report of the Home Office working party of which I was a member. You omitted reference to the Institute of Psychosexual Medicine, whose training method was quoted in the consultative document as a possible model for other workers (pp 70, 58, 61).

This method had its origins 25 years ago in a Balint research group under the auspices of the Family Planning Association, from which the institute took over responsibility four years ago. Thus for almost a quarter of a century we have trained primary care doctors in the treatment of sexual and marital difficulties. Training is by in-service group case discussion so that doctors treat their own patients in their own setting from the outset. The practical, empirical therapeutic approach so acquired may be used to treat a variety of psychosomatic problems also, so that its application is much broader than the more recently published behavioural techniques for simple dysfunctions of sexual performance. In addition to employing these skills in their everyday surgery or clinic practice, more than 300 doctors have during these years acquired sufficient expertise to accept referrals from other practitioners and agencies. The institute grants a certificate of competence in psychosexual medicine to such doctors, who are required to satisfy an examining panel that they have reached the required standard by clearly defined criteria.

Some 40 primary care doctors have so far developed sufficient additional skills to become trainers themselves in their own areas of the British Isles (and one in Australia). Since the formation of the institute four years ago some 500 new doctors have come into training, approximately half of whom are general practitioners.

PRUDENCE TUNNADINE
Director of training, Institute
of Psychosexual Medicine

London W1N 1DG

¹ Home Office, *Marriage Matters. A Consultative Document by the Working Party on Marriage Guidance*. London, HMSO, 1979.

SIR,—I have combined motherhood and medicine for the last 20 years and am reminded daily in both roles of the need to comprehend the whole person—it seems impossible to separate the emotions of the individual from the bodily state.

My work in medicine, after the inevitable sequence of odd jobs a part-time medical graduate finds herself doing, has led via family planning practice to an almost full-time involvement in psychosexual medicine. It seems that the presenting symptom of marital disharmony is very often sexual dysfunction. However, what is a psychosomatic manifestation of the marital distress often engages the doctor's attention. The patient expects the doctor to cure the physical symptom and often hopes that help will be found for the sexual and marital problem. He selects the doctor carefully as being a suitable person whom he trusts and with whom rapport may already exist. The doctor feels inadequate or ill equipped or too short of time to respond and may refer the patient. I find that many patients are lost by referral. The doctor appears to have endorsed the difficulty or insolubility of the problem and the patient often feels unable to face this unknown expert and confess. If in fact he does get to the expert his and the referring doctor's expectation of magic are unrealistically high. Sometimes the initial interview with the doctor of the patient's choice has more therapeutic value than the doctor recognises.

It seems to me that the doctor the patient

has selected is the one who should feel able to listen. This may be the patient's general practitioner or the family planning doctor most commonly. These are the doctors who should develop the necessary skills to comprehend the whole patient. If these skills can be acquired in this field only through supervised work then the Balint-style seminar is admirably suited to this shared learning process. There are such seminars in this country under the auspices of the Institute of Psychosexual Medicine, which seeks to set standards in this area of medicine. Money to train the trainers is often hard to come by from local authorities, which can see the need to finance physical medicine but have little to spare for long-term good housekeeping.

The Marriage Guidance Council has an important role to play in this area of distressed relationships but often seems also to feel ill equipped to respond to the physical component of the client's distress, particularly sexual. To delegate this fundamental aspect of doctoring to a voluntary body when it belongs where the patient put it, in the doctor-patient setting, seems to me to be a terribly retrograde step for the medical profession.

Larger government investment is needed for training if marriage really matters (5 May, p 1164), but surely it should be used for the basic training of doctors so that the need for specialists and other experts becomes less. Then the patient will have a chance of keeping mind and body together.

JOAN COOMBS

Leeds, W Yorks

Premature rupture of the membranes

SIR,—Your leading article on premature rupture of the membranes (5 May, p 1165) deals with an important perinatal problem, but we would challenge one point.

