at any age, and thus caution is necessary when extrapolating the data from normal volunteers to patients.

There is ample evidence that many hypnotics, including the commonly prescribed long-acting benzodiazepines, are unsatisfactory, particularly in the elderly—as reiterated in a subsequent leading article (15 March, p 743). While temazepam and chlormethiazole are not ideal for all subjects, it is surely justifiable to claim that they are an improvement on most alternatives.

R S BRIGGS
M CASTLEDEN
C A KRAFT

University Department of
Geriatric Medicine,
General Hospital,
Leicester LE5 4PW

¹ Castleden CM, George CF, Marcer D, Hallett C. *Br Med J* 1977;ii:10-12.
² Gronwall D, Wrightson F. *Lancet* 1974;ii:605-9.
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Prophylaxis against febrile convulsions

SIR,—Paediatricians and general practitioners should attempt to differentiate on the one hand between a simple febrile convulsion and on the other a convulsion precipitated by fever in a child who will later have epileptic fits. This is more than a matter of semantics as the prognosis and therefore the management of the two conditions are different. The label "febrile convulsion" should be reserved for patients between the age of 12 months and 5 years who have a generalised fit lasting not more than 10 minutes occurring once during an episode of fever. There should be no pre-existing history of neurological disorder, nor should there be any abnormal neurological findings.

In contradistinction, any history of cerebral birth injury, current neurological abnormality, or evidence of prolonged convulsion should alter the diagnosis to one of an epileptic fit precipitated by fever. Wallis and Aldridge-Smith (9 February, p 353) blur this distinction and confuse the issue by including in their group of children with "febrile convulsions" those who have obvious neurological disease.

We accept that further fits in epileptic children should be prevented where possible and that sodium valproate and phenobarbitone are two drugs which have been used successfully for this purpose. There is at present, however, little evidence that sodium valproate is effective in the prevention of simple febrile convulsions,¹ although phenobarbitone given in doses which result in therapeutic serum concentrations may be.² Furthermore, Wallis and Aldridge-Smith minimise the side-effects of drugs unduly. Phenobarbitone frequently produces unacceptable effects, particularly on behaviour. Although sodium valproate is relatively safe it can effect the haemopoietic system and liver function, with occasional irreversible hepatic failure.³ It is therefore important to determine the need for long-term anticonvulsant therapy before starting any treatment.

If paediatricians and GPs use the simple criteria stated above, they should save a large number of children from unnecessary treatment.

JENNIFER COWEN
FRANK HARRIS

Alder Hey Children's Hospital,
Liverpool L12 2AP

¹ Williams AJ, Evans-Jones LG, Kindley AD, Groom PJ. *Clin Pediatr (Phila)* 1979;18:426-30.
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SIR,—Dr Sheila J Wallace and Mr J A Smith (22 March, p 864) says that "up to 11% of children with fits may convulse to death." What an alarming assertion, enough to make any parent or paediatrician break out in a cold sweat. Your readers may, however, find reassurance in the fact that the paper quoted¹ was written 30 years ago about 91 children admitted to hospital in Finland from 1920 to 1941 and that all that was said about them is that they had convulsions associated with infection and 10 of them died. Nowhere in the paper is it said that the children "convulsed to death." It seems very likely that the cause of death was the infection rather than the convulsion in most, if not all, of those children. Among 1706 American children with febrile

convulsions in the 1960s there were no deaths or persistent motor deficits.²

D P ADDY

Postgraduate Centre,
Dudley Road Hospital,
Birmingham B18 7QH

¹ Ekholm E, Niemineva K. *Acta Paediatr* 1950;39:481-501.

² Nelson KB, Ellenberg JH. *Pediatrics* 1978;61:720-7.

Intravenous chlormethiazole in status epilepticus

SIR,—We were interested to read the article by Dr S Lingam and others (19 January, p 156) on problems with intravenous chlormethiazole in status epilepticus. We have found a further problem associated with this drug given by the intravenous route in that the droplets tend to be of much smaller size than equivalent droplets of electrolytes, etc. Therefore the volume delivered by any pump system that measures drops per minute will in fact be considerably less than expected. This can lead to under-dosage with this drug.

T V STANLEY

Department of Paediatrics,
Wellington Hospital,
Wellington 2, New Zealand

K M GOEL

Royal Hospital for Sick Children,
Glasgow G3 8SJ

Unrecognised dehydration during parenteral nutrition?

SIR,—It is unfortunate that your "Lesson of the Week"—"Unrecognised dehydration during parenteral nutrition"—by Drs R W G Prescott and J C Stoddart (22 March, p 847) is inaccurate in title, fact, deduction, and conclusion.

