Author's response to reviews

Title: Bayes Rules for Optimally using Bayesian Hierarchical Regression Models in Provider Profiling to Identify High-Mortality Hospitals

Authors:

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Author's response to reviews: see over
Reviewer: Byron J Gajewski

Reviewer's report:

-Major Compulsory Revisions
None.

-Minor Essential Revisions

1 P. 16. The author indicates that a hospital is included in the analysis only if there are at least 30 AMI patients in that hospital during a 12 month period. The author claims that this exclusion is common practice and gives references. I would like to see a paragraph or so devoted to explaining this reason. Further, I do not see why this exclusion is necessary. I think that it is fairly clear that one of the strengths of the hierarchical model is that hospitals can borrow information from others. The paper has been revised so that all hospitals that treated at least one AMI patient in 2000 are included in the case study. This increases the number of hospitals to 163 and the number of patients to 19,757 (page 16, lines 1-4). There is now no restriction on hospital volume in the case study.

2 P. 19, fourth paragraph, line 3. I think it should say âhospitals A and B should beâ. This paragraph has been rewritten in the revised manuscript (page 19, lines -3 to -1).

3 P. 20. The author defines the unacceptable high mortality to be \( \beta_0 + \log(1.5) \) and to be a fixed point. But since \( \beta_0 \) is a distribution, this should be made clear that the line is the expected value of this threshold. Perhaps it would be more appropriate to place a 95% interval in Figures 1 and 2 instead of a fixed point. This clarification has been made in the revised manuscript (page 20, lines 16-18). In addition to the solid line denoting the posterior mean, 95% credible intervals have been added to Figures 1 and 2.

-Discretionary Revisions

1. P. 7. The author defines Beta_thresh as the threshold for acceptable quality of care. It would be beneficial to the reader to offer some real guidance on how this threshold can be defined. For example, Gajewski et al (2006) discusses this point and offers references for defining acceptable thresholds for different outcomes in the nursing home. It would be interesting if the author could offer other references. The manuscript has been revised to add some guidance on how this threshold can be defined. The manuscript now cites the suggested reference (page 7, line -1 to page 8, line 3).

References:

What next?: Accept after minor essential revisions

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

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests: I declare that I have no competing interests.
Reviewer: Michael Racz

Reviewer's report:

Major Compulsory Revisions

1) In the case study, the author flags high mortality hospitals under 9 different criteria; Three loss functions each with 3 different tolerances for the type of errors acceptable. I think the paper could be improved if there is a comparison with some of the methods endorsed in the literature or used by states that publish report cards. For example, the author refers to the Normand et al. method and a follow-up analysis by Austin and Brunner which demonstrated that the use of posterior tail probabilities was the Bayes Rule associated with generalized 1-0 loss. In the case study there are hospital A-E which are high mortality outliers under different loss function specifications. If the Normand method was applied, how many of A-E would be outliers? Consider a method from a state which does not apply a shrinkage methodology. How many of hospitals A-E would be high mortality outliers under this methodology? The manuscript has been revised to consider a method that does not use shrinkage. We report the ratio of observed-to-expected mortality using model-based indirect standardization (page 21, lines 1-11). This resulted in 15 hospitals being identified as having higher than acceptable mortality. The primary objective of the current study was not to compare different methods for provider profiling (I have published several papers comparing the relative performance of different statistical models for identifying high-mortality hospitals). Instead, the objective of the current manuscript was to derive Bayes Rules for use with Bayesian hierarchical models to minimize costs when classifying hospital performance. For this reason, comparing profiling outcomes with different methods has been kept to a minimum.

2) On page 17, the case study MVN proper prior mean vector, (-2.06, 0.91), is very specific. How sensitive are the results to these numbers? It might be worth repeating here how these numbers were chosen. We have revised the manuscript to describe how these numbers were chosen (page 18, lines 1-3). They were obtained from fitting a similar model to the data from the previous year. These are the estimates for the mean random intercept and mean random slope.

Minor Essential Revisions

3) In the first paragraph of the Background, the author puts Massachusetts in the category of publishing hospital-specific mortality rates. They recently released surgeon-specific as well. The manuscript has been revised to reflect this change (page 4, line 7).

4) In the second paragraph of the background, the author discusses two main goals of provider profiling and makes reference to a paper by McGlynn. It might be helpful to give the reader some additional references which give summary overviews...
of profiling issues. An example is: Normand, S-L T, Shahian, DM (2007) Statistical and Clinical Aspects of Hospital Outcomes Profiling Statistical Sciences 22 206-226. The manuscript has been revised to cite the suggested manuscript (page 4, lines -5 to -4).

5) In the 3rd line of the first full paragraph on page 5, pediatric is misspelled. The spelling in the manuscript is the British spelling of the word. The reviewer is suggesting the American spelling. I will leave the decision as to which is appropriate to the editors.

6) The term “random effects” is in the title of Figure 2. The text states these are random intercepts. Also, it should be indicated on the figures that the solid vertical line depicts a threshold for high mortality. The requested modifications have been made to Figure 2.

Discretionary Revisions

7) There are several derivations in this paper. One suggestion is that first one or two be part of the text and the others in an appendix. I would prefer to leave these in the body of the text, since the form the key contribution of the paper.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I have non-financial competing interests in that I am an analyst for the New York State Department of Health report cards which assess providers of coronary artery bypass graft surgery and percutaneous coronary interventions.