Reviewer’s report

Title: LEVEL (Logical Explanations & Visualizations of Estimates in Linear mixed models): Recommendations for reporting multilevel data and analyses

Version: 1  Date: 13 Oct 2019

Reviewer: Marco Geraci

Reviewer's report:

I feel that the authors were largely unresponsive to my comments and that there are still strong contradictions.

Specific comments

- Since this "guideline" is not the result of an expert panel or community-wide consensus, these are essentially "recommendations" that reflect the views of the authors. For example, I very much disagree that the "multilevel diagram" is necessary (perhaps recommended when the design is rather complex). Similarly, if a sample size is calculated using a given ICC, then I think a justification is necessary (not simply expected). In other words, Table 4 would be probably different if the discussion was to involve a larger pool of statisticians and mixed model users. I believe that the term "guideline" should be removed altogether from the article.

- The authors cite Casals et al (2014) in response to my objection of lack of evidence on whether these recommendations are needed. That article is indeed in the right direction but not sufficient to compensate for what the authors are supposed to do. First of all, Casals et al's review is now dated (2000-2012). Secondly, the review focuses on *generalized linear mixed models* (GLMM). In their search strategy, they did not include normal linear mixed models, and their discussion is focused on non-normal response distributions (see their Table 3). This means that the sample of studies they analyzed is a subset of (likely much smaller than) all studies based on mixed-effects models. So their results may not be representative of how researchers report the results of *linear mixed models* (LMM). If I were to guess, I would expect that the quality of reporting for LMMs is somewhat better than that depicted by Casals et al for GLMMs with non-normal response because the latter are not as widespread as LMMs and because they carry several complications as compared to LMMs. But we don't know. The authors still need to do a systematic review and provide evidence about their claim than their recommendations are needed, especially because the proposed recommendations focus on LMMs, not GLMMs (see next point).

- The authors replied that "the guidelines in Table 4 are meant to be general, for all GLMM. We chose the LMM for illustration purposes and for its simplicity Discussing the specifics of all GLMM - for binary, Poisson, negative binomial, zero-inflated - would make the manuscript quite long; and the point is to illustrate the general principles of what to report [similarly to STROBE]". This answer is evasive and the parallel with STROBE is unfitting. GLMMs have their specific characteristics. For example,
what would be the recommendation for reporting estimates from a model with probit, logit, or log link? Interpretation is paramount. The authors should remove any reference to GLMMs because their article is not about GLMMs, but (normal) LMMs (even the title says "linear mixed models" and all the formulas provided are for LMMs). For this reason, the example as well should be about normal LMMs.

- The authors show some confusion about link functions. This is rather worrying given that this paper is supposed to be a "guideline" for GLMMs. See next two points.

- As stated in supplementary, the model is supposed to be logistic but the authors wrote "prevalence = b0 + b0i + ... ". Seriously? The prevalence (probability) is NOT equal to the linear predictor in a logistic model. It's the logit of the probability that is equal to the linear predictor, logit(probability) = b0 + b0i + ...

- Because of the confusion about the logit link, I started wondering about what the authors mean by "linear link" in supplementary and on page 5 of the text. In my previous review of this paper, I thought the authors meant the "identity link". But now I realize the authors are calling "linear link function", any link function. Nobody does that because except for the identity link, which is linear and rarely used in GLMMs (as I commented before), link functions in GLMMs are NONLINEAR (e.g., probit, logit, cloglog, log).

- In supplementary the authors write "The ICC is estimated by assuming that errors are logistic with mean 0 and variance pi^2/3". First of all, the assumption is not that the errors are assumed logistic. It's the *latent errors* that are assumed to be logistic (as in the discrete choice model literature). Secondly, the authors do not seem to understand that the reader who is not aware of the nuances of the GLMMs may have no idea of where pi^2/3 comes from. Yet, in Table 4 the authors indicate as "necessary" to report VPCs/ICCs without explaining that in GLMMs with scale parameter fixed to 1, VPCs require a calculation that depends on the link function. So, once again, either the authors give a full, proper account of GLMMs, or remove any reference to GLMMs because, as it stands, this article may generate confusion.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

Are the conclusions drawn adequately supported by the data shown?
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