Reviewer's report

Title: Can volunteer companions prevent falls among inpatients?: A pre-post comparative design.

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Reviewer: David Oliver

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General

1) Is the question posed by the authors new and well defined?

The specific question posed by the authors is whether the use of volunteers in a 'high risk bay' for four patients deemed at risk of falling can reduce the incident rate ratio of falls per 1,000 bed days. Falls are the biggest reason for critical incidents in hospitals and are a major risk management issue. They are also extremely common as this study demonstrates and lead to physical injury, impaired rehabilitation and loss of confidence for the patients. They also lead to anxiety and guilt amongst staff to complaints and to litigation, therefore, attempts to reduce their incidents are highly relevant to institutions and to older people. There have been a number of recent systematic reviews of fall prevention strategies and certainly the use of volunteers specifically to prevent falls has not been proven.

2) Are the methods appropriate and well described and are sufficient details provided to replicate the work?

This paper uses a mixed methodology. The quantitative components (ie reduction in falls rates) has been performed using what is authors describe as a ‘pre-post comparative design’ sometimes called a ‘prospective (historical control)’ study or a ‘before and after’ study. Such an approach is a pragmatic one but there are problems. It is hard to adjust for confounders such as changes in case mix, staffing or length of stay. There may also be underlying secular trends in falls rates which are responsible for apparent effect. Perhaps using the periods of February to May in two successive years was an attempt to counter this but there are very few data presented to let us know whether the comparison is a valid one. In any case, there should be a clear acknowledgement that a randomised controlled trial would have been a better method, although it may be difficult to obtain individual consent from ill or confused patients for this kind of study. There are already any number of before and after studies of fall prevention in hospital settings and most of them are of poor methodological quality. The authors have also chosen to look at falls rates and not number of persons who fall or injuries. There is some evidence of ascertainment bias here because the routine recording of falls maybe altered by the very process of performing a research study (the Hawthorne effect) and may also vary from time to time, ward to ward etc. There is also no evidence of a power calculation so that the apparent null result may simply be due to insufficient duration of intervention. The statistical analyses on incident rate ratio are appropriate.
There is also a qualitative component to the study. Telephone interviews of volunteers who participated were conducted using what appears to be a semi structured interview. Hours spent by volunteers were estimated retrospectively. Families of patients in the high risk bays and ward nurses who work with the volunteers were interviewed and the nurses completed a questionnaire. An outline of an economic evaluation is described with an estimate of 24 Australian Dollars per hour for volunteer companions and therefore the total cost of the intervention - though no details are given of the methodology employed. Some very interesting quotes and observations are picked out from the interviews but there is little detail on any more systematic approach to analysing the narrative in the qualitative data or the questionnaires so that the basis of the particular quotes and observations picked out in the results is not clear. I would question that at the moment there are insufficient data to replicate the work from the interviews of analysis of qualitative data or the economic evaluation component, but I am not sure how constrained the authors are by space in the journal.

3) Are the data sound and well controlled?

See my comments above. The quality of before and after studies is generally much lower than that from other study designs for reasons outlined. If you are going to employ a before and after design then some attempt to examine various confounders and ensure comparability should be made. In addition, a power calculation based on knowledge of previous falls rates would have been helpful.

4) Does the manuscript adhere to relevant standards for reporting and data deposition?

The manuscript reports the core quantitative outcome data of effect of falls rate and selectively reports some of the qualitative findings. Although quality scores are themselves flawed, it might serve the authors well to apply a score such as the ‘Downs and Black’ score to the study in order to improve the comprehensiveness of reporting of data. I also think there are a few potentially interesting pieces of data that haven’t emerged. For instance, having collected data on the times and locations of the falls whether the intervention made any difference to these, whether the kind of patients that would have been selected for high risk bays before and during the intervention were comparable and the specific falls rates of those individuals? Whether STRATIFY or clinical judgement were more or less useful in predicting falls etc.

5) Are the discussion and conclusions well balanced and adequately supported by the data?

The authors principal conclusions are that firstly there was no overall significant effect on the falls rate with a non significant increase in the number of falls per 1,000 bed days during the intervention (this is supported by the data). Secondly that no falls occurred while volunteers were in a bay (a very important and interesting finding). They go on to discuss with very relevant comparisons the example of hip protectors, an approach akin to realistic evaluation where adherence explains some of the mechanism for apparent effect. They also go on to discuss in view of the
qualitative findings that the intervention was generally well received but that there was drop out from the volunteers and that some nurses felt it generated extra work for them and some nurses and relatives were unclear about the role of the volunteers. All of this is quite reasonable discussion. However, in discussing the limitations of the methodology some of the problems outlined in my earlier comments could perhaps be addressed. Also, a major additional problem I have with this paper (and it may be my fault for not reading it properly) is that the patients deemed to be at high risk were put into a special high risk bay. From what I can see, a high risk bay was not used in the pre intervention year. I totally accept the authors argument that it could not be that the high risk bay alone that stopped falls as volunteers seemed to have an effect. However, the intervention is confounded by the fact that it consisted of volunteers plus a high risk bay during the intervention period. Moreover the patients at high risk have not been characterised at all. We are simply told that they were either selected using the STRATIFY score or clinical judgement. Thirdly, it seems perverse to examine the effect of the intervention on overall falls rate on a ward when it is only being applied four patients at any one time. Of course we know that up to 50% of the falls on the ward will occur in patients falling repeatedly and that these individuals can be targeted and overall falls rates may drop but there is no clear justification in the paper for using four beds for three months or for targeting the patients who were targeted or any comparison with falls rates in those who would not. Even using a before and after design it might have been better to have one group having a high risk bay and no volunteers and another group with a high risk bay and volunteers and perhaps a third with volunteers and no high risk bay. Also to analyse the effect specifically on the small number of high risk patients selected.

6) Do the title and abstract adequately convey what’s been found?
Yes.

7) Is the writing acceptable?
I found the paper very well written and feel it would be accessible to a general readership.

Overall Comments
This paper addresses an important subject, namely strategies for fall prevention in hospital inpatients and a relatively unproven intervention namely the use of volunteers to prevent falls. This intervention has some face validity as many patients who fall are those who are repeatedly trying to stand, transfer etc for whom proactive vigilance and assistance would intuitively prevent falls. It is also interesting because the use of volunteers should be cost neutral. The qualitative observations garnered from relatives, volunteers and staff are very interesting however as it stands the paper reads as an interesting pilot study or research letter rather than a definitive article. Insufficient attention has been given to the design or reporting of the quantitative (falls reduction) element of the study. It does not appear to have been adequately powered. Potential confounders have not been adequately discussed. The patients selected for the intervention have not been adequately characterised. The comparison is
not like with like in view of the use of high risk bays. The denominator of overall falls rates seemed an odd one to describe in isolation as this intervention was only applied to a pre-selected high risk group of individuals. There is insufficient detail on the analysis and synthesis of qualitative data or the methods used in economic evaluation. I am not sure if the authors can remedy this based on the data they already have in their possession. Nor am I sure what the word limit is for articles in the Journal but I would be happy enough to see a further version if you want any more peer review.

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

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

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after discretionary revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests.