Reviewer’s report

Title: Predicting hospital mortality among frequently readmitted patients

Version: 1 Date: 1 July 2010

Reviewer: Mohammed Amin Mohammed

Reviewer’s report:

Review of manuscript “Predicting hospital mortality among frequently readmitted patients. By Wim F van den Bosch et al

Reviewer Name: M A Mohammed
Date of review: 27 June 2010

Overall comments

The authors present an important paper demonstrating the susceptibility of HSMRs to the unit of analysis - admission vs patient. This is a fundamental issue.

The authors elegantly demonstrate that when the numbers of readmissions per patients differs substantially between hospitals then the resulting HSMRs are biased against hospitals with lower readmissions. This is an important message for producers and consumers of HSMRs but there are issues in the manuscript which need further consideration and/or clarification.

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.

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.

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.

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”.

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).

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.

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.

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 comorbidites 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.

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”.

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.

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.

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).

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....”

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

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.

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?

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)
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.

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.

10. Pg 11. Occasionally the terms SMR is used when perhaps the term HSMR was meant?

11. The equation used to derive the Dutch HSMR should be reported (perhaps in an appendix).

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.

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.

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.

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.”

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

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

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'