Reviewer's report

Title: Impact of case management by advanced practice nurses in primary care on unplanned hospital admissions: a controlled intervention study

Version: 2 Date: 29 September 2007

Reviewer: Martin Roland

Reviewer's report:

General

This is a carefully prepared and analysed piece of work (apart from a large number of typographical errors). However, a number of points need to be addressed before I can make a recommendation for publication.

The comparison (within- and between- practices) is a) for patients over 50, and b) a higher risk group. The higher risk group is defined as ‘people who had already undergone an admission or multiple admissions in the previous year’ (i.e. #1 admission), whereas later on (e.g. r in box 1), the high risk is those admitted more than once in the previous year, i.e. #2 admissions. The authors need to clarify which it is.

Next, in order to understand the results, it would be very helpful to know what the criteria were for admission to the APN scheme. In most schemes, this includes something about a past history of emergency admission.

Assuming this is the case, the results are very odd, because only 30% of the reduction in RRR occurs in patients with a previous history of admission: 70% occurs in the general population over 50 (page 13). In other words, the APNs had the greatest effect on admission in patients who they were not seeing.

Assuming my interpretation is correct (and it seems to be what the authors are saying), there must be something else going on in the intervention practices to reduce the rates of admission. Therefore the conclusion that the APNs caused the reduction may be flawed.

In understanding the authors’ response to this point, it would be helpful to know the absolute numbers of a) patients over 50 in the intervention practices, b) patients with a part history of admission (clarifying whether it is #1 or #2), and c) patients actively case managed. It will then be easier to understand whether the ‘effect’ produced is plausible, or whether one has to postulate some ‘wider’ effect of APNs over and above the patients they were seeing.

Finally, not all admissions are included in the analysis (e.g. they excluded admissions to community hospitals). It would be helpful if the authors could confirm that exactly the same types of admission were available to intervention and control practices before and after the study, and that the results might not
have been due to APNs selectively admitting patients to community hospitals. This is important because in the US Evercare study, the intervention didn’t stop old people getting sick, but provided better community based alternative management when they were ill. Do the authors have any idea whether admissions from the intervention group to community hospitals might have gone up?

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Clarification of the above

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Typographical errors

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

Non financial interests: I have published a similar study based on the English Evercare intervention.