Author's response to reviews

Title: Adverse Childhood Experiences and Frequent Insufficient Sleep in 5 U.S. States, 2009: A Retrospective Cohort Study

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Version: 3 Date: 23 November 2012

Author's response to reviews: see over
November 23, 2012

Dr. Jimmar Dizon
BMC Public Health
Journal Editorial Office
PubMed Central

Dear Dr. Dizon,

Please find attached the revised version of our manuscript, “Adverse Childhood Experiences and Frequent Insufficient Sleep in 5 U.S. States, 2009: A Retrospective Cohort Study” for publication in BMC Public Health. We would like to thank the reviewers for their many helpful comments which vastly improved the quality of this manuscript.

We have attached the reviewers’ comments – along with our responses – which helped us improve the manuscript as directed. These comments immediately follow in this cover letter.

Thank you very much for your suggestions and continued consideration. We look forward to seeing this manuscript published in BMC Public Health.

Sincerely,

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Reviewer's report

Title: Adverse Childhood Experiences and Frequent Insufficient Sleep in 5 U.S. States, 2009

Version: 2 Date: 1 October 2012

Reviewer: Karolin Roeser

Reviewer's report:

Minor issues not for publication

1. The title on the title page differs from the title placed before the background section. Please use the correct title consistently. Thank you for finding this error. We have made the needed change.

2. The authors introduce ACE as an abbreviation in the abstract, 1st paragraph. Please consistently use this abbreviation throughout the manuscript (Abstract, 1st paragraph, 2nd sentence and Background, 5th paragraph). We appreciate your pointing this out and have made the corrections accordingly.

3. The structure of the 1st sentence of the methods section needs to be revised. Revisions made as requested.

Discretionary Revisions

1. It would be helpful if the authors specified “psychological factors” and “traditional risk factors” referred to in the background section, 1st paragraph These factors are now specified explicitly.

2. Please provide the distribution of ACE scores (median, range) and explain why, of all scores, 5 or more were combined into one category (Methods section, Adverse childhood experiences, last paragraph). The rationale underlying this categorization is now described. Thanks for the suggestion.

3. In the results section, please indicate whether or not the study sample was representative of the total population. Thank you for this suggestion. In the Methods section, we note the five U.S. states comprising the study sample and reiterate early in the Results section that the study was comprised of respondents from five U.S. states. We now acknowledge this as a limitation in the Discussion section.
4. In the discussion section, 3rd paragraph, the authors correctly state that sleep disturbance is a criterion for the diagnosis of depression. Moreover, post-traumatic stress disorder is also characterized by sleep problems/nightmares and could possibly result from childhood abuse. The authors may wish to include this aspect in their discussion section. Thank you for this suggestion. Accordingly, there is now a new passage describing the associations between childhood abuse and posttraumatic stress disorder (PTSD), as well as between PTSD and sleep, including relevant references.

5. The authors may wish to comment on the different strengths of associations between single ACEs and insufficient sleep. We were seeking to explain if the relationship between ACEs and frequent insufficient sleep was attributable to residual variation or solely to the ACEs, so we were not examining which ACEs were more important to insufficient sleep. Moreover, all of the individual ACEs were statistically significantly associated with frequent insufficient sleep. While the odds ratio of the association of “parental separation/divorce” is the lowest of all of the ACEs examined, the resulting confidence interval overlaps with that of several of the other ACEs. For these reasons, we prefer not to discuss potential differences in the strength of association between individual ACEs and frequent insufficient sleep. We would welcome any further suggestions if we have misunderstood the Reviewer’s comments.

Minor Essential Revisions

1. Table 2 provides prevalence of insufficient sleep per percentage, but absolute numbers (N) do not reflect frequency of insufficient sleep. We deleted N in Table 2, as per your suggestion, as this had already been listed in Table 1.

