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.

³ Yudkin, P, *et al*, *British Journal of Obstetrics and Gynaecology*, 1979, **86**, 257.

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.

² Dobbing, J, and Sands, J, *Archives of Disease in Childhood*, 1973, **48**, 757.

³ Brandt, I, In *Perinatal Medicine*, ed G Rooth and L E Bratteby, p 221. Stockholm, Almquist 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-

inclined to this view. Perhaps Dr Newton and his colleagues can reanalyse their data excluding all those life events (some of which were listed by Dr Roland) that are open to subjective interpretation.

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¹ Bartlett, F C, *Remembering: A Study of Experimental and Social Psychology*. London, Cambridge University Press, 1932.

² Brown, G W, et al, *Psychological Medicine*, 1973, **3**, 74.

³ Stott, D H, *Journal of Psychosomatic Research*, 1958, **3**, 42.

Samaritans versus suicide

SIR,—Minerva asks (22 September, p 743) "Just how effective are the Samaritans in preventing suicide?"—a question impossible to answer owing to lack of controls. Many of us have personal experience of individual cases where suicide has definitely been averted by our intervention; but these cases cannot be enumerated.

The following data, however, were extracted from the records of a random sample of 57 224 of the 250 740 individuals who sought our help during 1978. A total of 13 421 callers admitted suicidal thoughts or intentions, and 15 763 callers admitted no such thoughts or intentions; in 28 040 cases for various reasons this information was not available. Thus 45.9% of the 29 184 cases in which the topic of suicide was broached (either by the caller or by the Samaritan volunteer) admitted to suicidal ideas; or, to put it another way, 23% of the whole sample were found to be harbouring them. This gives the population a "potential" suicide rate of 23 453 per 100 000, whereas the official figure is only about eight per 100 000. Evidently we do attract those with a suicidal bent, but quite a lot of them think better of it.

Minerva also states that the suicide rate is showing no further decrease despite our increased activity. True. It is levelling out at about eight per 100 000, having dropped steadily from 12 per 100 000 in 1963 (coincidentally the year when Samaritans Incorporated came into action). One of the possible reasons for its not falling further may be that the number of our new callers each year has increased since 1973 by 60%, whereas the number of our volunteers has risen only 17%. Many of our 170 branches are over-stretched; to achieve some sort of parity with this increasing work load we need at least 6000 more volunteers—urgently.

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High- and low-carbohydrate diets

SIR,—Dr K W Heaton of Bristol Royal Infirmary has written to us pointing out that the dietary fibre figures given in our paper concerning the use of high and low carbohydrate diets in maturity onset diabetes (30 June, p 1753) differ from those calculated using the new McCance and Widdowson food tables.¹ The discrepancy has arisen for two reasons.

Firstly, at the time the calculations were made the new tables were not available and we used preliminary figures given in the

Journal of Human Nutrition (Vol 30, Nos 5, October 1976, p 303), which for some foods differed quite considerably from the definitive version. Secondly, our calculations included the fibre content in the skin of soft fruit. The correct figures for the high- and low-carbohydrate diets should be 64 g and 17 g respectively. Clearly the overall conclusions of the study are not affected in any way by this discrepancy.

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¹ Paul, A A, and Southgate, D A T, *McCance and Widdowson's The Composition of Foods*, 4th edn, p 266. London, HMSO, 1978.

Cancer of the ovary

SIR,—We were interested to read your leading article (22 September, p 687) on ovarian cancer but question the therapeutic conclusions, which exclude radiotherapy from consideration.

"Advanced" ovarian cancer is an ill-defined term. Any discussion of treatment must take into consideration the known routes of spread to the para-aortic lymph nodes and diaphragm. The latter should be examined in all cases as metastases may otherwise be missed.¹ Internal mammary node involvement may be detected by lymphoscintigraphy and is probably of importance in the pathogenesis of ascites.² The role of cytoreductive surgery is now well established and it has been found that it is the amount of residual disease that affects prognosis rather than initial extent of the disease.³

What therapies should be used as adjuncts to surgery? For many years radiotherapy was the only treatment available. Its role has now been reinvestigated in a prospective randomised trial at the Princess Margaret Hospital, Toronto.⁴ Some stage I cases probably need no treatment other than surgery if staged adequately with lymphangiography and laparoscopic examination of the diaphragm. A prospective study to test this hypothesis is in progress at the Royal Marsden Hospital; patients are followed closely with repeated laparoscopy to detect recurrence early should it occur.

