Author's response to reviews

Title: Social determinants of rest deprivation amongst Ghanaian women: national and urban-rural comparisons with data from a cross-sectional nationally representative survey

Authors:

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Version: 3 Date: 30 April 2010

Author's response to reviews:

This document includes all the comments offered by the reviewers, and following each comment, our response in bold text.

MS: 7165992443471753:

Klerman review:
Reviewer's report
Title: Social determinants of rest deprivation amongst Ghanaian women: urban-rural comparisons with data from a cross-sectional nationally representative survey
Version: 2 Date: 7 April 2010
Reviewer: Elizabeth Klerman
Reviewer's report:
A. Review of “The association of living conditions with rest deprivation amongst Ghandian women: a cross-sectional nationally representative survey” by Mittelmark and Bull.

This manuscript should be combined with “Social determinants of rest deprivation amongst Ghanaian women: urban-rural comparisons with data from a cross-sectional nationally representative survey” by Mittelmark and Bull. The data sets are the same and results are stronger if presented together, especially since there are no clear conclusions.

We agree, and one paper has now been produced, for BMC Public Health.

B. Review of “Social determinants of rest deprivation amongst Ghanaian women:
General:
This is an interesting study that is potentially vital for our understanding of rest and its determinants in women in third world conditions.

Manuscript should be combined with those in “The association of living conditions with rest deprivation amongst Ghanaian women: a cross-sectional nationally representative survey” by Mittelmark and Bull into one report.

1. The question is well defined.

2. The methods have some problems: (1) Why was a cutoff of 6 hrs used? Why was a continuous variable not used? Is this the reason for poor model fit? (2) No statement is made whether correlations among variables were tested or accounted for in the multivariate model.

We have modified the text to explain that the data were collected in categories (top of page 10). The 6 hour cut-off was used because the literature suggests that 7 plus hours is salutogenic while 6 hrs or less is pathogenic.

We believe the reason for the poor model fit is not distributional problems with the rest variable, but due to having a poor selection of social indicators. This is really the main point of the paper – the classical social determinants of health (income/wealth, education, occupation) do not have very much utility in explaining variation in rest deprivation, at least in these data. To save space we have kept the model fit data in Tables 4-6 but cut the discussion of this in the text, since the data in the Tables is self-evident.

Many of the variables in this study are inter-correlated, but we did not include those data to save space, and because the multivariate analyses account for such correlation, assuming there is no problem with multicollinearity. We did test for possible multicollinearity and found no evidence for it. This sentence is inserted on page 12: Intercorrelations for all study variables were examined and the data were examined for possible multicollinearity, but no evidence for that was found.

3. One question about the data: Could the illiterate women “tell time”? Did they have an accurate assessment of time spent in rest.

We don’t know, and on page 13 we have added this text, together with our other comments on limitations of the rest measure: Research is also needed on the validity of self-report measures of rest pattern, and it is a limitation of this study that the validity of the rest measure has not been ascertained.

4. Data deposition is referenced
5. Discussion and conclusion minimize the problem that the models fit the data very poorly. Indeed the prediction accuracy was 0% for one measure. The authors should not draw so many conclusions with such a poor fitting model.

We agree, and our conclusions are mostly about what we failed to observe, that might have been expected based on the literature (a strong relationship between social indicators and health measures). The paper now concludes with this: We conclude with a point of substantial importance; the social indicators that were examined in this study accounted for very little variance in the rest variable, which is clear from the model fit data in Tables 4-6. As mentioned already, we assume that rest patterns do have social determinants, but none of substantial significance was evident among the social variables available in this study. We are convinced that qualitative research in local contexts is needed in order to illuminate the social determinants of rest pattern, and to provide guidance about better ways to measure such determinants in future survey research.

6. Limitations of the work are clearly stated.

7. Prior work is cited.

8. Titles are appropriate. However, the conclusion of the Abstracts overstated results

We have revised the abstract accordingly, and hope the reviewer agrees that we have been as circumspect as our modest findings dictate.

9. Writing is acceptable.

Major Compulsory Revisions

1. Combine the two reports

Done

2. Move the statement about doing multivariate logistic analyses using significant indicators from Results to Methods

Done

3. Methods: Make clearer that did both testing of individual measures and with these measures as part of a model.

We hope that it is now clear that Tables 1-3 provide data in the bivariate relationships and tables 4-6 on the multivariate relationships.

4. Methods: why were All non-Christian in one category? Why was not Muslim a separate category?

We have added this text to page 16: Data on religious affiliations other than the Christian faiths were available, including affiliation with Moslem and traditional
faiths. In preliminary analyses affiliation with either of these was a risk indicator for rest deprivation. This was not pursued as a main point in this paper due the complexity of understanding how cultural indicators such as religious faith are factors in health studies. We wished to avoid the possibility of stigmatisation and chose instead to focus on the protective relationship that Christian faith has with rest deprivation.

5. Methods: Do analysis by continuous (hrs rest) rather than dichotomous (rest vs rest deprivation). Or, provide good rationale why six hours was chosen as a cutoff. Do this for both individual measures and as part of a model.

Dealt with above.

6. Methods: Consider using one model for both the capital and rural areas but with an indicator variable for the location of the women.

We did consider this possibility, but this would only control for urban/rural differences, and not illuminate them, as we wish to do.

7. Methods: What were the correlations between variables? Was adjustment made in the model for them? Were attempts made to redo the model with some of the variables significant individually removed, especially if there were correlations among the variables? How were variables added/subtracted from the model?

The first two questions have been addressed above. We have not addressed the details of the logistic regression data analyses, including the great number of preliminary analyses that were used to explore the data. However, we always included education, occupation and wealth index simultaneously (adjusted for age where indicated), since these are the typical social determinant indicators reported in the literature.

