searched several electronic databases for published studies of the use of guidelines in primary care. Only studies describing clinical care by family doctors that produced significant improvement in patient outcomes in conditions that are normally treated by family doctors were examined. We included only studies that were methodologically sound according to the criteria of the Canadian task force on periodic health examination.[1]

We originally found 91 studies in our search but, after applying our criteria, only four studies remained. Although all of these studies had produced statistically significant changes in patient outcomes, the magnitude of the changes was small and the studies were not long term.

In short, we concluded, as do Conroy and Shannon, that more research needs to be done into the dissemination and implementation of clinical guidelines in family medicine; unless this occurs, the effect of clinical guidelines on our work will continue to be minimal.

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Aspirin and acute myocardial infarction

Sir,

As authors of one of the papers[1] referenced in the editorial by Deeks and colleagues on the use of aspirin in acute myocardial infarction (August *Journal*, p.395) we should like to comment on some of the points made.

First, we agree entirely with the authors on the need to emphasize the long-term role of aspirin in the management of acute myocardial infarction rather than only its immediate use.

Secondly, while accepting that the evidence demonstrates that early administration of aspirin confers no additional benefit in terms of survival,[2,3] we would agree with the authors that 'there is no reason to introduce any delay in its administration, given it relative safety and the ease of administration'.

The main purpose of our study[1] was to test whether or not one of the guidelines issued by the British Heart Foundation in 1989 (namely that 'effective anti-platelet treatment... should be used and could be started outside hospital')[4] was being followed two years later. Because we found relatively little adherence to this guideline we would strongly support Deeks and colleagues' advice that guidelines should 'reflect the correct interpretation of the research evidence' and should be 'actively disseminated'. But most of all we need to see guidelines that are widely implemented.

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Fourth national morbidity study

Sir,

We were surprised by Carr-Hill and Rice's letter (September *Journal*, p.505) regarding Professor Ebrahim's editorial (June *Journal*, p. 283) on the fourth national morbidity study.[1] The primary aim of the study was to describe morbidity, not general practitioner workload, and that is why we chose to analyse, for example, the proportion of patients who consulted for serious illness during a one-year period rather than the mean number of consultations per person, as Carr-Hill did.[2] We would argue that the number of sick patients is highly relevant to general practitioners as a measure of need. Given that we and Carr-Hill were analysing different things it is hardly surprising that we obtained somewhat different results.

Carr-Hill and Rice claim that we used single level modelling: this is incorrect. We agree entirely with Carr-Hill and Rice that practice effects need to be taken into account, and our model was in fact a multilevel model with practice as one of the levels. The main difference in our approach was that we treated practices as fixed effects and Carr-Hill treated them as random effects,[2] that is, practices were assumed to have been drawn as a random sample from all practices. We regarded the fixed effects approach as more appropriate since in the study the practices were not sampled randomly but volunteered and then were selected to participate. Although only main effects for practices were fitted for reasons of simplicity, interactions with sex and broad age groups (0–15 years, 16–44 years, 45–64 years and 65+ years) were allowed for by fitting separate models to age–sex subgroups. The small area estimates based on our fitted model provide a basis for external validation of our results. Results show that our estimates of serious illness rates among men aged 16–44 years are highly correlated across local authority areas (r = 0.81) with mortality rates for males aged 15–64 years, and this provides further justification for our approach.

Carr-Hill and Rice claim that we included a supply factor (for example, practice staff per 10 000) in our analyses. This is inaccurate — any supply effects would be subsumed in the practice effects terms in our model, and we were not interested in why practices were different. We did test whether practice effects made a difference to the model estimates, and found that they did. In conclusion, we believe that we analysed the data appropriately and have drawn the correct conclusions.

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