AE Report on "Bayesian Restricted Likelihood Methods" by John R. Lewis, Steven Neil MacEachern, and Yoonkyung Lee

Summary

This is a thought provoking paper that pursues a novel idea how to perform Bayesian inference under the assumption that only part of the data are so-called good data, while the rest are bad data or outliers in the sense that they are generated by a probability law different from the good data.

I agree with both reviewers that this is an interesting issue and that the paper provides a novel and ingenious contribution in this context. However, both referees raise several concerns which I share:

1. The exposition of the material in the paper needs to be improved considerably and reading of the paper has to be made easier. One of the reviewer made very detailed suggestions how this could be achieved. Like this reviewer, I found the exposition of the MH algorithm on p. 12 particularly obscure, see also Comment 7 below.

2. I agree that the term ,,restricted" is misleading. How about a term such as "Bayesian inference for data with incompletely specified likeilihood", or something similar?

3. I found the discussion how the suggested approach is related to ABC methods very vague and would appreciate more details.

4. There are several references to unpublished material such as Jung et al. (2014) which makes it hard to evaluate the value added by the corresponding material in the present paper.

5. As for ABC methods, the suggested approach heavily relies on choosing appropriate statistics of the data. Since robustness to ,,bad" data is the major concern of the authors, they should pursue one reviewer's advice go beyond M-estimators and consider high-break down estimators such as LMS and LTS. Also the choice of a suitable statistic for the scale is an important issue that should be discussed in more detail.

6. As noted by one referee, the gain in performance of the suggested approach for the two models discussed in the paper is not terribly convincing. Hence, I strongly encourage the authors to include an additional example that shows a significant edge of their method over classical ones, or at least to provide artificial data, where this is the case.

7. One reason for this performance could be that, strictly speaking, the model is not only misspecified for the "bad" part of the data, but also for the "good" part of the data which are count data, rather than genuinely Gaussian. In particular, if these outcomes are small counts (there is not scale on Figure 3, so it is difficult to say), the square root transformation might not be enough to ensure normality of the outcomes in regression (14) and its generalization in Section 4.3. Apart from non-Gaussianity, also the assumption of homoscedasticity of the error term might be misspecified. I wonder, how robust the whole approach is to model misspecification also for the "good" part of the data, since the acceptance rate of the MH algorithm in (7) and (8) seems to be based on the (potentially misspecified) outcome regression.