Subject: BMJ - Decision on Manuscript ID BMJ.2015.027246.R1

Body: 28-Sep-2015

Dear Dr. Elfar

Manuscript ID BMJ.2015.027246.R1 entitled "Shorter length of stay is associated with decreased early mortality after hip fracture: a total cohort study in the United States"

Please respond to the additional comments of the reviewers.

Yours sincerely

Georg Roeggla
groggla@bmj.com

First, however, please read these four important points about sending your revised paper back to us:

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Reviewer: 1

Recommendation:

Comments:
Comments from Peter Nordström

In general I would say that the authors have done a good job in responding to my comments and questions, and I feel that the manuscript has improved. I have a few new comments in response to the

changes made.

I'm not sure about the authors' intention concerning the supplemental Tables and what I believe is a supplemental Figure. These are not referenced to in the Results, the supplemental Tables are marked "Tables" and some of the Figures are not marked at all.
For me supplemental Table 1A-L could be removed since most of the information of interest is presented in supplemental table 1M, and also graphically in what I believe is supplemental Figure 1. Please ad also LOS 1-5, that probably is the reference, for the supplemental Tables.

In a sensitivity analysis presented in the statistics section the authors suggest that: "To control for a greater proportion of early discharges in New York State patients, a sensitivity analysis was performed by evaluating the odds of mortality between 15 and 45 days after hospital admission in all patients alive at 14 days post-admission, which created a theoretical LOS of 14 days for all patients in the cohort…"
I do not understand this, and I could be wrong. We clearly have two different systems in the USA and in Sweden. In USA 90% of all patients are discharged to rehabilitations centres with a much shorter LOS in hospital than in Sweden. From the Table I can see that 82.1% of the patients have a LOS of less than 11 days. For me that means that most of the patients with a longer LOS have complications or are generally frail with more comorbid conditions (as also evident from Table 1). This is important, because in our Swedish cohort the number of comorbid conditions did not clearly increase with longer LOS, and dementia clearly decreased. So in a model where those that die before a LOS of 14 days are removed, but those with longer LOS (e.g. 11-14 days) due to complications or more comorbidities will still be at higher risk of death after 14 days. Also, in general the risk of death is highest the first days after surgery. Thus, I do not understand how this analysis could adjust for the different systems, and that healthier patients are discharged early in the USA. In my mind this analysis will control for the generally increased risk of death early after surgery. I suggest that you remove the text "To control for a greater proportion of early discharges in New York State patients", or remove this sensitivity analysis if there is no other purpose.

In the BMJ paper we could not analyse whether the discharge location influenced the risk of death. The authors have also commented this. However, in a recent published study in JAMDA, in a similar material, we did also have that information available. I encourage the authors to study the paper.

What is most important/interesting with this article is if we could learn from the different systems to increase our knowledge concerning how hip fractures patients should be optimally cared for in the period after the fracture. This question is of increasing importance with an increasing number of elderly and tight economic situations in many countries. To give some examples from Table 1 it is clear that a significant portion of the patients is given "non-surgical" treatment. In Sweden basically all patients are surgically treated, since we know that death is much higher in those not surgically treated. As of now the second paragraph in the Discussion is discussing the fact that we did no subgroup analysis of those not surgically treated. This simply relates to the fact that there were basically no such patients. This is no criticism towards the current manuscript given that the actual number of patients not operated in Sweden is not easily found in published papers. Furthermore, in Sweden there are guidelines stating that all patients should be operated within 24 hours (and almost all are), while in this material the mean time to surgery is 1.8 days. In the analyses performed both non-surgical treatment and longer time to surgery was also associated with higher risk of death. It is also of interest that the 30 day post-discharge mortality was only 5.1%, whereas 3.9% died in hospital. This should be compared to our 30 day post-discharge mortality that was 5.8%, whereas 5.0% died in hospital. In summary I feel that the Discussion is well balanced but the different systems compared to in Sweden could perhaps be further emphasized in an Editorial with a more in-depth evaluation of the differences, and perhaps especially how optimal care after a hip fracture should be organized.

Please emphasize throughout the manuscript that you have analysed 30 day post-discharge mortality.

Additional Questions:
Please enter your name: Peter Nordström

Job Title: Professor

Institution: Geriatric Medicine

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: Yes

Funds for a member of staff?: No

Fees for consulting?: Yes

Have you in the past five years been employed by an organisation that may in any way gain or lose financially from the publication of this paper?: No

Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this paper?: No

If you have any competing interests (please see BMJ policy) please declare them here: I believe I have no competing interests of significant value, except the recent paper published in BMJ, and a follow up study recently published in JAMDA.

Reviewer: 2

Recommendation:

Comments:
All required revisions to this paper as previously reviewed have been made by the authors; this has strengthened the paper considerably. I recommend publication at this time.

Additional Questions:
Please enter your name: Edward J. Fox, MD

Job Title: MD-Physician Professor of ORthopaedics

Institution: Penn State - Hershey Medical Center

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

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Reviewer: 3

Recommendation:

Comments:

Statistical Review:
In my view the new version of this manuscript adequately addresses all the questions asked by the reviewers. There are three very minor points that need checking:

1) In their letter of response to reviewers (page 2) the authors write that as part of their sensitivity analysis –"However, it found that for each 1 day increase in length of stay for these patients, there would have been an associated 6% increase in their offs of death during 11-30 day time period after hospital admission over the study period (95% confidence interval 1.05 – 1.08; $p<0.001$)." In the next page (page 3 of the reply) they state that in the Results this increase was of 7% with an associated 95%CI of 1.07 – 1.10. This is consistent with what is presented in the manuscript but not with the paragraph above. Please check.

2) Looking at Figure 2, the result for 2009 appears to be an outlier. If possible please can they check the influence of this extreme year on their results? It could just be reported as a single sentence in either the same Figure or in the Discussion.

3) In their response to reviewers letter, the authors comment that they had originally planned to use Cox models but that the assumption of proportional hazards was not met. Please can this also be reported in the Limitations/Conclusions as it is a change from the original approach that could be useful for readers. This also explains why they use Kaplan-Meier estimates but do not report hazard ratios.

These are all very minor points that have minor impact on the current manuscript.

Additional Questions:
Please enter your name: Rafael Perera

Job Title: Professor of Medical Statistics

Institution: University of Oxford

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

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If you have any competing interests (please see BMJ policy) please declare them here: none

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