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.

1. **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.**

   We agree that the supplemental tables must be referenced and presented better in the manuscript. We have made several adjustments:

   We have added text to the Results -> Sensitivity analysis:

   > *"The interaction variable (each 1-day incremental increase in length of stay over 1 day) was significantly associated with increased odds of mortality in seven of the twelve years in the study (Supplemental Table 1)."*

   Figure 2, which was intended as a primary and not supplemental figure, has been referenced in the Results -> Subgroup analysis subsection with the following text:

   > *"After adjustment for comorbid characteristics, injury, and demographics, each year from 2000-2011 demonstrated a consistent trend of increasing odds of 30-day post discharge mortality with increased hospital LOS, as shown in Figure 2 (unadjusted odds ratios for 30-day mortality showed a similar trend and are shown in Supplemental Table 2)."*

   Supplemental Tables 3 and 4 have now been referenced in the Results -> Subgroup analysis subsection.

   All supplemental tables have been relabeled as such and re-ordered to correspond to their reference in the text.

Supplemental Tables 3A-I have been combined into a new “Supplemental Table 3” to include just the primary variable of interest (mortality at LOS 1-5, LOS 6-10 etc) for each disposition subgroup.

Supplemental Tables 4A-E have been combined into a new “Supplemental Table 3” to include just the primary variable of interest (mortality at LOS 1-5, LOS 6-10 etc) for each comorbid condition subgroup.

We hope these changes make the Supplemental Tables more easily accessible and ease interpretation of the key findings.

2. **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.**

   We have removed supplemental Tables 1A-L as the important aspects of the adjusted analysis by year are included in Figure 2 (as previously noted). We have changed Supplemental Table 1M to Supplemental Table 1 to demonstrate the unadjusted subgroup analysis by year of study.

3. **Please add also LOS 1-5, that probably is the reference, for the supplemental Tables.**

   We have added LOS 1-5 to all Supplemental Tables, which is the reference.

4. 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.

We performed this analysis in conjunction with our second sensitivity analysis (odds of death between 11 and 30 days for patients alive at day 10) in response to a request from the Editorial Board, which was to move the goalposts from earlier to later discharge to address the possibility of survivorship bias. However, we acknowledge the point Dr. Nordstrom raises and agree that this analysis may not add significantly to the manuscript and may created unintended confusion. Therefore, at the recommendation of Dr. Nordstrom, we have removed the paragraph in question and changed the first sentence of the following paragraph from, "The second sensitivity analysis evaluated the odds of death between 11 and 30 days for patients alive at day 10 with a length of stay of 10 days or less while controlling for other covariates in the primary multivariate model" to:

A sensitivity analysis to assess evaluated the odds of death between 11 and 30 days for patients alive at day 10 with a length of stay of 10 days or less while controlling for other covariates in the primary multivariate model.

We have also removed the paragraph in the results section referencing this sensitivity analysis and Supplemental Table 2.

5. In the BMJ paper we could not analyze 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.

We have removed the statement
"No discharge destination was available for the Swedish study so direct comparisons between patients discharged to rehabilitation facilities vs. home discharge was not possible."

We have added the following to the discussion:

"The effects of discharge destination on short-term risk of death after hip fracture in Sweden has also been evaluated.[14] In a recent Swedish study of 89 301 patients from 2004 through 2012, most patients were discharged to nursing homes (61.8%) or their own homes (31.1%). After adjustment for comorbid characteristics and functional level, discharge to a nursing home and especially a short-term nursing home with a LOS of 10 days or less was associated with higher risk of early mortality. In contrast, only 12.9% of patients in New York State were discharged to their own homes; the vast majority were

discharged to short- or long-term care facilities, and longer LOS was associated with higher odds of 30-day post discharge mortality."

6. 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.

