receiving regular prescriptions for digoxin, amiodarone, flecainide, verapamil or procainamide; patients with 'atrial' in their notes summary; patients with 'transient ischaemic attack' in their notes summary; and patients receiving regular prescriptions for warfarin or phenindione (Dindevan®, Goldshield).

From these data it was possible to define a group of patients with probable non-rheumatic atrial fibrillation and to determine their anticoagulation status. The groups with atrial fibrillation who were not receiving anticoagulation were further divided into three therapeutic groups as defined by Sweeney and colleagues:[1] patients aged less than 60 years with no complicating factors; patients aged less than 75 years without hypertension, congestive heart failure, thromboembolism or diabetes; and patients not in the two previous groups (the at risk group).

Each practice partner was given a list of patients in the at risk group and asked to decide, with the aid of patients' computer records, whether or not the patient was still in atrial fibrillation and if so whether to include or exclude the patient from being offered anticoagulation. Suggested reasons for exclusion included unsteady gait, tendency to falls, dementia, poor compliance, poor eyesight, excess alcohol consumption and active peptic ulceration (including maintenance on acid suppressing drugs).

Results revealed 224 patients with non-rheumatic atrial fibrillation (16 cases per 1000 patients) with an age range of 32–92 years and a mean age of 76.6 years. Of these 224 patients, 75 (33%) were receiving anticoagulation, with individual general practitioner rates ranging from 23% to 53%. Of the 149 patients not anticoagulated, six were aged less than 60 years with no complicating factors, 26 were aged less than 75 years with no additional risk factors and 117 were in the at risk group. Overall, practice partners included 59 of the 117 patients (50%) for consideration for anticoagulation, with inclusion rates for individual partners ranging from 17% to 86%.

This preliminary study indicates that with computerized records an audit of at risk patients is possible but this is time consuming (the audit took up to 20 hours of doctor time). The study produced a group of over 100 patients considered to be at risk and yet individual practitioner assessments as to which patients should be offered anticoagulation varied greatly despite having received the same suggested exclusion criteria. The resulting group of 59 eligible at risk patients would not all be expected to take up the offer of anticoagulation but even if half were to do so this would represent a substantial workload in terms of surgery time and practice nurse time.

Despite the commendable effort of Sweeney and colleagues to define an at risk group of patients, the variation in individual practitioner's decision making may still prove to be an insurmountable hurdle in the effort to deliver care to a group in need.

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Urban community hospitals

Sir,

In their letter (June *Journal*, p.326) Hamilton and Round came to conclusions about the potential usage of an urban community hospital which do not appear to have been supported by the results of their study. Having found that 53% of the local general practitioners said that they would probably use such a hospital (82% for respite care, 75% for social reasons, 79% for elderly acute medical patients, 77% for observation, assessment and simple investigation, 56% for early hospital discharges following surgery, 59% for terminal care, and 56% for early hospital medical discharges), the authors conclude that 'an urban community hospital would provide services not now available, rather than being an alternative to district general hospital admission'.

A different conclusion from their study could have been that about half of the local doctors said that they would use an urban community hospital for a wide range of patients, many of whom currently occupy beds inappropriately in the local district general hospital.

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Sir,

Cook agrees with the conclusion of our letter (June *Journal*, p.326) that an urban community hospital would generate a reduction in work for the local district general hospital. Where we do not agree is in the magnitude of the effect. We believe it would be small.

Admissions to community hospitals come from three main sources: early district general hospital discharges, direct admissions which would otherwise have gone to the district general hospital, or patients who would never have been sent to hospital at all. Our study shows general practitioner preference for this last group (respite care and social admissions), and least interest in the first group.

The rate of 'inappropriate' admissions to a district general hospital, usually quoted as around 15%, has always been hospital-defined, and is approximately halved when general practitioners' opinions are sought.[1] Such a false positive rate for emergency hospital admission of under 10% is excellent, especially when one considers the potential consequences of failing to admit a patient to hospital when admission is needed. The myth that there is a large pool of inappropriate admissions that can be redirected to a community hospital does not stand up to examination.

Nonetheless, an urban community hospital may still have a role — it was supported by 49% of general practitioners, in some cases strongly. It cannot be seen, however, as a quick-fix solution for rising medical admissions to district general hospitals.

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Clinical guidelines

Sir,

The paper by Conroy and Shannon (July *Journal*, p.371) was a masterly review of the pitfalls that await the implementation of clinical guidelines.

We recently conducted a critical review of clinical guidelines, assessing the evidence to see whether guidelines have had any statistically significant effect on the outcomes of patient conditions in primary care, as opposed to merely changing the process by which family doctors deliver care to their patients (Worrall G, Chaulk P. Hope or experience? A critical appraisal of the effects of clinical guidelines on patient outcomes in primary care, *J Fam Pract* 1995; under review). We

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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