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Fourth national morbidity study

Sir,

We were interested to see the editorial by Professor Ebrahim (June *Journal,* p.283) on the fourth national morbidity study in general practice.[1] He is, of course, correct in saying that 'this is a study of major importance', but we would like to take issue with his remarks on the statistical approach employed in the study and the utility of its findings.

First, in 1995 it is not 'an innovation' to use multivariate analysis to disentangle the effects of several different variables. Secondly, the particular mathematical model used in the study (relying on whether or not someone consulted once or more during the year), although probably of considerable interest to epidemiologists, is of little interest to general practice where the concern is workload as measured by the number of consultations. Counts such as these can be adequately modelled with an additional twist of innovation using readily available software (for example, generalized linear interactive modelling *GLIM*). Thirdly, the particular form of the model used (logistic single-level multiple regression) is inappropriate in this kind of situation where there is good reason to believe that there are substantial practice effects.[2] The variation between practices needs to be explicitly modelled in a multilevel framework[3] which is now becoming standard statistical practice:[4] it is quite inappropriate to include a supply factor (for example, practice staff per 10 000 population) on the same level as whether or not someone is divorced or widowed.

These are not just statisticians' quibbles: they make a difference. For example, Ebrahim cites the finding that ethnic minorities have higher rates of consultation. First, this cannot be concluded from the analysis which only purports to show that those from the Indian subcontinent and 'other' are more likely than whites to consult once or more during the year — not at all the same thing. Secondly, a proper analysis of counts within a multilevel framework sometimes generates diametrically opposite conclusions to those made in the study.[1] For example, among ethnic minorities, the largest odds ratio reported in the study is for female children;[1] in our analysis this variable is not significant.[5]

These data are important and may well be used, as Ebrahim suggests, by health service purchasers: all the more reason that the analysis addresses the appropriate issues and uses the correct statistical approach.

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Immunization: precautions and contraindications

Sir,

I refer to the review of the second edition of my book *Immunization: precautions and contraindications*, published by Blackwell Scientific Publications (April *Journal,* p.222). The reviewer posed three questions and compared the answers found in this book with those in the Department of Health's *Immmunization against infectious disease.*

The first question referred to whether general practitioners should give pneumococcal vaccine to elderly diabetic patients. The reviewer found that both books recommend this but neither is 'encouraging'. Of course, neither book is written specifically about diabetes and there are other more important aspects of pneumococcal immunization beyond singling out one condition. For example, how should the general practitioner deal with an asplenic patient with regard to pneumococcal immunization? What other vaccines and what other precautions are recommended for these patients? The reader will find extensive advice in my book.

The second question referred to whether general practitioners should give hepatitis A vaccine to individuals who have the human immunodeficiency virus (HIV). The reviewer found that 'neither book gives easily accessible help'. In my book I deal specifically with HIV positive patients, whether symptomatic or not, and recommend that they could be given all vaccines except those against tuberculosis, yellow fever and the oral typhoid vaccine.

The third question posed the dilemma: does a businessman travelling to Japan for one week in July need Japanese B encephalitis vaccine? The reviewer found the answer to be no according to the Department of Health's book, which recommends this only for travellers staying for over one month in rural areas, and yes in my book. In fact, there is no clear cut answer to this question and the recommendation of the Department of Health is based on statistical chance of infection. In my book, I give the general practitioner or the nurse advice that they can use when discussing the question with the patient, so that the patient can make an informed decision. My recommendation is based on international experience. I recommend immunization for travellers to endemic areas of south-east Asia and the Far East if the traveller: will be there during the summer monsoon months; will visit a rural area; will stay for over one month, irrespective of rural or urban location; or is a frequent visitor to cities surrounded by endemic areas.

I believe that reference books should give the general practitioner and the nurse advice that can help the patient make an informed decision. No matter how carefully we formulate our advice, there will still be cases where we fail. Take the recent case of a previously healthy Swedish woman, aged 60 years, who visited Bali for 10 days.[1] She stayed at a hotel by the coast and made only one day trip to the countryside. She could recall no mosquito bites during the stay. One day after her return to Sweden, she was admitted to hospital with Japanese B encephalitis.