In stages II and III whole-abdominal radiotherapy has been shown to be effective in eliminating microscopic disease in patients with no visible residual tumour⁴; unfortunately few patients fall into such a favourable group. For those with bulky residual disease throughout the abdomen after maximal surgical resection chemotherapy is appropriate. Such "advanced" cases do indeed require determined treatment with both modalities.

The techniques of radiotherapy need further evaluation. Pelvic irradiation alone is often inadequate because of the high frequency of abdominal relapse.⁴ The whole abdomen may be irradiated either by opposing fields or by a moving strip technique. Supplementary dosage may be directed to the pelvis, para-aortic lymph nodes, and diaphragmatic lymphatics.⁵ The instillation of intraperitoneal colloidal radioactive isotope may have a promising future.⁶ It is of theoretical interest that its absorption follows the route of spread of cells released into the peritoneal cavity.

Future progress will depend on well-organised prospective randomised clinical trials with internationally agreed criteria of

response. Chemotherapy is at an early stage of its development; although response rates of 50-60% are a marked improvement, they have yet to be translated into long-term survival. This is most likely to be achieved by interdisciplinary co-operation in the management of these patients.

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¹ Rubin, P, *Seminars in Oncology*, 1975, **2**, 235.

² Bronskill, M J, Bush, R S, and Ege, G N, *Cancer*, 1977, **40**, 2375.

³ Griffiths, C T, *National Cancer Institute Monographs*, 1975, **42**, 101.

⁴ Dembo, A J, et al, *Cancer Treatment Reports*, 1979, **63**, 249.

⁵ Glatstein, E, Fuks, Z, and Bagshaw, M A, *International Journal of Radiation Oncology, Biology and Physics*, 1977, **2**, 357.

⁶ Buchsbaum, H J, Keetel, W C, and Latourette, H B, *Seminars in Oncology*, 1975, **2**, 247.

SIR,—I consider your leading article on ovarian carcinoma (22 September, p 687) to be a fair review of the epidemiology of the disease. However, I believe every statement in the final two paragraphs is open to question.

The recommendation that at laparotomy for inoperable disease as much tumour as safely possible should be removed may seem reasonable—although no operations are entirely safe, and I would query the use of radical but macroscopic surgical techniques in treating a disease which by definition metastasises and invades initially at a microscopic level. I am not clear how a maximum response to therapy of a disease with such a varied natural history can be assessed—so when (if at all) should a repeat laparotomy be undertaken? If further "adequate" chemotherapy is required to eradicate residual tumour, is this to be established by a third laparotomy, and how long should this cycle continue? Personally (and not surprisingly) I have found in-vitro techniques for assessing tumour response to chemotherapy in vivo to be disappointing.

I deplore the use of survival figures in the final paragraph as a sole measure of prognosis. Enthusiasm is an important motivation to those involved in attempting to overcome such a common and formidable disease. However, I would urge them to remember that clinicians must treat their patients as individuals and not as survival figures, and that there may be occasions when relapse and rapid death may be a merciful release not only from the illness but from radical surgical expertise and determined chemotherapy.

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Soft-tissue sarcomas

SIR,—Your recent leading article (8 September, p 562) emphasises correctly the therapeutic problems in treating soft-tissue sarcomas. The guidelines in soft-tissue sarcoma treatment are highly variable, probably owing to the fact that many of the reports on soft-tissue sarcoma investigation are based on a relatively small number of patients.

A recent study on 153 soft-tissue sarcoma patients,¹ treated in the University Central Hospital, Helsinki, Finland, suggests that both the histopathological diagnosis and especially the grading of the tumour seem to