The osmolality/osmolality of Glucoplex is 2387 mmol/l (2387 mosmol/kg; 2072 mmol/kg) not 3125 mmol/l, and the free water is 0.745 g water/ml not 0.764 g/ml as stated (personal communication, Messrs Geistlich Sons Ltd). These figures are, of course, irrelevant. If the glucose in the hyperosmolar solutions is "burnt," it represents no osmolar load. Metabolic water is produced giving a total of 0.985 g water/ml. "Correction" for fractional water content is obviously incorrect. If, however, the mass of glucose given exceeds the mass metabolised, glucose or its metabolites will accumulate. The intracellular osmolality rises and water is transferred from the extracellular compartment to the intracellular compartment. The extracellular sodium therefore rises. The extracellular osmolality reflects the intracellular environment.

In addition to the high serum sodium concentration, the hyperosmolar syndrome due to carbohydrate overload has other features. (1) The carbohydrate calorie input exceeds the measured carbon dioxide output by more than 25%. (2) The glucose level usually becomes more difficult to control. (3) Urinary sodium excretion paradoxically drops and may be less than 10 mmol/l in extreme cases. (4) Oliguria is usually obscured by clinical management. (5) The mean corpuscular volume rises, reflecting the increase in size of the intracellular compartment, which is due partly to a similar increase in size of the red cells themselves and partly to the method of measurement.¹

On the basis of the authors' figures, cases 1 and 3 were in positive fluid balance and negative sodium balance, yet had large rises of the serum sodium. Calculation suggests a shift of extracellular water into the intracellular compartment of 21% in case 1 and of 16% in case 3. Diagnosis: Over-provision of carbohydrate calories. Management: Stop intravenous nutrition until the small molecules

are metabolised, and the serum sodium drops to less than 150 mmol/l. Intravenous nutrition may then be restarted at a level indicated by the carbon dioxide production (calories) and the nitrogen loss (amino acids). Do not increase water intake.

Case 2 has a different aetiology. The positive sodium balance would be expected to give a rise in serum sodium of 25 mmol/l. The observed rise suggests a shift of water from intracellular to extracellular compartments of 9% of extracellular volume as a result of excessive sodium intake. The water shift is in the reverse direction from that seen in cases 1 and 3. Diagnosis: Sodium overload. Management: The sodium intake should be restricted and water intake increased, since the intracellular compartment is depleted. There is no indication for withdrawing intravenous nutrition.

The "Lessons of the Week" are therefore: (1) A high serum sodium concentration may be due to dehydration, carbohydrate calorie overload, or sodium overload. (2) Careful attention to glucose metabolism, sodium and water balance, and transcellular shifts can differentiate the causes and hence delineate the management of the problem.

C C WISE

Intensive Care Unit,
Cardiff Royal Infirmary,
Cardiff CF2 1SZ

¹ Allen SC, Balfour IC, Wise CC. *J Clin Path* (in press).

SIR,—The problem of "inexplicable" hypernatraemia in very sick patients undergoing intensive therapy is all too familiar, but I found the explanation offered by Drs R W G Prescott and J C Stoddart in their article (22 March, p 847), on this topic less than convincing.

Two of their patients were shown to have positive "corrected" water balance with a negative sodium balance, which seems to me to compound rather than explain the problem. I endorse their statement that parenteral nutrition can induce dehydration but believe that this is usually due to osmotic diuretic mechanisms in cases where the blood sugar is inadequately controlled or the administered amino-acids are poorly utilised. While hypertonic glucose solutions do have a fractional water content considerably less than unity, 1 g of glucose when metabolised produces 0.6 ml of water, so that 1 litre of a 40% glucose solution (for example, Glucoplex 1600) although only containing 764 ml of water, produces an additional 240 ml of metabolic water. Whether or not this was taken into account, if we assume an accurate sodium balance these patients must have been considerably more dehydrated than even the "corrected" fluid balances indicated.

Perhaps the real "lesson of the week" is that it is almost impossible to assess water balance accurately in very sick, febrile hypermetabolic patients, because of the inevitable errors incurred in estimating metabolic water production and water losses via skin and lungs. Accurate weighing, which is now a simple procedure in the intensive care unit, reduces many of these errors but there remains the problem due to changes in body cell mass, which really can only be resolved by radioisotope techniques.¹

R G WILKES

Royal Liverpool Hospital,
Liverpool L7 8XP

¹ Shizgal HM, Spanier AH, Kurtz RS. *Am J Surg* 1976;131:156.

