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Correspondents are urged to write briefly so that readers may be offered as wide a selection of letters as possible. So many are being received that the omission of some is inevitable. Letters should be signed personally by all their authors.

Diet and the diabetic child

SIR,—In the *BMJ* of 11 December (p 1447) a group of distinguished Belgian paediatricians raise matters which prompt a British paediatric response although the original leading article (2 October, p 780) did not. I applaud their stand and will try also to correct a false impression which they may have gained.

Firstly, I support their plea that the practice of good diabetic control be motivated by *faith* in our ability to prevent or slow the development of diabetic angiopathy or at least by the conviction that its efficacy has not yet been disproved. I speak of "faith" because I know not of proof, but it is faith strengthened by circumstantial evidence, some of which they review. It is not surprising that 55 years and thousands of publications after the advent of insulin therapy we remain ignorant of its relationship to the vascular complications of diabetes. In that time patients, and particularly children, have been subjected to new stresses, new foods, new drugs, new pollutants, and new insulins as well as to a behavioural revolution at an unprecedented and gathering pace. Who can define "good control" and who has proof of which patients achieve it and for how long? Physicians themselves (and especially research fellows) come and go frequently in the life of each diabetic. Which medical team is omniscient about individual patients in a population of diabetics which truly represents all sections of the community rather than one skewed toward the privileged or the deprived—and can retain each part of these individual lives in sharp focus and proper perspective at the end of 20-30 years before making its objective judgment on the relationship between control and the health of the individual's microcirculation? Animal experiment and short-term observations of the kind

which the Belgians quote may indeed prove some day to represent adequately the long-term outcome of good or poor control, and to them can be added studies of, for example, basement membrane thickening in relation to treatment. The human fetus (often the best judge of influences noxious to man) and his placenta clearly favour normoglycaemia and find poor diabetic control to a greater or lesser extent intolerable.¹ Paediatricians are wise to prefer faith based on some reasonable evidence to a doctrine of despair on imperfect knowledge so far as diabetic angiopathy is concerned. Their diabetic children have far to travel and may be handicapped by retinopathy or nephropathy or coronary artery disease when their adult responsibilities are greatest.

What price must the patients pay? Certainly not the tightly restricted diet which Dr Dorchy and his colleagues may imagine to hold throughout Britain; not at this large child and adolescent diabetic clinic and not at many others known to me. Our practice aims at permitting a normal life style and it is shared by many others if the large nationwide sales of our "Notes for Parents"² reflects the practice of the country. Belgian colleagues have had it for some years and have commented favourably upon it. A portion of protein, average for what the family can afford, is given at each main meal. Vegetable cooking oil and soft margarine high in polyunsaturates are used even if their benefit to the patient also remains unproved. We teach moderate use of sugar by everyone in the family, including the diabetic, but have relaxed a previous rule of total abstinence. In hospital, at home, and at school the diabetic child participates in a standard meal optimally suited to diabetics and non-diabetics alike. Our book shuns the word "diet" and teaches a

"good food system." The child decides for himself the average daily need of carbohydrate and it is appropriately spaced—more or less being given according to appetite and activity (and weight for height). We oppose a "free diet" because Scottish children have an appalling record of sugar ingestion and of carious teeth—and Scottish men of coronary artery disease. We campaign therefore for the use of health foods, natural fibre, by all the family. We are moving rapidly to a policy of twice-daily insulin, especially from the age of 10 years, and we encourage self-injection even by 5-year-olds. Our urine testing routine is identical with that pertaining in the Belgian clinics.

I have divided young diabetics into three groups—those who co-operate, those who cannot, and those who will not.³ Recommendations like "12-15% [energy intake] from protein, 30-35% from lipid, and 50-55% from carbohydrate" (in an open-ended daily energy intake) may be applicable to intelligent co-operators with some talent for prophecy but not to the "cannots" or to the "will nots." This community has its fair share of both but no monopoly of either. They too must occur wherever there are fully representative diabetic clinics for all the community, but it is true that they will form a smaller proportion if the diabetic life is made as normal as possible, especially in later childhood and adolescence. We have special programmes for the difficult ones but try hard to safeguard the co-operators from being neglected because of them. Such a programme as ours, and it must be commonplace, can be applied world-wide and my experience of the International Diabetes Federation is clearly a happier one when it comes to having a forum in which to express new views. I at least was given complete freedom to express somewhat radical ideas on the adaptation of our "good food system" to reactive adolescents in any land.³ I think it likely that our Belgian paediatric colleagues would find them acceptable. International meetings in Europe may need to see fewer

British physicians in the next year or two but should interpret this in economic terms and not in those of British disinterest. Long live the International Study Group of Diabetes in Children and Adolescents!