The quoted 40% incidence of intrauterine infection 72 hours after membrane rupture seems higher than that generally experienced in Britain. We agree that management should include "judicious induction of labour to strike a balance between the risks of fetal immaturity and those of infection associated with conservative management." However, it is dangerous to equate maturity, which in this context signifies functional maturation of the fetal lungs, with either a particular gestational age or a specified birth weight. We therefore challenge the suggested policy of immediate induction of labour when the membranes have ruptured at 34 or more weeks' gestation, or when the fetus is judged (how?) to weigh at least 2000 g. Your leader rightly states that "we have no reliable means of knowing when the fetus is at risk from infection," but it is now possible to assess fetal pulmonary maturation with considerable accuracy.

Although the incidence of respiratory distress syndrome (RDS) is small after 36 weeks of gestation, there was an overall incidence of 18% in five studies reviewed by one of us (MJW), and Usher *et al*¹ reported that the mortality from RDS in babies of this gestational age was about 6%. Labour will often ensue within a week after membrane rupture at between 34 and 36 weeks, but each additional day in utero at that stage will increase the likelihood that the fetal lungs will be safely mature at birth. If therefore, in the absence of an over-riding maternal indication, delivery prior to 36 (or perhaps even 37) weeks is contemplated, it is essential first to determine

the functional state of the fetal lungs by estimation of the lecithin: sphingomyelin ratio (or other surfactant test) in the amniotic fluid. The sample for testing can usually be obtained by high vaginal aspiration, but transabdominal amniocentesis may occasionally be needed.

C R WHITFIELD
M J WHITTLE

Department of Midwifery,
Queen Mother's Hospital,
Glasgow G3 8SH

¹ Usher, R H, Allen, A C, and McLean, F H, *American Journal of Obstetrics and Gynecology*, 1971, **111**, 826.

Nutrient intake

SIR,—Professor Harry Keen and his co-workers claim (10 March, p 655) that the less people eat the heavier they are, and the more sugar they eat the lower the concentration of their fasting blood glucose. These unexpected conclusions are valid only if they used a reliable and reasonably precise method of assessing both energy intake and sugar intake. Surprisingly, although their paper gives their results in a number of tables and graphs, the methods by which they made their dietary assessments—the basis on which all their calculations and conclusions rest—are insufficiently described in a very few lines.

It appears that subjects were asked to describe their diet for periods of one, two, or three days, and clearly the three-day weight method recommended by the International Biological Programme¹ as the acceptable minimum for assessment of individual food intake was not employed. Other methods of dietary assessment are notoriously inaccurate, and Acheson (1975) showed that even among subjects who were concurrently weighing their food the 24-hour recall underestimated energy intake by some 16%. Much of the error is attributable to omission of snack foods and drinks, many of which contain sugar, and the degree of inaccuracy will vary both with the food pattern and with the composition of the diet.

It may be that the authors managed, despite these sources of error, to achieve a degree of precision and reliability that other workers have not been able to achieve. If so, it is reasonable to ask why they have not demonstrated this by describing how they have validated their method of dietary intake.

PAMELA MUMFORD
ELIZABETH EVANS

Department of Nutrition,
Queen Elizabeth College,
London W8 7AH

¹ International Biological Programme, *Handbook No 9*. Oxford, Blackwell Scientific Publications, 1969.

² Acheson, K J, *British Antarctic Survey Bulletin*, 1975, **41-2**, 87.

SIR,—In their paper on nutrient intake, adiposity, and diabetes Professor Harry Keen and others (10 March, p 655) interpret the highly statistically significant but small negative correlation between total energy intake and body mass index (BMI) as one between energy intake and adiposity. However, this may be an artefact of using BMI as a measure of adiposity.

If, as is suggested by some authors,¹⁻³ there is a negative correlation between BMI and height, the taller subjects in the study would tend to be towards the left of the published scatter diagram (smaller BMI) and the shorter towards the right, leading to the alternative

interpretation of a positive correlation between energy intake and height. Since height is a measure of the size of the subject, this interpretation is not implausible. In order to rule it out, it would be necessary to show that in the large sample studied there is no significant correlation between BMI and height.

