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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