Author's response to reviews

Title: Predicting hospital mortality among frequently readmitted patients

Authors:

Wim F van den Bosch (w.bosch@antoniusziekenhuis.nl)
Johannes C Kelder (keld01@antoniusziekenhuis.nl)
Cordula Wagner (c.wagner@nivel.nl)

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Author's response to reviews: see over
Dear Editors,

We have received your comments on our manuscript as well as the comments of the two reviewers. We think these comments are valuable and we have incorporated these in the new version (v2) of the manuscript. Please find below our point by point response to the comments. We think the quality of our manuscript improved by this substantially.

Also on behalf of my two co-authors I like to thank you and the reviewers for the excellent comments provided.

Best regards,
Wim F. van den Bosch

Response to the comments of the editors

In your Acknowledgements section, please list the source of funding for all authors and all those listed in the Acknowledgments.

Reaction of authors
There was no organisation funding this study.

Submission of a manuscript to BMC Health Services Research implies that readily reproducible materials described in the manuscript, including all relevant raw data. Is all your data in the public domain, in accordance with this policy?

Reaction of authors
The raw data used in the study concerns confidential data (patient records) that is not allowed to be opened to the public domain. Although anonymised, hospital names could be retrieved by their sample sizes. If one of the editors would like to reproduce the figures, we kindly invite them to visit us.

Please also highlight (with 'tracked changes'/coloured/underlines/highlighted text) all changes made when revising the manuscript to make it easier for the Editors to give you a prompt decision on your manuscript.

Reaction of authors
All changes that we made in Word are visible through tracked changes.

Response to the comments of Mr Mohammed

Major comments (some comments are discretionary)

1. It is not clear to what extent the HSMR used by the authors overlaps the HSMR developed by Dr Foster. If possible, an appendix comparing and
contrasting the Dutch HSMR with the English HSMR would be useful with further clarifications in the text as required. For example it is not clear if the remark “Currently the HSMR is not adjusted however for any form of readmission” (pg5) applies to the HSMRs in general or the Dutch only HSMR or the English only HSMR.

**Reaction of authors**
Both comments have been included in the text of the manuscript

2. Pg 5 quotes Jarman as suggesting that HSMRs with one-year re-admissions are not very different from normal HSMRs. What method was used to make the comparison? If correlation analysis was used then it is worth noting that where alternative assessments are to be compared, reliance on correlation analysis is illusory [Altman D & Bland M (1983) Measurement in medicine: the analysis of method comparison studies. The Statistician 32:307-317]. Consider that Dr Foster have used an equation with over 60 covariates to derive HSMRs and then repeated the calculation with one covariate (eg re-admissions) removed and found that the HSMRs hardly change. The two equations are virtually identical and the comparison is designed to produce a high correlation. The issue is not the extent to which HSMRs with and without a covariate correlate, it is the extent to which the HSMRs of individual hospitals change.

**Reaction of authors**
We agree. However the article by Dr Jarman does not mention in detail how the comparison was made and how he arrived at his conclusion. Our quote was meant to demonstrate that Dr Jarman suggests there is no issue here, whereas we think there is an issue as demonstrated in our article. In the discussion (page 15) of our article we explain why Jarman did not find any association (“…..This result matches the findings of Jarman [11], where no differences in HSMR were detected by picking n\textsuperscript{th} admissions for any value of n. ….”)

3. The authors state the use of “admission frequency” as a term in the HSMR model provides a more accurate HSMR. This may be correct, but (a) i could not see evidence to support this and (b) a test for interactions would provide statistical evidence for/against the constant risk fallacy and this is the crucial issue. Indeed a key message which the authors may wish to explore is that incorporation of tests for interactions for each term in the HSMR model as tool to screen for non-constant risk relationships.

