was about two-and-a-half times as large as the corresponding US study from the National Institute of Child Health and Development^{7, 8} and at least as much care was taken to avoid biased assessments as in that study; there were no matched controls in the Canadian study.⁹ It would thus not be surprising if the MRC study proved to be a more sensitive indicator of the possible risks of amniocentesis than either of the other two. The findings, which differ in several respects from those of the US study, certainly deserve more careful consideration than this premature and sweeping dismissal from the United States accorded them. Your own commentary¹⁰ on the MRC report made this clear.

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¹ US National Institutes of Health, *Antenatal Diagnosis*, Publication 79-1973. Bethesda, Maryland, NIH, 1978.

² Medical Research Council, *British Journal of Obstetrics and Gynaecology*, 1978, **85**, suppl 2.

³ Milunsky, A, *Lancet*, 1979, **1**, 546.

⁴ Alexander, D, *et al*, *Lancet*, 1979, **2**, 577.

⁵ Turnbull, A C, and Bantock, H, *Lancet*, 1979, **1**, 547.

⁶ Bantock, H, and Sutherland, I S, *Lancet*. In press.

⁷ NICHD National Registry for Amniocentesis Study Group, *Journal of the American Medical Association*, 1976, **236**, 1471.

⁸ Lowe, C U, *et al*, *The Safety and Accuracy of Mid-trimester Amniocentesis*, DHEW Publication (NIH) 78-190. Bethesda, Maryland, US Department of Health, Education, and Welfare, 1978.

⁹ Simpson, N E, *et al*, *Canadian Medical Association Journal*, 1976, **115**, 739.

¹⁰ *British Medical Journal*, 1978, **2**, 1661.

Induction of labour

SIR,—Your leading article (18 August, p 407) failed to point out that most of the studies on induction of labour approach the problem from different angles. Only three¹⁻³ compared induced and non-induced patients in the same unit over the same period of time who were looked after by the same consultants. Even here there were differences in that Cole *et al*² did not allow their non-induced patients to go beyond 41 weeks. All the induced women of Yudkin and his colleagues³ were reported to have had normal antenatal histories. Both workers used oxytocin regimens different to mine.

For those interested primarily in the fetus, the possibility of continuous monitoring from the start of labour, the ability to see the liquor and the presence of an experienced midwife give the induced fetus many advantages over the baby whose mother goes into spontaneous labour outside hospital.

If, however, one's principal concern is the ability of the uterus to empty itself might it be that this occurs more readily when there is some pathology present? It is striking to note in Yudkin's paper that most of the increased operative interference attributed to induction, including all the caesarean sections, occurred in their preterm patients and that women induced after 40 weeks laboured well and delivered spontaneously. I stressed in my paper that term should be regarded as 40 not 38 weeks and that induction before 40 weeks always demanded a strong indication. In the original version I also suggested that the purpose of oxytocin was to initiate contractions rather than to drive the baby out of the uterus, and that the usual doubling-up technique

could cause excess contractility leading to signs of fetal distress and an increased caesarean section rate.

I would suggest that observance of these two principles led to our 15 500 induced patients having a lower section rate than our 28 000 non-induced cases.

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¹ Clinch, J, *British Journal of Obstetrics and Gynaecology*, 1979, **86**, 340.

² Cole, R A, *et al*, *Lancet*, 1975, **1**, 767.

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Fetal malnutrition—the price of upright posture?

SIR,—I fear that Dr A Briend (22 September, p 734) has not understood any of my letter (25 August, p 492) and is seriously misinformed about its subject. Firstly, I would not suggest "modifying biparietal diameter curves by a fourth-degree polynomial." A much better expression is suggested in our paper.¹ Secondly, fetal head circumference, far from being appropriate for the purpose, is a linear dimension like biparietal diameter and is just as inappropriate an indicator of growth in weight or volume, both parameters showing a decreased velocity towards term at a time when brain weight growth is accelerating.¹ I think I understand the verb "to accelerate." It means to increase velocity, as Dr Briend says; and our findings on the quantitative growth of the human brain² show that just that is happening at the time towards the end of gestation when the linear dimensions of the skull (circumference and diameter) are flattening out. There is no conflict here for anyone who understands lower school arithmetic. It may be that the vagaries of constructing velocity curves, which depend greatly on the intervals selected, have misled Dr Briend, as they did us.¹

Our brain growth data come from fetuses and babies carefully selected for normal fetal growth, as Dr Briend could have discovered for himself by reading our paper.² His suggestion that our findings may be artefactual are therefore neither necessary (since they are not in conflict with the ultrasonographers' longitudinal data) nor just.