May I suggest a question that is nearer to general practitioners' daily practice for which they will need advice: what to

advise a mother who has a baby aged two months and wishes to discuss pertussis immunization if both she and her daughter aged five years suffer from idiopathic epilepsy?

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Reference

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Urinary tract infection in children

Sir,

In their letter, Kinmonth and Smellie state that further studies are required to determine the proportion of children with urinary tract infection at risk of renal damage (April *Journal*, p.219). I recently reviewed all cases of bacteriologically proven urinary tract infection in children aged under seven years on my personal list of approximately 2500 patients, occurring between 1989 and 1994. There were 16 cases (12 females and four males), with ages ranging from four weeks to six years. All were investigated by urinary tract imaging through direct access to the Imaging Department of the Luton and Dunstable Hospital. Abnormalities were found in 50% of the children (six females and two males). Vesico-ureteric reflux was found in two (12.5%), ureteric and/or pelvicalyceal dilatation in four (25%) and scarring in six (37.5%). All were managed conservatively, and in none was paediatric referral considered necessary. Management included prophylaxis with antibiotics (the duration of which depended on the nature of the abnormality), regular culture of the urine, and repeat imaging as appropriate to detect renal growth, new scars, and the persistence of cessation of reflux. The aim of management is to prevent avoidable renal damage and renal failure.

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Complementary medicine — a definition

Sir,

Medical professionals seem to talk more and more about complementary medicine these days, for example as in Paterson and Peacock's recent paper (May *Journal*, p.255), but what is really meant by the term? The abundance of publications on the subject sharply contrasts with the lack of a valid definition; this would account for the excessively large number of therapies (more than 100) falling under the umbrella term and for the fact that complementary medicine is more than just treatment. Exclusive negative definitions are prevalent, for example 'diagnosis, therapy and preventive procedures excluded from mainstream medicine'[1] or 'a system of health care which lies for the most part outside the mainstream of conventional medicine'.[2] However, an inclusive, positive approach, not defining what complementary medicine does not represent but what it actually means, would clearly be more constructive.

During a series of six staff meetings we (seven complementary health care professionals with medical and non-medical backgrounds) have attempted to define complementary medicine in this way. There were long, sometimes fascinating, discussions in which the four non-medical professionals (a herbalist, an acupuncturist, a chemist and a clinical psychologist) often deviated from the views of the medical professionals.

In spite of these difficulties, a consensus definition was finally found: complementary medicine is diagnosis, treatment and/or prevention which complements mainstream medicine by contributing to a common whole, by satisfying a demand not met by orthodoxy or by diversifying the conceptual frameworks of medicine.

We hope that this presents a step forward and will gradually phase out nonsensical definitions of complementary medicine[3,4] that so often obstruct our thinking.

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Teenage sexual health

Sir,

In defence of their belief that contraception provision and sex education at school do not increase teenage sexual activity, Jacobson and Wilkinson state 'there are several studies and reviews... which strongly suggest that no such increase occurs' (letter, May *Journal*, p.271). However, they cite only one such actual study,[1] from 1985, and fail to mention that it was commissioned by the Alan Guttmacher Institute, a wing of the International Planned Parenthood Federation which is heavily involved financially in the marketing and provision of contraceptives.

A recent review indicates that simply making contraceptives more readily available is associated with an increase in sexual activity in those aged under 16 years, and that sensitive abstinence education programmes are associated with a decrease in sexual activity.[2]

Jacobson and Wilkinson also refer to a conference which reported Dutch teenage pregnancy rates seven times lower than in the United Kingdom for all teenagers and 11 times lower for those aged under 16 years.[3] Their claim that these rates can only be attributable to 'successful contraception provision and sex education' is untenable. If that is so, why are the differential rates better for younger teenagers? Are we to believe that a 13-year-old is more proficient than an 18-year-old at putting on a condom or remembering to take a pill? I also attended the conference at which these data were presented, and the speaker pointed out that the greater differential rates for younger teenagers necessitated an explanation other than just contraception and sex education provision.

Finally, Jacobson and Wilkinson imply that those who disagree with their view automatically tend to be judgemental with sexually active teenagers. Well they may