Reviewer's report

Title: Delirium Risk Screening and Prophylaxis Program in hip fracture patients is a helpful tool in identifying high-risk patients, but does not reduce the incidence of delirium

Version: 1 Date: 2 June 2011

Reviewer: Laurent Michaud

Reviewer's report:

General remarks

This is an interesting article, which should be published if revisions according to comments are done. Results on Risk Model for delirium are noteworthy because few validated tools exist for the identification and specific management of high risk patients for delirium. Results on haloperidol prophylaxis are also interesting but should be discussed again after the revisions, which could change them.

Questions

1. Is the question posed by the authors well defined? Partly, see major revision 1
2. Are the methods appropriate and well described? Partly, see major revisions
3. Are the data sound? Yes mostly
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes
5. Are the discussion and conclusions well balanced and adequately supported by the data? Partly, see discretionary revisions
6. Are limitations of the work clearly stated? Partly, see major revision 3
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes
8. Do the title and abstract accurately convey what has been found? Partly, title should mention that prophylaxis is by mean of haloperidol and abstract should be revised considering the revisions below
9. Is the writing acceptable? As French speaker, we are not able to assess this point.

Advice on publication

Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions.
Reviewer's report

- Major Compulsory Revisions

1. We do not understand the rational of the statistical design of the study. This investigation has the objective to determine if haloperidol prophylaxis could be effective in reducing delirium incidence for a specific high-risk group and the major (negative) result use the comparison between the incidences in the whole prospective group (i.e. high risk AND low risk patients) and the whole control (historical) groups. In our comprehension, the right comparison should be between the high risk group in the prospective group (2008-2009) and the high risk group in the historic cohorts (2005-2006-2007). Even if we consider that it is likely that the control historic groups are comparable with the studied group, this should be established.

2. Ways of diagnosing delirium both in the experimental and control groups should be better explicated in the method. We understand that the diagnosis was made as part of a routine evaluation by the staff of the hospital (and not in a proactive way by specific and educated investigators and by means of specific tools (for exemple CAM)). As noted in the article, delirium is usually massively underdiagnosed. Consequently, it is likely that the study measures indeed the incidence but also the identification (recognition) of delirium by the staff. This implies a possible major bias: that the identification of the delirium enhances with time, education of the staff and attention given on this pathology by the research.

3. In the results, we understand that the 23 patients with a low risk for delirium were wrongly prescribed haloperidol prophylaxis and that this group had a significantly higher incidence of delirium. That should be addressed in the discussion because it is in contradiction with the study hypothesis.

- Minor Essential Revisions

1. In the paragraph Method/patients, the sentence “Minimum follow-up was 1 year” should be developed: does it mean that some patients were followed for more than 12 months?

2. In the last paragraph of Methods, it is stated that the model was developed in 2004 and we could understand that haloperidol prophylaxis was introduced at the same time. We suppose that it is not the case (this would suppress the difference between investigation and control group!) but it should be better formulated.

3. Page 6: a . is missing after ref 11-17

4. Page 9: is it the right website?

5. Table 1 could be easier to consult if only the percentages were given.

- Discretionary Revisions

1. In the discussion, emphasis could be more on the necessity of non pharmacological interventions for delirious patients and/or high risk patients.
2. Conclusion could also mention that maybe haloperidol prophylaxis is simply not effective in reducing delirium incidence.

Quality of written English
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As French speaker, we are not able to assess this point.

Statistical review
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Is it essential that this manuscript be seen by an expert statistician?
Yes, but I do not feel adequately qualified to assess the statistics.
Particularly, one question would be to know if it is relevant to include age and dementia in the confounding variables as they are part of the RD.

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

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests