

evidence or the process for inclusion or exclusion of studies? What was the level of evidence on which final recommendations were based?

Importantly, high quality guideline development processes require a “balance of healthcare disciplines in the guideline development group”.⁴ Getting the right “who” is a prerequisite for getting the “how” right. Edmonds et al state that membership was arbitrary, with predominant representation from rheumatologists and relevant pharmaceutical companies. Given the problems associated with physician–industry interactions,⁵ it has been suggested that authors with significant conflicts of interest should be excluded from participating in guideline development.⁶ The rationale for arbitrary selection of members and inclusion of members from the pharmaceutical industry is not explicitly stated.

These issues may have contributed to the difficulties the group experienced, and may detract from the validity of their recommendations. Future trips down the “road to consensus” should run more smoothly after careful consideration of the “what”, “how” and “who” at the outset — no “ifs and buts” about it.

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5. Wazana A. Physicians and the pharmaceutical industry. Is a gift ever just a gift? *JAMA* 2000; 283: 373-380.
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IN REPLY: Both Vitry and Hurley and Gazarian and Kaye would have had our consensus group address different or broader issues than safe prescribing and use of COX-2-specific inhibitors (CSIs). Indications for use, leakage and cost effectiveness are important issues, but our goal, clearly stated in our article,¹ was different and, we believe, important: if a clinician has

decided to use a CSI, what considerations are needed to prescribe the drug safely? Disagreements in reaching consensus were not, as suggested by Gazarian and Kaye, due to confusion about the aim of the exercise, but to differences in interpreting evidence and expressing conclusions in simple and direct terms. It would have been easy to avoid these problems by limiting participants to a small group of like-minded colleagues, but we chose to involve a broad range of people who may represent a more realistic spectrum of attitudes and approaches.

We find Vitry and Hurley gratuitously pejorative in their description of the participants in this exercise. With the exception of two rheumatologists with epidemiological expertise (who did not sign off on the position statement²), all the rheumatologists involved were members of one or both advisory boards. They were a relevant group precisely because this role should involve a responsibility to provide sound advice to the industry paying for it, and equally to the profession, both in the interests of good patient care. “Current financial links” is not the way such a consultancy is usually described. They call the exercise “at best a tight collaboration between some healthcare professionals and drug companies” and “at worst ... as the ‘happy end’ of a successful marketing campaign”. Given that one of the two pharmaceutical companies involved declined to sign off on the statement, as did two rheumatologists who were advisory board members for the other company, this is a curious outcome of “tight collaboration”.

With respect to the relative safety of selective versus non-selective COX inhibitors, our considerations were based on data available from peer-reviewed studies published to the end of May 2001 and available on the United States Food and Drug Administration website, as indicated in the position statement² and the accompanying article.¹ A number of the references quoted by Vitry and Hurley became available after May 2001. Renewed scrutiny and analysis of existing datasets is interesting, but the results are best used to decide whether unresolved issues are of sufficient importance to justify further studies, and how these could be designed to deliver evidence that will convince us all, one way or the other. We made the point at the conclusion of the position statement that this is an evolving field and that conclusions may well change with emerging data.³

We consider the statements made in the considerations article¹ represent a fair expression of our assessment of the data available to us. Not everyone in the group agreed. In publishing the position statement

with the list of participants who endorsed it and those who did not, and by adding an article on the process we adopted, we hoped to highlight the fact that there are controversies and uncertainties about aspects of CSIs which require careful consideration in clinical use and further high quality data to resolve currently unresolvable issues.

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Guideline-discordant care in acute myocardial infarction: predictors and outcomes

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TO THE EDITOR: Advocating implementation of evidence-based clinical practice guidelines is one aspect of the current drive to provide quality healthcare across different centres.

Quality theory demands that outcomes are continuously sought and that practices are modified accordingly — the “quality loop”.

Therefore, Scott and Harper are to be applauded for their pursuit of improved outcomes, not just improved processes, in studying guideline-discordant care in acute myocardial infarction.¹ I believe that this type of study, which objectively demonstrates the role of practice guidelines in “real world” practice, is very important.

However, as a geriatrician, my patient population is unlikely to intersect with populations enrolled in large cardiology trials (eg, those for thrombolysis in myocardial infarction).^{2,3} Comorbidities, such as renal impairment, cognitive impairment and poor functional status at baseline, were not explicit exclusion criteria, but, when present, would have reduced an individual's chance of being enrolled.

These types of comorbidities are likely to be associated with a reluctance on the part of patients and physicians to pursue life-prolonging interventions. They are also likely to be associated with poorer outcomes, whatever the intervention. Therefore, I believe that these non-cardiac comorbidities are potential confounders for study designs, such as that of Scott and Harper.¹

Older age *per se* has been well studied in the cardiology literature on management of myocardial infarction. However, in the literature on adherence to guidelines, few studies have attempted to fully identify the non-cardiac-related characteristics of those receiving guideline-discordant care. Krumholz et al reported that altered mental state is one factor, and that, of a large “real-world” cohort aged 65 or more, only 8% were considered ideal candidates for thrombolytic therapy.⁴

Quality healthcare involves multiple dimensions, including both personal and process factors. Practice guidelines are valuable tools to reduce practice variation, but we need to continue to evaluate whether they can be applied as broadly as may be advocated.

Surely, evidence-based guidelines can only be confidently applied to situations for which an evidence base exists. It will be important to test the application of guidelines in many settings, with attention to potential confounders, and, in particular, to outcome measures.

1. Scott IA, Harper CM. Guideline-discordant care in acute myocardial infarction: predictors and outcomes. *Med J Aust* 2002; 177: 26-31.
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4. Krumholz HM, Murillo JE, Chen J, et al. Thrombolytic therapy for eligible elderly patients with acute myocardial infarction. *JAMA* 1997; 277: 1683-1688. □

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IN REPLY: We thank Pearson for her kind comments and agree the design of our study¹ prevented identification of all patient factors that may, quite reasonably, impact on clinicians' decisions to administer specific treatments to older patients with acute myocardial infarction (AMI). These factors may also have precluded such patients from enrolment in clinical trials, the results of which underpin recommendations within clinical practice guidelines.

On the other hand, we know advancing age is an independent predictor of increased mortality after AMI, with several possible causes: age-related reductions in protective mechanisms (such as myocardial preconditioning),² presence of cardiac and non-cardiac comorbidities unaffected by treatments for AMI,³ and — the focus of our study

— underuse of effective therapies in the absence of discernible contraindications.^{4,5} While cognitive impairment, renal dysfunction and poor functional status may dissuade patients and/or clinicians from pursuing “aggressive” management, we have no evidence that these factors, singly or in combination, necessarily attenuate the benefits of specific interventions for AMI in patients at high baseline risk of cardiac death.⁶ We also adjusted mortality comparisons between concordant- and discordant-care groups for multiple measures of illness severity at presentation which predict a poor prognosis.

Nevertheless, we support calls for more randomised trials of treatments for AMI and other conditions in older patients with liberal, “real-world” inclusion criteria in determining absolute risks and benefits of intervention in the presence of multiple comorbidities and impaired function.

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A Quality Use of Medicines program for continuity of care in therapeutics from hospital to community

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TO THE EDITOR: Several studies have documented the high incidence of adverse events arising during hospital admission. The potential for discontinuity of care and poor communication is significant when patients are admitted to and discharged from hospitals, hence the Australian Pharmaceutical Advisory Council (APAC) has established guidelines to ensure continuity in the quality use of medicines.¹ A study reported in 2001 by Mant et al found very low compliance with a minimum dataset based on the APAC guidelines.² These

authors subsequently held workshops to identify problems, develop action plans and refine these strategies. However, the follow-up report, published recently in the *Journal*, reported little change in adherence to the minimum dataset.³

Why are providers failing to follow the APAC guidelines? Certainly, one cannot assume that the formulation and dissemination of guidelines will necessarily lead to their implementation.⁴ To be effective, users must be aware of guidelines and convinced that they will add value to the way in which they work. Guidelines need to be credible and should make sense in the “real world”. Given the attitudinal barriers of some groups to the uptake of guidelines, multiple strategies are required to ensure their effective implementation. Among these is the involvement of key stakeholders in guideline development.

Who are the key stakeholders for ensuring continuity of care regarding therapeutics between hospital and the community? While Mant and colleagues report workshops involving general practitioners and hospital staff, their reports do not identify which hospital staff were involved.^{2,3} Were clerical, pharmacy and junior medical staff included? These staff could make a critical difference in adherence to the minimum dataset. Furthermore, are these staff even aware of the APAC guidelines?

The APAC guidelines use the definition of discharge planning established by the Council on the Ageing (Victoria). This describes *people*, hospitals and community-based services working together — but the guidelines and associated minimum dataset place little importance on the patient. Patients' knowledge of their medications is discounted. Despite being mentioned in principles 4 and 6 of the APAC guidelines, patient knowledge of medication changes and satisfaction with the communication regarding medications is not considered in the minimum dataset.¹

Strategies involving consumers should be explored as a mechanism for improving information exchange between hospitals and GPs. Similarly, an enhanced role for pharmacists warrants further consideration.⁵ Certainly, further critique of the APAC guidelines and exploration of reasons for their poor uptake is important to ensure optimal patient outcomes.

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