**Reaction of authors**
We did not conduct an additional regression calculation. Currently we have data from six hospitals only; predicted death values per admission were provided to us by Prismant. Your observation is correct and the statement “By introducing ‘admission frequency’ as an additional adjustment variable for the HSMR model, a more accurate HSMR can be calculated” is too strong for the moment and should be replaced by “The accuracy of the HSMR may improve by taking admission frequency as an additional adjustment parameter.”
In the near future we will conduct such a regression calculation and interaction analysis for the Netherlands over a five year period (we have requested to make the necessary data available to us) and plan to report the results of this through a subsequent article.
4. On page 6 the authors state: “Risk factors used in the adjustment may be related in different ways to the in-hospital risks. To ignore this, therefore, may result in the so-called ‘casemix fallacy’ [1]. This could be clearer. Ignoring non-constant risk relationships commits the constant risk fallacy. Attributing the residual (unexplained) variation from case-mix adjusted mortality to quality of care commits the “case-mix adjustment fallacy”.

Reaction of authors
Thanks for better articulating this phrase; comment accepted.

5. The authors state that “A large interaction was found between numbers of admissions per patient and HSMR-predicted risks.” but did not provide any statistical quantification of the interaction effect (ie effect size, 95% CI and p-value).

Reaction of authors
See new table 6

6. Given the results of the authors analysis, the findings of the Heijink et al’s report (ie that they did not find evidence that the HSMR cannot be used as an indicator to monitor and compare hospital quality in the Netherlands,) could do with further comment perhaps in the discussion.

Reaction of authors
Currently there are more issues with the quality of the HSMR in the Netherlands as described in [10] (unfortunately in Dutch). Heijink et al do not pay quantified attention to these aspects. Issues concern measurable inconsistencies in coding practices, casemix variations on ICD-9 level that are not properly adjusted and variations in special medical procedures (some hospitals are allowed and obliged to conduct these, others are not allowed, again resulting in non-constant risk conditions for admissions within the same diagnostic group). A further description of this in our article may confuse the reader (an earlier version contained some of it, but we removed it again because proof readers were confused by it). It would deter the attention from the subject we like to address: the effect of variation in admission frequency.

7. The definition of readmission is clear but fails to recognise the distinction identified by the authors – ie between and emergency (unplanned) re-admission and planned re-admissions. This does seem important because the clinical risk of death is unlikely to be similar in the two modes of re-admissions. Any further information and clarification on re-admissions would be helpful. Also, it is not clear how transfers of patients between hospitals have been handled. Example 3 invites the question as to how transfers are handled in the Dutch HSMR and to compare and contrast this with the English HSMR. This is likely to be a universal issue wherever HSMRs are used.

Reaction of authors
For the time being we took readmission frequency as a first proxy. Since admission method is a separate adjustment parameter in the regression these effects are
already accounted for to a certain extent. Making distinction between planned and unplanned readmissions can thus be considered a possible way of fine-tuning, but we are not sure to what extent confounding effects will pop up. (Compare this for example to age group, where also no distinction is made between planned age group versus unplanned age group.) This is another aspect that needs further investigation when conducting a nationwide regression calculation.

With respect to transfers: in the Netherlands the ‘source of referral’ is coded in the LMR but not yet used in DHM-2008. (It was used in DHM-2009). In UK source of admission is used as regression parameter.

8. Table 6 shows that co-morbidities increase as re-admissions increase although it is not clear why the charlson score should, on average, increase over re-admissions. Further comment on this would be useful – perhaps the process of recoding comorbidities becomes more accurate over time? On pg 14 the authors state that “Patients with higher admission frequencies bear lower predicted risks per admission, which can be explained by shifts in the casemix”, but table 6 also suggests shifts in % emergency admission may be a factor and this is likely to be bigger factor because it dominates the HSMR equation. This point needs to be incorporated into the discussion on pg 16.

Reaction of authors
It is a logical consequence of the fact that we applied the notion of readmission for any disease. So patients with more co-morbidity may be admitted more often for the various diseases they have. We will mention this in the manuscript. The lowering of % emergency admissions (which we consider part of the casemix) indeed is the main driving factor behind lowering of predicted risk.

9. The individual HSMR plots in figure 3 should be incorporated into the results under the section “Mortality per patient view class and per admission view class”.

