Author's response to reviews

Title: Intimate partner violence and mental health in Bolivia

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Version: 3 Date: 9 February 2013

Author's response to reviews: see over
Domestic violence and mental health in Bolivia

Reply to reviewer #1

Major Compulsory Revisions

Comment #1: Intro: Although worldwide prevalences are important to note, specific comparisons with Latin America and/or Bolivia would be even more important to include... this is not done in the paragraph on emotional abuse.

Reply: We agree with the reviewer that it would be help to include information on emotional abuse in Latin America and/or Bolivia. However, while there are several studies that examine physical and/or sexual abuse in these regions, we have not been able to find literature that describes emotional abuse in Latin America.

Comment #2: The reviewer pointed out that the last paragraph on page 2 was lacking references with respect to the issue of machismo.

Reply: Because literature is lacking on the connection between the cultural concept of machismo and abuse, and because our data do not permit us to analyze the role of machismo, we have deleted the comment about machismo.

Comment #3: The reviewer recommended that we separate data/population from methods and measures from data analysis. The reviewer also commented that the measures section is very long, and recommended to make it more succinct.

Reply: As suggested by the reviewer, we separated these topics and revised the methods and measures section.

Comment #4: The reviewer enquired what the DV questions based on, and pointed out that the questions seems to be a modified version of the Conflict Tactics Scales.

Reply: The reviewer is correct that the DV module of the DHS is based on the Conflict Tactics Scales. We have clarified this in the text, and have added relevant references.

Comment #5, part 1: The reviewer asked why probit regression was used while logistic regression is the more standard regression technique for cross-sectional data?

Reply: Both probit and logit/logistic link functions are valid methods for modeling the response probability when researchers are faced with a binary outcome represented by a series of ones (yes) and zeroes (no). In any case, estimating the effects of a set of covariates on the response probability of a binary outcome requires specifying a cumulative distribution function. The logit/logistic model assumes a logistic distribution; the probit model assumes a normal cdf. We are aware that logit (logistic) models have often been favored in some disciplines because of their computational ease and because the interpretation of odds ratios tends to be more straightforward, while probit models have been favored (mostly by economists) when there is a strong a priori assumption that the underlying error distribution is normal as opposed to logistic.
Empirically, the response probability curves for both probit and logit models both follow a sigmoid shape and are relatively flat at low probabilities (close to 0) and at high probabilities (close to 1). The principal difference is that the logistic distribution is slightly flatter in these tails. In our data, we do not appear to be in these tails. Hence, differences in the effects using probit and logit models are likely to be extremely small. Further, in this case, we are principally interested in modeling the adjusted proportions of respondents experiencing different symptoms of depression - and not the log odds of depression - for given experiences of domestic violence. For example, we use the probit regression model to show that the adjusted proportion of women who report “feeling tired all the time” is only 49.9% for women who report no abuse but is 66.4% for women who report being victims of sexual abuse. In our professional opinion, this is a more illustrative way to present our results than using log odds.

Comment #5, part 2: The reviewer suggested that the analysis should focus on the association of partner violence with mental health outcomes, and noted that the data on the relationship between the control variables and the mental health variables are irrelevant.

Reply: As requested by the reviewer, we removed data on the relationship between the control variables and mental health variables from the text. However, because some readers may be interested in a more comprehensive view of mental health in Bolivia, these data were retained in the tables.

Comment #5, part 3: The reviewer asked if all of the control variables were used in each analysis or whether the authors used some form of model building.

Reply: Variables were included in models based on underlying theoretical justifications. The control variables in these models are standard in the literature. Stepwise procedures were not used.

Comment #5, part 4: The reviewer asked why we reported on each symptom, and suggested that it may be more relevant (and succinct) to combine symptoms for each major outcome category (depression, anxiety, etc.)

Reply: For several reasons, we have chosen to report on each symptom instead of combining symptoms for each outcome category. First, because there are numerous depressive disorders, which vary in their diagnostic criteria, self-reported symptoms are not sufficient to diagnose “depression” or “anxiety.” We believe that this limitation makes combining each symptom unhelpful, and potentially misleading. In addition, we believe that the reader would lose information from this simplification of categories.

Comment #6: The reviewer pointed out that better labeling of the columns in the results/tables was needed. The reviewer also suggested that we add the standard errors of CIs.

Reply: We improved the labeling of the columns, and added notes to add further clarification. In addition, we added the confidence intervals, as requested by the reviewer.

Comment #7: The reviewer commented that the first paragraph of the discussion section was repetitive of the introduction. The reviewer suggested beginning the discussion with key
findings, to compare with other studies where applicable, and to make the conclusions more specific.

