Reviewer's report

Title: Does using a femoral nerve block for total knee replacement decrease postoperative delirium?

Version: 1 Date: 9 November 2011

Reviewer: Luzius A Steiner

Reviewer's report:

1. Is the question posed by the authors well defined? Yes
2. Are the methods appropriate and well described? The methods are clearly described but the study is insufficiently powered, further comments see below
3. Are the data sound? Yes
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Some further data are required (see below).
5. Are the discussion and conclusions well balanced and adequately supported by the data? Yes, however, I disagree with some of the conclusions (see below).
6. Are limitations of the work clearly stated? Yes
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 Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?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? Yes, but I do not fully agree with the conclusions (see below).
9. Is the writing acceptable? Yes

The authors investigate the effect of a continuous femoral nerve block on the incidence of postoperative delirium after total knee replacement. This is an interesting and well written paper. This is an interesting and well written paper on an important topic from a group with an impressive track record regarding investigations on the role of pain in the development of postoperative delirium.

I have three major concerns with this manuscript.

First, the assignment to the groups was not randomized. This led to an imbalance with three times more patients in the “PCA only” group. This imbalance accentuates the problem of insufficient statistical power.

Minor essential revision 1: Even though this was not a randomized trial, I think it would be important to know how many patients were screened for inclusion, how many completed the trial and what were the reasons for not completing the study. Were there any catheter complications such as dislocations? Was there any scheduled sensory testing to ensure proper functioning of the catheter, was the analysis based on the intention to treat?

Second, there was no effect of the femoral catheter on opioid consumption. The
differences in pain scores between the two groups were small and statistically not significant. Hence, pain management was not more efficient with the femoral catheter in this group of patients which makes interpretation of the data and testing the authors’ hypothesis very difficult. The authors address this issue with a multivariate logistic regression. However, missing data (only 63 of 71 patients were included; why only 63, according to Table 1 TICS scores are available for 64 patients?), and the small number of risk factors included in the model (again in part due to the small study size) limit the conclusions that can be drawn from this analysis.

Major compulsory revision 1: Please check the number of patients included in the multivariate analysis.

Minor essential revision 2: I suggest expanding the discussion regarding the lack of effect of the femoral catheter.

Discretionary revision 1: please consider reporting also absolute pain scores (table or preferably figure) in addition to the differences between pre- and postoperatively which you provide.

Third, the two groups differ significantly regarding baseline CNS disease including delirium and dementia. Preexisting cognitive impairment is a key risk factor for the development of delirium which in my view makes this imbalance relevant. The authors acknowledge the insufficient power of their study and suggest that a study of 2x 62 patients would be sufficient to demonstrate or exclude an effect of a femoral catheter on the incidence of delirium. I have some reservations regarding this statement. This calculation is based on the assumption that the observed proportions would again be 58 and 33% in the two groups. In view of the higher rate of CNS disease in the “PCA only” group this difference is in my opinion likely to be lower, hence the suggested sample size is too low. Personally, after looking at these data I would be reluctant to embark on the larger study you propose and perhaps rather investigate an intervention that is more “multimodal”. However, it is up to the readers to make up their mind on this issue.

Discretionary revision 2: Please consider adding this line of thought (multimodal intervention rather than femoral catheter only) to the discussion.

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

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

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