Reviewer’s report

Title: Impact of briefly-assessed depression on secondary prevention outcomes after acute coronary syndrome: a one-year longitudinal survey

Version: 3 Date: 23 December 2005

Reviewer: John Rumsfeld

Reviewer’s report:

First, I was specifically asked to evaluate whether the revised manuscript was responsive to initial reviewer comments, as the initial reviewer was not available for re-review: The response to initial reviewer comments is very good. Each point raised by the reviewer is addressed, with appropriate revisions to the manuscript. One minor comment: Query 5 response: the Discussion point re: why patients with unstable angina were more likely to be depressed than patients with MI is speculative. It may also be that depressed patients have more chest pain as part of the somatization of their depression.

Second, although formal review of the manuscript was not requested, there are several comments I would like to pass on for consideration by the authors for final revision:

1. Critical to survey studies is the potential for selection bias in who participated in the study and completed surveys on follow-up. The Methods section should explicitly note how many patients were admitted during the study period, how many patients were screened for the study, and then how many were consented and then how many completed follow-up. Ideally, some comparison of patients who were in the final analytic cohort to those that were eligible but did not enroll and/or did not complete f/u should be provided, to allow the reader to judge how selected the study cohort was.

2. Why were 2 different depressive symptom scales (HADS and BDI) used? Also, the authors say in the methods that ‘depressed cases from either scale were combined to provide the overall sample’, but then the results are broken out separately by HADS and BDI. It would make most sense to provide results combining the two (i.e. depressed cases from either scale, as intimated in the Methods) with secondary/sensitivity analysis to show that results were generally consistent with either scale. As written, the results end up seeming like a contrast/comparison of HADS and BDI, which detracts from the primary message of the study.

3. The methods/approach to multivariable modeling are unclear. Based on variables listed in Table 1, should the reader assume no other clinical data was available for risk adjustment? Table 1 itself is not intuitive re: what groups are being compared…..it appears to be the study cohort at baseline and follow-up, rather than a comparison of depressed versus non-depressed patients at baseline, which is what is expected based on the study question. A comparison of depressed versus non-depressed patients at baseline would allow the reader to see baseline differences by the primary independent variable, which is important in judging the potential for unmeasured confounding.

4. In the Results, it should be clear when unadjusted versus risk-adjusted associations are being presented. For example, I believe the medication associations are unadjusted (i.e. that the OR’s are from simple logistic regression), but this should be clarified.

5. The study is under-powered overall, as evidenced by wide CI’s. Therefore, the authors should be appropriately circumspect with any conclusions of ‘no association’. For example, depression was not significantly associated with cardiac rehab attendance, but there was a trend and wide CI’s….the analysis is therefore inconclusive with regard to this association (i.e. there is not power to conclude no association).

6. The study limitations should be expanded to include: a) limited power of the study, as evidenced by wide confidence intervals; b) potential selection bias based on who participated in the study and...
completed follow-up; c) the potential for unmeasured confounding in the associations found.
7. Minor: response rate to survey noted as 86% in the abstract and 76% in the Methods.