E S COOPER

Department of Child Health,
University of the West Indies,
Kingston, Jamaica

J M GURNEY
P J JUTSUM

Caribbean Food and Nutrition Institute,
University of the West Indies,
Kingston, Jamaica

¹ Womersley, J, Durnin, J V G A, *British Journal of Nutrition*, 1977, **38**, 271.

² Benn, R T, *British Journal of Preventive and Social Medicine*, 1971, **25**, 42.

³ Newens, E M, and Goldstein, M, *British Journal of Preventive and Social Medicine*, 1972, **26**, 33.

* *We sent copies of these letters to the authors, whose reply is printed below.—ED, BMJ.

SIR,—Miss Pamela Mumford and Dr Elizabeth Evans are right in pointing to the relative imprecision of the methods we used for collecting dietary information from the 3500 subjects in our studies. The difficulties and inaccuracies inherent in all methods of collecting such data probably account for the absence of relationships between food intake, body weight, and metabolic characteristics in some studies. However, even using these imprecise methods we *did* find relationships, the key ones in our argument being statistically most unlikely to have arisen by chance. The question, then, is whether the imprecision of the methods we used might have spuriously created the correlations we found or affected their order of magnitude. We could only consider both these aspects briefly in our condensed report.

However inaccurate a method of measurement may be, so long as those errors in estimate (be they erroneous overestimates, underestimates, or both) are randomly distributed, they will not, other than by chance, introduce a systematic trend and so generate artefactual correlations. There is no reason for supposing that the method used for the assessment of food intake systematically underestimated for the heavier or overestimated for the lighter subjects. We accept that obese people sometimes fail to recall the whole of their food consumption but so do non-obese subjects^{1 2}; lapse of memory is not necessarily proof of self-deception. A few obese subjects may consciously mislead their inquisitors but a "deception factor" is hardly likely to operate throughout the leanness-adiposity spectrum; the inverse relationship which we reported extended right across the range of BMI, not just between the obese and the remainder. While we think it very unlikely that the method of data collection actually created the relationship we agree that its imprecision may well have led to an underestimate of its true strength.

Other, apparently more accurate, methods of dietary evaluation might increase the value of the correlations we found; but they are not without their own quite serious problems of application. In theory, weighing and recording every item of food consumed is ideal but, in practice, it is so tedious, messy, socially embarrassing, and intellectually demanding that many subjects alter their eating habits or

make guesses to ease the task. Many "drop out" or decline to participate, introducing biases in the population sampled, particularly at the lower end of the social scale³; whereas our method is acceptable to most. We have been able to compare our assessment of food intake with actual food consumption in a sample of subjects and have found a high degree of correlation. Repeated estimates weeks or months apart on other samples of our populations show very comparable mean intakes and significant preservation of rank order within the groups. We shall be describing our methods and these validating studies elsewhere in detail.

Finally, we would enter a mild protest at the slightly distorted way in which our conclusions have been put. We did not really "claim that the less people eat the heavier they are," a statement which might be thought to carry cause-and-effect implications that we certainly did not draw. We recorded data which suggested that the amount of food people ate was inversely related to their degree of adiposity, leaving the interpretation of this correlation open, though we made a few suggestions.

The comments of Drs Cooper, Gurney, and Jutsum are well founded. Keys *et al*¹ noted that, of all the indices of obesity, the BMI was least correlated with height, but some residual relationship remained. We did find a small but statistically highly significant negative correlation between height and BMI in our populations and consequently there was a positive correlation between caloric intake and height, although this only achieved significance in the Beecham and Whitehall men.

Multiple regression analysis, however, showed that there was no significant independent relationship between caloric intake and height when the BMI was introduced into the regression equation other than in the Whitehall men, while the BMI remained significantly negatively correlated with food intake independently of height in all population groups. Thus part of the inverse correlations we reported may be due to the confounding variable of height, but even after making statistical allowance for this the effect remains.

H KEEN
BRIONY THOMAS

Unit for Metabolic Medicine,
Guy's Hospital Medical School,
London SE1 9RT

¹ Campbell, V A, and Dodds, M L, *Journal of the American Dietetic Association*, 1967, **51**, 29.