Reaction of authors
The plot is not intended to be a result of research, but rather a illustrative confirmation of the point we made: higher admission frequency → lower (H)SMR. See also our comments on minor point 13.

10. It is not entirely clear to me that the suggested solution (add “admission frequency” variable to the HSMR model) has been undertaken in this paper. If the authors have produced revised HSMRs using patient views with readmission counters then it would be useful to see a table comparing HSMRs for each hospital using this approach.

Reaction of authors
We did not conduct an additional regression calculation. Currently we have data from six hospitals only; predicted death values per admission were provided to us by Prismant. In the near future we will conduct such a regression calculation and interaction analysis for the Netherlands over a five year period (we have requested to make the necessary data available to us) and plan to report the results of this through a subsequent article.

11. I would also like to point out that the issue of “interactions” is relatively new to
non-statisticians and can be difficult to grasp and so the authors attempt to help
develop understanding (eg by using the three examples) is welcome. Perhaps I
could urge them to shoulder more of this educational burden than is typically the
case for a manuscript and suggest that the “same patient” approach is easy to
grasp. By explaining that when the “same patient” is re-admitted in some
hospitals and not in others on a large enough scale then the adjustment may be
biased. The scale can be determined by examining the size of interaction effects.

Reaction of authors
We are not sure whether you are proposing to amend the manuscript or whether you
just want to express your appreciation for our clarifying examples. In view of the other
changes in the manuscript we propose not to further amend the text for this specific
comment

Minor Comments (some comments are discretionary)

1. The authors have felt the need to not identify the individual hospitals. This is
fine but does restrict the information available in tables. In most tables the
sample size for each hospital is missing. I would prefer to see the sample sizes
and so suggest that hospital names from table 1 be removed and replaced with
letters A-F from table 2. If the authors were agreeable to this then it would also
enable funnel plots (eg % P01 vs N from table 2 to be plotted).

Reaction of authors
Yes, we have made the split. So the number of admissions are mentioned for A-F.

2. Table 3 should incorporate the “m” terminology that is used in page 11: “We
analysed the distribution of the admission frequency per patient class P(m) per
hospital (table 3). For m=1 hospital B has the highest percentage admissions....”

Reaction of authors
Yes, we have adapted the tables according to this terminology; also for the n.

3. Table 3 last column may be better labelled as “All hospitals” as opposed to
“Total”.

Reaction of authors
Yes, we have adapted this.

4. Table 6 could also report means and SD for the Chalson index and each CCS
in the table. Sample sizes in each column would be helpful.

Reaction of authors
SD for the Charlson and CCS would complicate the table; we prefer to not make the
table more complex than it already is.
Sample sizes are now contained at the top of the table.

5. On page 6, it may be worth clarifying that “In order to analyse this, we
addressed the following research questions using HSMRs from six Dutch
hospitals”.
6. Table 4 shows the crude mortality within strata. There is an argument that those patients in the previous strata are still in the at-risk set for the next strata. (see BMC Pregnancy and Childbirth 2003, 3:3 http://www.biomedcentral.com/1471-2393/3/3). To make table 4 and 5 consistent, the admissions column in table 4 (although informative) may be removed?

Reaction of authors
Each table must be read on its own. The informative point of table 4 is that it shows how the number of admissions increases in relation to an increase of patient view class, so how a single patient can increasingly contribute to the number of admissions. It also enables us to show mortality per patient and mortality per admission. Since admission is the basic building block of the regression, we prefer to keep the information on admission as well in this table at the cost of inconsistency between the two tables.

7. Pg 12: “Overall (bottom line of figure 1) hospital D has the highest average readmission frequency (1.14) and hospital B and F have the lowest average (0.57); a factor of 2 difference between the highest and the lowest.” I could not relate this to figure 1. Eg y-axis is >0 so 0.57 does not appear to make sense. Also y-axis label in figure 1 needs to clarify that numbers are sample sizes (ie Neoplasms: n=55642)

Reaction of authors
You are right; the scale should start with 0 (admissions in excess of initial admission) and the title of the X-axis should read ‘readmission frequency’ in order to be consistent with the text. We have adapted accordingly and added that the y-axis mentions sample sizes.