JAMES W FARQUHAR

University Department of Child
Life and Health,
Diabetic Clinic,
Royal Hospital for Sick Children,
Edinburgh

¹ Farquhar, J W, in *Clinics in Endocrinology and Metabolism*, No 5, *Aspects of Neonatal Metabolism*, ed J O Forfar. Philadelphia, Saunders, 1976.

² Farquhar, J W, *Notes for the Guidance of Parents of Diabetic Children*, 2nd edn. Edinburgh, Churchill Livingstone, 1975.

³ Farquhar, J W, *International Diabetes Federation Bulletin*, 1976, 21, October, p 6.

Lumbar epidural analgesia in labour

SIR,—Dr I D Hoult and his colleagues (1 January, p 14) focus attention on an important question concerning the safety of epidural block. Does an increased incidence of forceps delivery, and especially with Kielland's forceps, entail an inherent risk to the baby or mother? The unequivocal answer must be yes; interference may always lead to morbidity. However, there is room for substantial qualification.

A comment must be made on the technical difficulty of the use of Kielland's forceps. Dr Hoult and his colleagues discuss only two fetal presentations at delivery, the occipito-anterior and the occipitoposterior positions. Since malposition has not been clearly defined (whether the occiput was posterior or lateral) the apparent high incidence of malposition could be interpreted as a failure to recognise that the head normally engages in the transverse position, and in the presence of epidural analgesia rotation commonly occurs only at the outlet. It seems to us that a high incidence of rotation from the transverse position might be expected if the second stage is curtailed before the head has descended to lower levels. Our own experience suggests that assisted rotation from the transverse position is required more frequently than from the posterior position. Otherwise the implication is that in some way epidural block actively favours posterior rotation of the occiput, which we would strongly contest. The more serious malpresentations of deep transverse arrest (as opposed to transverse position) and the assisted persistent occipitoposterior position should have been separately presented. The implications in terms of fetal and maternal damage of an uncomplicated rotation from a transverse position are very different from the more serious, disproportion-induced malpresentations. (In our experience most instances of malrotation after epidural block can be easily turned). Difficulties with Kielland's forceps arise from cephalopelvic disproportion and are not related to the method of pain relief.¹ The skill of the operator is fundamental in making a decision as to whether forceps delivery can be performed safely.

We have found no evidence of increased perinatal mortality after epidural block. In fact, there may even be a protective effect on the premature infant.² The question of increased morbidity is far more difficult to quantify, although Dr Hoult and his colleagues partly answer that point since none of the 50 babies delivered by Kielland's forceps required admission to their special care baby unit. There

is clearly no substitute for adequate operative experience.

We completely agree that the mother should know beforehand of any likely complications from epidural block. It has been our practice for some years, in the mothercraft class or labour ward, to tell every mother who chooses an epidural block that the incidence of forceps delivery is about 3-4 times more than after other forms of pain relief. (We also mention the headache which can occur after accidental lumbar puncture.) Twenty-five per cent of our patients elect to have epidural block.

Since there are no unequivocally proved medical indications for epidural block other than excellent pain relief (although there are relative advantages) epidural block must be carried out and managed expertly. We believe that with nursing and medical staff trained to standards appropriate for a well-organised obstetric unit there should be no measurable additional risk to the mother or her baby. Contrary to the apparent practice of Dr Hoult and his colleagues we believe that the primary indication for epidural block is the request of the well-informed mother for complete pain relief.

BRYAN HIBBARD J B WEAVER
JAMES F PEARSON G A D REES
SHEILA M WALKER MICHAEL ROSEN

Departments of Obstetrics and
Gynaecology and Anaesthetics,
Welsh National School of Medicine,
Cardiff

¹ Davidson, A C, et al, *British Journal of Obstetrics and Gynaecology*, 1976, 83, 4.

² David, H, and Rosen, M, *Anaesthesia*, 1976, 31, 1054.

