Reviewer’s report

Title: Bayesian adjustment for measurement error in continuous exposures in an individually matched case-control study

Version: 1 Date: 10 February 2011

Reviewer: annamaria guolo

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Major Compulsory Revision

I would thank the Editor for considering me as a referee for this manuscript.
I found the manuscript interesting in proposing a Bayesian approach to measurement error correction in matched case-control studies. From a theoretical point of view, I found the construction of the approach well described. The Authors review the existing literature on the topic and suggest an approach whose construction is well motivated while taking into account the features of the available data. From a practical viewpoint, the Authors face the difficulties of multiple mismeasured continuous covariates.

My comments and suggestions about the work are mainly related to the Methods and Conclusions Sections.

Methods Section

1. The Authors follow the tradition of the literature in measurement error problems when applying a standard classical measurement error structure, i.e. $W = X + U$, with nondifferential, additive, independent and unbiased errors. While the structure can be justified in many circumstances, I am wondering if the method can accomplish different situations. Among others, a multiplicative error structure, while maintaining nondifferentiability, is known to provide substantial effects on the estimators (bias). Some details, if not a complete simulation study, describing how to extend the method to account for nonclassical structures could be of interest and add a measure of flexibility to the approach.

2. The approach is developed under the assumption of normally distributed exposure variables $X$. This is a common assumption in measurement error literature, although it implies the correction procedures being prone to robustness problems. The topic has been previously investigated in literature, see for example Carroll, Roeder, and Wasserman (1999), Richardson et al. (2002), Guolo (2008), who suggested to alleviate the problem through flexible specifications of the distribution of the unknown $X$s.

The Authors should at least mention the robustness implications of the normality assumptions. With respect to this, some details could be given about the
possibility of increasing the robustness of their approach against misspecifications of the exposure distributions. Specifying flexible distributions of $X$s might be a possibility, but implications from a computational point of view should be discussed.

3. It is my thinking that a reorganization of the section could make the reading easier. I would anticipate the description of the measurement error model and of the disease model, which are common in the measurement error literature, at the beginning of the section. Then, I would insert the novelty of the manuscript, that is focused on the description of the retrospective Bayesian model, followed by the justification of the conditional logistic regression and related details (joint posterior density, ...)

Conclusions Section

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1. In the second paragraph of the Section, the Authors comment Bayesian and maximum likelihood methods, describing their main advantage over alternative approaches, that is the fact that they provide more accurate inferential results. This is true and proved in many papers, also in matched and unmatched case-control studies, especially with respect to regression calibration. Nevertheless, the Authors did not evaluate the performance of their approach with respect to alternatives. I think it would be interesting, and not too computationally demanding, to perform a comparison between the proposed Bayesian solution and alternative correction methods, as for example the regression calibration approach (according to McShane et al. (2001) in the matched setting) and evaluate that, as I suppose, the Bayesian proposal outperforms the competing approach.

2. In the last sentence of the Section, the Authors claim that alternative methods to provide estimation of components of the measurement error structures can be developed. I think this point should be clarified: do the Authors refer to the possibility of having alternative additional information available in form of replicates, instrumental variables, ...?

References

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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.