8. The title of the paper could be more informative if the idea that the HSMR is biased by re-admission frequency is included in the title.

Reaction of authors
We added a subtitle “HSMR biased by readmission”

9. The manuscript is clearly written but could be more succinct. Eg statements such as “We conclude that the first research question can be answered positively.” may be edited out.

Reaction of authors
We attempted to make it more succinct here and there. The example you mention was also addressed by the other reviewer: “…the ‘research questions’ are mentioned, without helping the reader remember which questions they refer to at specific times, etc.” We have repeated the question in the text.

10. Pg 11. Occasionally the terms SMR is used when perhaps the term HSMR
was meant?  
Reaction of authors  
The term SMR was used here to indicate a part of the total population of a hospital, for example only the patients in patient view class m=4. (Similarly SMR is used to indicate the standardised mortality ratio for a diagnostic CCS group). We only apply the term HSMR if the overall hospital figure is meant.

11. The equation used to derive the Dutch HSMR should be reported (perhaps in an appendix).  
Reaction of authors  
We have expanded the text on this also describing the differences between Dutch and UK HSMR model.

12. Pg 9 “The model considers each admission to be an independent experiment, separate from previous admissions, for which a risk of death number is predicted and accumulated into the denominator of the HSMR.”. The term experiment is potentially confusing here.  
Reaction of authors  
We adapted the text to make it more clear (...an independent stochastic experiment, like repeatedly throwing a dice, ....)

13. All plots should have clear backgrounds and light grey gridlines to aid visualization. Figure 3 could include labels A-D instead of points and also the correlation coefficient reported with 95% CI.  
Reaction of authors  
We have adapted the plots.  
In figure 3 we marked hospitals A-F and provided the \( R^2 \) value. Again: this picture is intended to be illustrative for the reader and we consider this not part of the research results.

14. The examples on page 15 are useful. It would be useful to know if there is any empirical evidence in this study for these examples. If available this would be important practical information which links “interactions” to “explanations” unrelated to quality of care. If the examples could be further investigated in respect of one or more hospital then they should be part of the results (a la BMJ 2009;338:b780.) Example 2 is approaching what i have in mid and could form part of the results if the authors regard “explanation” as a further aim in the study.  
Reaction of authors  
All of the examples certainly warrant further study. However we do not want to make that part of this manuscript, since we think we already addressed a lot of subjects in a single article.

15. Pg 15: Suggested change: “Example 1: Admission policies may increase the number of readmissions without proportionally increasing real risks. One hospital may systematically combine the diagnosis and treatment into a single admission. Another hospital may have an admission for diagnosis and a second one for
treatment, being granted a double predicted risk count, most likely without doubling of real risk, but doubling the expected risk.”

Reaction of authors
Yes, we have adapted accordingly

16. Pg 16: If this hypothesis is valid (preferred to true).

Reaction of authors
Yes, we have adapted accordingly

Response to the comments of the Mr Berg

This is a well-researched and relevant and timely paper, by a group that has proven to be knowledgeable and independent. I feel that the only issue to be taken into consideration is that the conclusion and discussion could be a bit better structured: the ‘research questions’ are mentioned, without helping the reader remember which questions they refer to at specific times, etc.

Reaction of the authors
We agree that the Discussion section is a bit long; maybe also because we illustrated the points made by means of 3 examples.
The references to the research questions occur at the end of the Results and as part of the Discussion section. We have repeated the question where appropriate.

An issue I missed in the discussion was that (frequent) readmissions are sometimes taken to be a proxy indicator for poor quality of care. There could be instances in the data where this would be a better interpretation of a higher readmission figure that e.g. the way a hospital organizes the care throughout a care trajectory. This would not necessarily make the claims of the authors less relevant, but a discussion of this potential interpretation is warranted.

Reaction of the authors
We have added your point to the Discussion section