Sir,
We welcome the comments from Morgan and Mant on our paper on the use of warfarin in atrial fibrillation (March *Journal*, p.153). We would like to address the points they raise.

The calculations for the number of patients who would need to be treated to prevent one stroke were derived initially from the paper by Cairns[1] and were also confirmed by Nolan and Bloomfield.[2] We checked these data and they were correct. We think that Morgan and Mant have produced calculations derived from the data in the meta-analysis but our data do not represent an erratum.

We accept that on Table 1 the 'relative risk of warfarin (%)' is better expressed as 'risk reduction with warfarin (%)'.

We regret that in Table 2, data for two of the studies were inadvertently transposed.

The Landefield paper referenced in Table 3 contains the correct data but these are associated with a different publication.[3]

We suggest in the paper that it may not be safe to extrapolate from the small residual group of patients who are in the trials to the mass of patients in the care of general practitioners in the United Kingdom. The challenge is not so much the quality of management as to know when physical, psychological and social factors justify stopping or starting warfarin. We were surprised that Morgan and Mant should raise the issue of specialist liaison nurses for whom they offer no evidence. Such nurses are unlikely to be able to balance the risks of starting or stopping warfarin better than clinical generalists.

There were two main points raised in our review article. The first was the discovery that high exclusion rates (up to 97%) of eligible patients in the main trials raises serious scientific questions about the generalizability of the findings. Secondly, the complication rates have not yet been satisfactorily evaluated in terms of patient safety and the impact on primary health care teams.

We would welcome further comments from practising general practitioners.

KIERAN SWEENEY

DENIS J PEREIRA GRAY

RUSSELL STEELE

PHILIP EVANS

34 Denmark Road
Exeter EX1 1SF

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Acute myocardial infarction

Sir,
I am rightly reproved by Michael Moher (letter, August *Journal*, p.444) for not mentioning the importance of aspirin in acute myocardial infarction in my editorial on the general practitioner's role in the early management of acute myocardial infarction (April *Journal*, p.171).

Aspirin is an antiplatelet drug that helps to prevent a coronary thrombus from extending or from reforming after it has been lysed. Because aspirin has a different mode of action from that of thrombolytic drugs, its benefits are additional to those of thrombolytic therapy, which was the main topic of my article. Aspirin also differs from thrombolytics in that the timing of administration is not so critical. From knowledge of its action, we would not expect the benefit of aspirin to be much increased by earlier administration, and this seems to be the case. Aspirin should certainly be given to the patient with suspected acute myocardial infarction, but when it is given is not critically important.

By contrast, the timing of thrombolytic therapy is of the utmost importance; restoring coronary flow within an hour or two of occlusion results in myocardial salvage, smaller infarcts, and substantial immediate and long-term benefits. The additional benefit of giving thrombolytic therapy before admission to hospital may exceed the absolute benefit of giving thrombolytic therapy in hospital four to five hours after onset, or of giving aspirin at any time. In terms of the potential number of lives saved, giving thrombolytic therapy is as urgent as the treatment of cardiac arrest; ideally, either treatment should be carried out by the first qualified person on the scene.

The general practitioner called to see a patient soon after acute myocardial infarction has the chance to reverse the underlying pathophysiology with thrombolytic therapy and aspirin, a golden opportunity indeed.

JOHN RAWLES

Medical Assessment Research Unit
University of Aberdeen
Medical School
Foresterhill
Aberdeen AB9 2ZD

Management of angina

Sir,
We thank McKinley and Khunti (letter, June *Journal*, p.328) for their interest in our paper,[1] but fear that they have misunderstood our main findings. We are puzzled that they reiterate the basics of exercise testing and its predictive value even though our paper categorically states that exercise test results from tertiary centre patients cannot be extrapolated to angina patients in the general population, and explicitly acknowledges the lack of data for community-based angina patients.

The central finding of our study was that the vast majority of patients presenting with angina are reportedly not referred for a specialist cardiac assessment. Observational data from prospective studies show that at least 10% of new angina patients experience a myocardial infarction or die as a result of coronary heart disease within one year of presentation,[2] and that the risk of angina patients developing unstable angina or myocardial infarction or of dying within two years of presentation is threefold higher than that of the general population.[3] We have further demonstrated that about 30% of patients with typical angina have marked ischaemia ($\geqslant$3 mm ST segment depression) at a low workload on exercise testing at the time of initial presentation to their general practitioner, and a similar proportion have coronary artery disease for which revascularization with coronary angioplasty or coronary bypass grafting is considered appropriate by consultant cardiologists.[2]

Thus, while we are pleased that McKinley and Khunti repeat our concluding remarks underlining the urgent need for randomized clinical trials to optimize selection of community-based angina patients for coronary revascularization, we believe that until such evidence is available, a policy of 'no action' by general practitioners would be a retrograde step. It is difficult to imagine how the short-term prognosis of new angina patients in the community can be improved without increasing the rate of specialist cardiology referral.

MANISH M GANDHI

Department of Cardiology
The Middlesex Hospital
Mortimer Street
London W1N 8AA

DAVID A WOOD

Department of Clinical Epidemiology
National Heart and Lung Institute
Dovehouse Street
London SW3 6LY

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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.

ROY CARR-HILL

NIGEL RICE

Centre for Health Economics
University of York
Heslington
York YO1 5DD

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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