**Reply:** In accordance with the reviewer’s suggestions, we removed the repetitive information from the discussion section, and added comparisons with other studies. As suggested, we also revised the conclusions section.

**Minor Essential Revisions**

Abstract and elsewhere: the data are cross-sectional... cannot assume the “effect of violence on mental health”

**Reply:** The reviewer is correct. We changed the wording accordingly.

Conclusions: abstract and text- DV programs do not treat injuries. They provide services. What programs in developing countries should “screen patients”?

**Reply:** We revised the phrasing to clarify this.

Intro “said to have” is not appropriate when citing studies. Please use “studies have found” or “demonstrated” as an example.

**Reply:** Done.

There are many different types of victims (children, elders, partners, etc.) included in the term domestic violence. Please be specific throughout the manuscript who you are referring to (ie spouses, intimate partners...).

**Reply:** Done. We also changed the revised the manuscript to make it we are focusing on intimate partner violence.

Last para of intro belongs in methods section and is repeated at the end of page 3 as well.

**Reply:** As suggested, this paragraph has been moved to the methods section and has been deleted from the end of page 3.

**Reply to reviewer #2**

**Comment #1:** The context of the study is not entirely clear. I suggest the authors explain their role and the role of their colleagues in Bolivia. What form of cooperation or collaboration exists and how was the study conceived? What sort of research capacity building was accomplished as part of the work?

**Reply:** As noted in the data section, this study is based on data from the Bolivia Demographic and Health Survey. As such, it is an analysis of secondary data. The Demographic Health Surveys are implemented through the MEASURE DHS project, which is funded by the United States Agency for International Development (USAID). The MEASURE DHS project is
implemented by ICF International. All DHS data, including those from the Bolivia DHS, are publicly available at www.measuredhs.com/data. We have added this link in the text.

Since this is an analysis of secondary data, the authors were not involved in the data collection. Data collection was implemented by the Bolivian Ministry of Health and Sports (Ministerio de Salud y Deportes, MSD) and National Institute for Statistics (Instituto Nacional de Estadística, INE) with technical support from ICF International. The authors do not have information on the capacity building that ICF International engaged in, but interested readers can contact ICF at info@measuredhs.com.

Comment #2: The acknowledgements indicate that the study was funded by USAID but the auspice for the study is not stated. The statement on research ethics needs to be made clearer. What organization provided the institutional ethics approval for each country? How does the commercial company ICF International relate to these organizations? I suggest these matters are added to the text of the paper as well as acknowledgements as appropriate.

Reply: We clarified that the data collection was implemented by the Ministry of Health (Ministerio de Salud y Deportes, MSD) and National Institute for Statistics (Instituto Nacional de Estadística, INE), and that the role of ICF International was to provide technical assistance for the implementation of the survey. The statement on research ethics is what was provided to us directly by ICF International, upon our inquiry. Since our study only deals with the Bolivia DHS, we did not enquire about the ethic approval procedures for other DHS countries.

Comment #3: The survey staff received special training. It would be useful to read about how the staff were recruited (local people or people familiar with the culture) and what was the nature of their experience and training. How were local cultures and languages considered? Is there any recorded information about their experiences of conducting the interviews? Where were the interviews held and how was privacy assured?

Reply: Since this is an analysis of secondary data, the authors do not have detailed information about the training beyond what is published in the Bolivia DHS survey report. To the best of our knowledge, there is no published information on the experiences conducting the interviews. Readers interested in this type of information should contact ICF International directly at info@measuredhs.com.

Comment #4: The statistical methods will need review by an expert in the field (not my expertise). How does the pattern of findings of significant associations translate into meaningful differences in morbidity or loss of health? What order of magnitude of difference is found?

Reply: Considering that reviewer #1 did not express any concern about the statistical methods, and considering that one of the authors teaches statistical methods we are confident that the methods are appropriate. We agree with the reviewer that it would be interesting to study how the observed associations translate into differences in morbidity or loss of health. However, since the BDHS was not designed for that purpose the data do not include relevant information on these subjects. Consequently, further examination of the effect on morbidity or loss of health is beyond the scope of this paper.
Comment #5: The reviewer asked if the lack of association between adverse early life events and mental ill health was unexpected, and suggested that we discuss this.

Reply: As noted in the background section, a number of studies have found that physical abuse in childhood as well as witnessing inter-parental abuse increase the risk of experiencing spouse abuse in adulthood. However, none of these studies focus on the association with mental health issues. Since the evidence base is lacking, it is unclear whether a similar effect on mental health should be expected. Hence, we opted to focus on what the evidence shows.