Chemotherapy for varicella-zoster infections

SIR,—Your leading article (18 December, p 1466) shows quite clearly the difficulties of treating varicella-zoster, even with the advent of second-generation antiviral agents. It shows that even in life-threatening cases there are no more than anecdotal reports of successful treatment with adenine arabinoside (ara-A). I recall that similar encouraging reports were made about intravenous idoxuridine (IDU) in the treatment of herpes simplex encephalitis² until the Boston controlled trial³ had to be abandoned owing to therapeutic failure and unacceptable side effects. In their book⁴ Juel-Jensen and MacCallum recommend the use of cytosine arabinoside (ara-C) and ara-A for the treatment of severe zoster without reference to clinical trials of these compounds.

One cannot leave unchallenged the conclusions in your final paragraph regarding treatment of "mild" cases of zoster. Most patients are incapacitated and even in the relatively young the symptoms can be protracted and temporarily disabling. Herpes zoster should be treated not only for the pain but also to minimise or prevent scarring and post-hepatic neuralgia. You recommend treatment of "mild" cases of zoster with a 40% solution of IDU in dimethylsulphoxide (DMSO). This product is not manufactured by any pharmaceutical company in the United Kingdom or in the rest of the world to my knowledge. If applied by the technique recommended by Juel-Jensen and MacCallum⁴ it is extremely expensive, may cause inflammatory and even necrotic skin changes, and in practice can be used properly only under careful medical supervision in hospital. In 1974 I reported in your pages⁵ the results of a

randomised double-blind trial assessing 5% IDU in DMSO in 118 patients. I concluded that this solution had as great an effect on zoster as 25% IDU. In 1975 Simpson⁶ compared 5% and 40% IDU in DMSO in 50 cases and showed that both produced equal benefit; 5% IDU in DMSO is available on prescription (Herpid; WB Pharmaceuticals), but 40% IDU in DMSO cannot be recommended for routine domiciliary treatment of mild zoster.⁷

RODNEY DAWBER

Department of Dermatology,
Slade Hospital and
Radcliffe Infirmary,
Oxford

¹ Nolan, D C, et al, *New England Journal of Medicine*, 1970, 282, 10.

² Juel-Jensen, B E, and MacCallum, F O, *Herpes Simplex, Varicella and Zoster*, p 154. London, Heinemann, 1972.

³ Boston International Virus Study Group, *New England Journal of Medicine*, 1975, 292, 599.

⁴ Juel-Jensen, B E, and MacCallum, F O, *Herpes Simplex, Varicella and Zoster*, pp 170, 172. London, Heinemann, 1972.

⁵ Dawber, R, *British Medical Journal*, 1974, 2, 526.

⁶ Simpson, J R, *Practitioner*, 1975, 215, 226.

⁷ Dawber, R, *British Medical Journal*, 1973, 1, 737.

Self-poisoning with drugs

SIR,—After all, the rise and rise of self-poisoning—in the news at least once a year—could be a paper tiger. Just frighten it and watch how it shrinks. Unlike last year's first claim of the monster's weakness,¹ which unfortunately didn't stand up to closer scrutiny,² this time there are impressive figures to support the claim. The good news comes from Sheffield. In recording the familiar increase in the past 20 years, Dr D I R Jones (1 January, p 28) found that "the only thing that had a demonstrably favourable effect on the rate of self-poisoning was the threatened withdrawal of the emergency services." Is it now the time to pay tribute to the junior hospital doctors who, by embarking on industrial action in December 1975, unknowingly set up the natural experiment?

Recognising that his data (table II) showed a seasonal variation Dr Jones states that in December 1975 the number of admissions for self-poisoning were "abnormally low"; from this he concluded that the junior doctors' industrial action was the probable reason for the drop. But such a claim can be justified only if the observed reduction (if any) can be shown to be independent of seasonal variation as well as statistically significant. As the author has included a tabulation of admissions for each month in the past six years an obvious discrepancy between his figures and their interpretation must be pointed out.

Dr Jones claims that the pronounced seasonal variation was "inconstant." A closer look at table II, however, does not bear this out. Even before the crucial year (1975) it can be observed that there was a trend towards high numbers in the summer and low numbers in the winter; spring and autumn were intermediate, the former having a higher average rate than the latter. If one calculates average daily numbers for each season it can be shown that winter has the lowest rank in four of the five years, while summer has the highest rank for the same number of years. A comparison of each observed monthly number of admissions with the expected number over the same period produces significant differences in 12 out of the 60 months in question. Four out of five significant drops concern winter months,