Some differences in treatment do exist between Sweden and New York State. We would like to emphasize the point raised by the reviewer that hip fracture patients ought to be treated surgically treated whenever possible, and in the New York State population in our study the number of patients managed non-operatively is small compared to those managed operatively (10% versus 90%). In designing our study we believed it was important to include these patients to prevent introducing bias toward healthier patients skewing the results toward improved outcomes and survival. However, to make our analysis comparable to the Swedish analysis, we excluded nonsurgically managed patients from the analysis and the results were not substantially different.

Notably, patients in our study with the LOS>14 days had similar 30-day post discharge survival to patients managed nonsurgically, as shown in Figure 1. We have added the following text to the results section:

"Patients with the LOS>14 days had similar 30-day post discharge survival to patients managed non-surgically."

In the section 'Results -> Clinical implications of the results' we have modified the third and fourth sentences to read:

"The Swedish cohort contained 116 111 patients who were managed surgically. In New York State the prevalent standard of care for hip fracture is surgical treatment in all patients who can tolerate surgery."

We have also added the following statement:

"The lack of a non-surgical group likely represents different standards of care for hip

fracture in Sweden, where virtually all patients with hip fracture are treated surgically."

7. 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.

The push toward early surgery is supported by multiple publications that have generally found decreased mortality with early surgery. Nevertheless, few of these studies controlled for comorbid characteristics, and there is some evidence that after adjustment for comorbid conditions mortality is not increased by delay of >24 hours. It is important to note that in Sweden surgical care seems to be better optimized for providing surgery within 24 hours than in the United States and we have no clear way of knowing whether the 0.8 day difference in time to surgery contributes significantly to mortality. We have added the following text to the discussion:

"With the generally accepted relationship in the hip fracture literature between increased time to surgery and increased mortality,[11 12] *the longer time to surgery in New York State patients would predict higher mortality. However, despite fewer patients treated surgically and longer mean time to surgery, the 30-day post discharge mortality in New York State was lower than in Sweden (5.1% vs 5.8%); in-hospital mortality was also lower in New York State (3.9% versus 5.0%) although patient factors may differ amongst populations. Our findings are in line with those of Grimes et al who found that after adjusting for comorbid conditions time-to-surgery of >24 hours was not associated significantly with 30-day mortality.*[13] *While there is no way to know whether the 0.8 day difference in time-to-surgery between Sweden and New York contributes significantly to mortality, it may be that early surgery is more important than surgery within 24 hours as seen by the respective differences in mortality."*

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

We have added text throughout the manuscript to emphasize 30-day post discharge mortality.

Additional Questions:
Please enter your name: Peter Nordström

Reviewer: 3

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 odds 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 8% 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.

We appreciate this astute observation; the numbers in the manuscript are the correct numbers; it was our oversight in not correcting the first numbers in the response. We apologize for any confusion this created.

The correct associated increase in odds of death during the 11-30 day time period after hospital admission over the study period in our study is 8% (95% CI 1.07-1.10; p<0.001).

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.

We have repeated the primary logistic regression analysis with exclusion of all patients from 2009 to evaluate whether this year influenced the results. The odds ratios for length of stay (other variables in model available but not presented here) are:

Length of stay		
1-5 days	(reference)	
6-10 days	1.06	(95% CI 1.01-1.13), P=0.03
11-14 days	1.40	(95% CI 1.29-1.52), P<0.001
>14 days	2.16 (95% CI 2.00-2.34), P<0.001	

We have added the following text to the Methods – Subgroup analysis section:

"2009 represented an outlier where the association between hospital length of stay and 30-day mortality was especially pronounced. Exclusion of patients sustaining a fracture in 2009 from the primary adjusted analysis did not substantially alter the findings in this study."

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.

We have added the following text to the methods section:

Risk of mortality while adjusting for comorbid conditions was performed with Cox proportional hazards models. However, the proportional hazards assumption was violated on testing with P<0.05; therefore multivariate logistic regression analysis evaluated risk of mortality following discharge based on categorical LOS while adjusting for covariates in the univariate model.

We have added the following text to the limitations section:

"The original study design planned to use Cox regression models but the assumption of proportional hazards was not met. Use of a logistic regression model represents a deviation from the original study design and may be considered a limitation."