We remain astonished and somewhat saddened that so many people still regard a plot of the linear dimensions of a solid body, such as the head, as a true reflection of its growth in volume or weight—and that this has led to importantly mistaken predictions about the catch-up potential of the brain in growth-retarded babies.³ Campbell's data for growth in biparietal diameter and head circumference⁴ are incomparably the best in the world. Dr Briend would do well to consult him about their meaning for growth in brain weight if he is still unable to understand the fundamental distinction.

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¹ Dobbing, J, and Sands, J, *Early Human Development*, 1978, **2**, 81.

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³ Brandt, I, In *Perinatal Medicine*, ed G Rooth and L E Bratteby, p 221. Stockholm, Almqvist and Wiksell, 1976.

⁴ Campbell, S, In *Fetal Physiology and Medicine*, ed R W Beard and P W Nathanielsz, p 271. London, W B Saunders, 1976.

Serum free thyroxine in pregnancy

SIR,—I read with interest the letter by A Margot B Boss and D Kingstone (1 September, p 550) concerning the recalculation of free thyroxine concentration in pregnancy in patients whom we had jointly studied.¹ In the light of this letter, I should like to draw attention to some clinical considerations in the assessment of thyroid function in pregnancy.

All our patients in this study remained clinically euthyroid and had uneventful pregnancies without any clinical evidence of hypothyroidism in mother or neonate. Despite an apparent fall in free thyroxine concentration in the second half of pregnancy,¹⁻² the mean concentration of thyroid-stimulating hormone in late pregnancy has been found to be within the non-pregnant range.³⁻⁴ Owing to increase in thyroid-binding globulin, a diagnosis of thyroid disease in pregnancy based on conventional tests may be quite misleading. The accuracy of the calculated free thyroxine index using the Thyopac-3 kit has been questioned in certain pathophysiological states⁵ and in women using oral contraception.⁶

Until we can eliminate methodological error therefore controversy regarding free thyroxine concentration measurement in pregnancy will continue. Values obtained in early pregnancy may assist in reaching the appropriate diagnosis of thyroid disease in pregnancy.

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¹ Boss, M, Djahanbakhch, O, and Kingstone, D, *British Medical Journal*, 1978, **2**, 1496.

² Kurtz, A, Dwyer, K, and Ekins, R, *British Medical Journal*, 1979, **2**, 551.

³ Braunstein, G D, and Hershman, J M, *Journal of Clinical Endocrinology and Metabolism*, 1976, **42**, 1123.

⁴ Djahanbakhch, O, *et al*, for publication.

⁵ Burr, W A, *et al*, *British Medical Journal*, 1977, **1**, 485.

⁶ Djahanbakhch, O, *et al*, *British Medical Journal*, 1977, **1**, 1413.

Psychosocial stress in pregnancy

SIR,—Dr J Dearlove (8 September, p 613) and Dr M O Roland (22 September, p 738) are right to question the validity of the findings reported by Dr R W Newton and others (18 August, p 411) that premature labour is precipitated by life events.

Those mothers whose babies were born prematurely may indeed have seen their pregnancy as complicated by more life events than those whose babies were born at term in an effort to explain the premature birth. This phenomenon was described by Bartlett in 1932¹ as "effort after meaning" and has been discussed at length by Brown² in relation to life events research. The classic instance of this phenomenon was Stott's report³ that the mothers of children with Down's syndrome reported more "shocks" during pregnancy than those whose babies were normal. The likely spuriousness of this result became apparent when the chromosomal abnormalities associated with Down's syndrome were reported.

It is quite likely that premature labour is precipitated by stresses, but it has not been proved by this study. Those of us who are concerned to demonstrate that psychosocial variables affect health and the course of illness must be sure that our work cannot be criticised on methodological grounds by those dis-