8. Methods: How were sample weights calculated?

The following text is added to page 11: The data were weighted in this way: “There are two main sampling weights in DHS surveys: household weights and individual weights. The household weight for a particular household is the inverse of its household selection probability multiplied by the inverse of the household response rate of its household response rate group. The individual weight of a respondent’s case is the household weight multiplied by the inverse of the individual response rate of her individual response rate group.” [27, page 12].

9. Methods: Speak with statistician about other statistical measures that may enable a better fitting model. If a better fitting model is not obtained, then alter the conclusions.
We have altered the conclusions, as mentioned above, on the last page of the paper.

10. Methods: Could the illiterate women “tell time”? Did they have an accurate assessment of time spent in rest.

Already addressed, above.

11. Minor

1. P8 typo. “.. seven or (not of) more…”

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 declare that I have no competing interests

Daponte-Condina review:

Reviewer’s report

Title: Social determinants of rest deprivation amongst Ghanaian women: urban-rural comparisons with data from a cross-sectional nationally representative survey

Version: 2 Date: 18 April 2010

Reviewer: Antonio Daponte-Codina

Reviewer’s report:

Both papers are based on the same data, and written by the same authors. So, both papers are reviewed jointly.

Both papers present data on the association of rest deprivation and some social indicators, at national level (first paper) and at two regions within Ghana (second paper). Results come from the same dataset, a national representative sample of Ghana. Both papers share data, method, references, background, and also part of the results and discussion. On the other hand, the results, the methodology, the complexity of the analysis, or the findings, discussion, or conclusions, do not warrant the need to convey results in two papers. In fact, one paper should be enough to present the results of this study. Although results are presented as independent (in two papers), they are based on the same subjects and using the same methodology. Moreover, results presented in the manuscript “Social
determinants of rest depletion amongst Ghanaian women: urban-rural…” suggest that there might be a differential effect (effect modification or interaction) of living conditions on rest deprivation in different regions of the country. This may warrant to explore this issue within the objective of the first manuscript.

The papers have now been combined.

A second major issue is if no other information from the national survey on women living conditions, is available for the analysis. Specifically, variables on women child caring activities, and variables that describe working activities in a more detailed form. Results in both manuscripts are not conclusive, and more detailed information could help to reach more definitive conclusions.

There are extensive data on reproductive health history, behaviour, knowledge and attitudes, but not on daily activities of living. Occupational codes are available, which we have used in the occupation variable, but no information is available about occupational activities (e.g., hours worked, number of jobs, position, responsibilities, etc.

Finally, a single paper with the results of this study may be very interesting because there is very little literature on women health an living conditions in southern countries, and because the issue, “rest deprivation and living conditions” among women, are of interest in public health.

We agree, and a single paper is now produced.

So, a Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached) is that a single paper be elaborated to be presented for publication to the journal.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests

MS: 1567642210346238:

Klerman review:

Reviewer’s report
Title: The association of living conditions with rest deprivation amongst Ghanaian women: a cross-sectional nationally representative survey
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B. Review of “Social determinants of rest deprivation amongst Ghanaian women: urban-rural comparisons with data from a cross-sectional nationally representative survey”

General:

This is an interesting study that is potentially vital for our understanding of rest and its determinants in women in third world conditions.

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1. The question is well defined.

2. The methods have some problems: (1) Why was a cutoff of 6 hrs used? Why was a continuous variable not used? Is this the reason for poor model fit? (2) No statement is made whether correlations among variables were tested or accounted for in the multivariate model.

This and all the comments in this review have been addressed in response to the review of the BMC Women’s Health submission, above.
3. One question about the data: Could the illiterate women “tell time”? Did they have an accurate assessment of time spent in rest.
4. Data deposition is referenced
5. Discussion and conclusion minimize the problem that the models fit the data very poorly. Indeed the prediction accuracy was 0% for one measure. The authors should not draw so many conclusions with such a poor fitting model.
6. Limitations of the work are clearly stated.
7. Prior work is cited.
8. Titles are appropriate. However, the conclusion of the Abstracts overstate results.
9. Writing is acceptable.

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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.
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Daponte-Codina review:

Reviewer's report

Title: The association of living conditions with rest deprivation amongst Ghanaian women: a cross-sectional nationally representative survey
Version: 2 Date: 18 April 2010
Reviewer: Antonio Daponte-Codina

Reviewer's report:

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Major Compulsory Revision:

Both papers present data on the association of rest deprivation and some social indicators, at national level (first paper) and at two regions within Ghana (second paper). Results come from the same dataset, a national representative sample of Ghana. Both papers share data, method, references, background, and also part of the results and discussion. On the other hand, the results, the methodology, the complexity of the analysis, or the findings, discussion, or conclusions, do not warrant the need to convey results in two papers. In fact, one paper should be enough to present the results of this study. Although results are presented as independent (in two papers), they are based on the same subjects and using the same methodology. Moreover, results presented in the manuscript “Social determinants of rest deprivation amongst Ghanaian women: urban-rural…” suggest that there might be a differential effect (effect modification or interaction) of living conditions on rest deprivation in different regions of the country. This may warrant to explore this issue within the objective of the first manuscript.

Addressed above, along with both comments under.
A second major issue is if no other information from the national survey on women living conditions is available for the analysis. Specifically, variables on women child caring activities, and variables that describe working activities in a more detailed form. Results in both manuscripts are not conclusive, and more detailed information may help to reach more definitive conclusions.

Finally, a single paper with the results of this study may be very interesting because there is very little literature on women health and living conditions in southern countries, and because the issues, “rest deprivation and living conditions among women”, are of interest in public health.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

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