Bacterial infection in the newborn

SIR,—Dr H B Valman (15 March, p 772) relegates chlamydial neonatal conjunctivitis to a mild condition. The spectrum of clinical symptoms is wide, from severe conjunctivitis indistinguishable from that produced by gonococcus (which may coexist) to little more than a "sticky eye." In an unpublished study at this hospital *Chlamydia trachomatis* was responsible for over 40% of purulent conjunctivitis. Up to 50% of the babies born to women harbouring *C trachomatis* in the cervix will develop chlamydial conjunctivitis.¹

In-vivo antibacterial studies have shown that chloramphenicol is unreliable in eradicating *C trachomatis*.² The organism will be suppressed, but the frequency of relapse is high, often after the baby has been discharged from hospital. A mild conjunctivitis may persist, which while usually self-limiting may result in impairment of sight.³ A further effect of chloramphenicol therapy will be the failure to isolate the organism in the laboratory if a diagnosis of *C trachomatis* infection is considered after therapy has commenced. The use of neomycin for the early treatment of ophthalmia will be as effective as chloramphenicol against staphylococci and coliforms, but will not impair the ability to make a firm diagnosis of chlamydial infection subsequently. If the conjunctivitis has not improved at 36 hours, a presumptive diagnosis of chlamydial ophthalmia should be made, and in the absence of a laboratory diagnostic service treatment with chlortetracycline locally plus erythromycin systemically should be commenced.⁴ Treatment should be continued for three weeks. Investigation and treatment of the parents are mandatory.

G L RIDGWAY
J D ORIEL

University College Hospital,
London WC1E 6AN

¹ Schachter J. *N Engl J Med* 1978;298:540-9.

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Symptomatic treatment of primary pneumatosis coli with metronidazole

SIR,—We were interested to read the report by Mr B W Ellis of the treatment of primary pneumatosis coli with metronidazole (15 March, p 763). As he points out, there is increasing evidence to suggest that gas-producing bacteria are responsible for the maintenance of these gas cysts, and the work of Van der Linden and Hoflin¹ and ourselves² on breath hydrogen excretion in these patients supports this hypothesis. Despite this, bacteriological studies have so far failed to identify any causative organisms and the underlying cause of the initiation of cyst formation remains obscure.

Treatment of symptomatic pneumatosis coli with antibiotics is an attractive prospect since oxygen therapy requires hospitalisation and is somewhat inconvenient for the patient. We also have occasionally observed clinical improvement in patients given antibiotics, one case of which³ is quoted by Mr Ellis, and have therefore been attempting a more systematic appraisal of such treatment. As yet we have no consistent evidence of benefit. Indeed, we have had to abandon a course of metronidazole in one of our patients because of rapid worsening of symptoms.

There are theoretical reasons for caution in the

use of antibiotic therapy in pneumatosis coli. The maintenance or resolution of pneumocysts may be critically dependent on the partial pressure of hydrogen within the colon and this in turn is determined by the balance between hydrogen-producing and hydrogen-catabolising colonic bacteria. Net hydrogen production may be increased or decreased by antibiotic therapy so a variable response of pneumatosis coli to antibiotics may be expected. It is even possible that the association of pneumatosis with chronic pulmonary disease arises as a consequence of the repeated antibiotic therapy these patients receive.

Until more information is available we would urge that antibiotic therapy, including metronidazole, be used with caution in these patients, possibly being restricted to those in whom oxygen therapy has failed or is contraindicated. While we share Mr Ellis's desire for a simple form of therapy, we believe that more experience is needed to establish the effectiveness and safety of antibiotic regimens for pneumatosis coli.

JOHN GILLON
RICHARD F A LOGAN
W SIRCUS

Gastrointestinal Unit,
Western General Hospital,
Edinburgh EH4 2XU

ROBERT C HEADING

Department of Therapeutics,
Royal Infirmary,
Edinburgh EH3 9YW

¹ Van der Linden W, Hoflin F. *Eur Surg Res* 1978;10:225-9.

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Volkman's ischaemic contracture

SIR,—Following your recent leading article on Volkman's ischaemic contracture (16 February, p 430) and the unhappiness about this condition in Toronto to which you refer,¹ it seems worth reporting the experience of supracondylar fractures in the Royal Hospital for Sick Children, Edinburgh, over a period of almost 30 years, during which the condition appears to have been eliminated with respect to this fracture.

In the three years 1947-9 inclusive 71 fractures required surgical intervention. Eight were explored because of suspected arterial damage at presentation. One of these patients developed an ischaemic contracture, but he had been manipulated no less than five times at another hospital before referral to Edinburgh. Open exploration of the newly presenting supracondylar fracture was abandoned following the realisation that the circulation was almost always restored by gentle manipulation of the fracture in order to relieve an entrapped or compressed artery (rather than to coapt the bone fragments). Stabilisation after reduction in as flexed an attitude as is compatible with adequacy of the peripheral circulation and the avoidance, unless absolutely essential, of an encircling plaster of paris cast are most important also. From January 1950 to June 1975 28 out of 486 patients with closed fractures presented with vascular problems. None was explored and none developed an ischaemic contracture. Four other patients developed signs of vascular compromise during their first manipulation, but suspension of the limb to minimise oedema and prevent extrinsic pressure on the artery allowed restoration of the circulation. One patient developed a temporary partial median nerve palsy and autonomic disturbance in the hand, but no contracture. Of the 23 open operations in this period, 21 were for persistent unacceptable deformity after manipulation. The