² Trulsson, M F, *Journal of the American Dietetic Association*, 1954, **30**, 991.

³ Thomson, A M, *British Journal of Nutrition*, 1958, **12**, 446.

⁴ Keys, A, *et al*, *Journal of Chronic Diseases*, 1972, **25**, 329.

Doctors and children's teeth

SIR,—Your recent leading article (12 May, p 1231) on children's teeth made many good points but unfortunately omitted to mention any means of ingesting fluoride other than in water. There are still many unenlightened parts of the country where this safe and simple procedure has not been instituted and the main alternative should be tablets; these ought to be stocked by all child health clinics and prescribed from infancy onwards in non-fluoridated areas. An earlier leading article¹ recommended a dose of 1 mg daily for children over 2 years, and advised the user to suck the tablet slowly rather than swallowing it to give a topical as well as a systemic effect.

Unfortunately drops for infants are not readily available and the tablets need to be crushed and dissolved in water—a task which will limit their use to the most dedicated of parents. What about a new preparation: Fluorabidec?

TONY WATERSTON

Department of Child Health,
Ninewells Hospital,
Dundee DD2 1UB

¹ *British Medical Journal*, 1975, **1**, 535.

Trauma and Paget's disease of bone

SIR,—Further to Dr L R Solomon's article (7 April, p 931) and Dr T M Gasper's letter (5 May, p 1217), it might be appropriate to mention that in the French medical literature several cases of localised^{1 2} and even generalised^{3 4} Paget's disease of the bone have been reported to develop after trauma.

In the cases described the latent period between trauma and diagnosis varies considerably.⁵ The relative frequency of involvement of the different bones—and particularly the fact that the right femur is twice as frequently affected as the left one—points to the possibility that trauma is at least a factor in the development of Paget's disease.⁶ Trauma may precipitate a breakdown in the normal mechanism of gradual continuous replacement to which bone is constantly subjected and result in the development of Paget's disease.⁶ The relative immunity of the bones of the foot and the mandible is difficult to explain.

Trauma, however, cannot be solely responsible for Paget's disease, as the microscopic ultrastructure, and especially the discovery of intranuclear inclusion bodies in the osteoclasts of bone specimens affected by Paget's disease and not in other normal or pathological bone specimens, provides indirect evidence that the condition may be due to a slow viral infection.^{7 8} This hypothesis would also explain the geographical distribution of the disease and its tendency to run in families; but the virus has not yet been identified.

At present the aetiology of Paget's disease seems to be multifactorial: it is probably due to a slow virus infection, with trauma acting—in some cases—as a localising factor and heredity as a predisposing factor.

RONNIE HAMDY

Department of Clinical Gerontology,
St John's Hospital,
London SW11 1SP

¹ Lievre, J A, *Journal Belge de Rhumatologie et de Médecine Physique-Acta Medica Belgica*, 1974, **29**, 322.

² Lievre, J A, *La Presse Médicale*, 1936, **3**, 45.

³ Louyot, P, and Gaucher, A, *Revue Médicale de Nancy*, 1961, **582**.

⁴ Froment, J, Bonnet, P, and Brun, J, *Lyon Médical*, 1939, **33**, 169.

⁵ Raulis, A, *Journal Belge de Rhumatologie et de Médecine Physique-Acta Medica Belgica*, 1974, **29**, 367.

⁶ Jaffe, H L, *Clinical Orthopaedics and Related Research*, 1977, **127**, 4.

⁷ Mills, B, and Singer, F, *Science*, 1976, **194**, 201.

⁸ Rebel, A, *et al*, *Calcified Tissue Research*, 1976, **20**, 187.

Differences between Leeds fractures and London fractures?

SIR,—We were surprised at the assertion of Dr M R Baker and others that their control data on plasma 25-hydroxy vitamin D (25-OHD) concentration are almost normally distributed (5 May, p 1218), and this led us to calculate the coefficients of skewness ($\sqrt{\beta_1/S}$) for their series. The coefficients were 1.9