Major Compulsory Revisions

1. In the results section of the abstract, the authors state that “All relationships were modestly attenuated by smoking and FMD, but remained significant.” However, in the result section of the manuscript (Mediating effects, last paragraph), it reads “However, frequent insufficient sleep was no longer significant for the associations with an incarcerated household member (AOR=1.23[0.95-1.60]), or parental divorce/separation (AOR=1.14[0.99-1.32]) with the addition of FMD to the model.” Please clarify these inconsistent statements. Thank you for identifying this inconsistency and it has been corrected.
2. In the background section, 1st and 2nd paragraph, the authors describe associations of ACE and negative behavioral and health outcomes in adults. Could the authors provide any findings or hypotheses on the mechanisms underlying these associations? As “ACEs have been demonstrated to be highly interrelated to each other” (Background section, 2nd paragraph), one may speculate that the presence of ACEs might also increase the risk of having adverse experiences after age 18, which was not controlled for in the study. As the survey used in the ACE Study only assessed events occurring during the first 18 years of life, we are unable to examine this. This is now acknowledged as a limitation in the Discussion section.

3. The negative behavioral and health outcomes that the authors name in association with ACEs (substance abuse, depression, medication, somatic conditions, etc.) could contribute to or result from insufficient sleep. Apart from these variables and the covariates assessed, other confounding factors, such as primary sleep disorders, previous sleep problems in childhood, unfavorable living conditions or work schedules might have influenced participants’ sleep. Please explain why smoking was considered a potential mediating factor while other parameters affecting sleep were not accounted for?

It should be discussed that these variables might have contributed to sleep difficulties or mediate the relationship of ACEs and insufficient sleep. The authors might consider excluding individuals with current mental or physical illness and medication/substance use from the sample or controlling for these confounding factors on participants’ sleep. As stated in the introduction, smoking has previously been found to be associated with insufficient sleep. We agree with the Reviewer, but this survey data set does not include assessment of sleep quality or duration.

Likewise, we controlled for frequent mental distress, but this survey data set contained very limited information on medication use and would not permit assessment of current physical illness. Thus, we could not reliably exclude such individuals, although we appreciate the suggestion.

4. Operationalization of the parameters assessed leaves questions unanswered:

- “Not enough rest or sleep” does not unambiguously refer to sleep quality or sleep duration. We agree with the Reviewer, but this survey data set does not include assessment of sleep quality or duration.

- Is there any information available on onset and persistence of insufficient sleep? These would be interesting factors to examine, but this survey data set does not include their assessment.

- Where previous sleep problems in childhood also assessed? No, they were not assessed in this survey data set.
• Why does physical abuse not include spanking? The Physical Abuse items on the ACE questionnaire were derived from the Conflict Tactics Scale, which does not include spanking as part of physical abuse.

• Why is sexual abuse limited to offenders at least 5 years older than the victim? We appreciate this question. Specified age of offender was selected to be consistent with other large national surveys collecting information on sexual abuse.

• FMD is only vaguely operationalized and its definition includes negative behavioral and health outcomes previously listed in association with ACEs (depression, emotional problems). The association of ACEs and FMD could therefore be circular logic. We appreciate the point raised by the Reviewer and we now include it in the study limitations described in the Discussion section.

5. In what way may “assessment of ACEs be useful in the evaluation of sleep insufficiency” (Abstract, conclusions section)? The authors should further elaborate on the implications of their findings in the discussion section. We appreciate the Reviewer drawing out attention to this oversight. Accordingly, we now describe the possible utility of ACEs as potential indicators of sleep insufficiency. We would welcome any further suggestions the Reviewer may have.

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

Reviewer's report

Title: Adverse Childhood Experiences and Frequent Insufficient Sleep in 5 U.S. States, 2009

Version: 2 Date: 5 October 2012

Reviewer: Anna Price

Reviewer's report:
This paper is well written, the logic is clear, and the data are presented simply and show interesting patterns. The paper is lacking an explanation why this investigation is important, a greater consideration of limitations of the measures, and details of sample representativeness so readers can understand
whether/how the results can be generalized.

Major Compulsory Revisions:

1. In the Introduction and Discussion you summarise the existing ACE and sleep research. You suggest that your findings warrant assessment of ACEs in individuals with sleep insufficiency, but do not describe why. What are the clinical and research implications of these findings? E.g., is it that sleep provides an avenue through which these issues can be broached; that an intervention (to sleep, ACEs?) is required etc? Thank you for the suggestion. Based on our findings, we believe that ACEs could serve as potential indicators promoting the investigation of sleep insufficiency. We have clarified this point in the Abstract, Background, and Discussion.

2. Validity of measures:

a. You mention that the sleep measure could be limited but do not explain why. Please describe if/how using the self-report single item question (instead of a diary or objective measure) could affect the results? We appreciate the Reviewer’s comment. We now site research in the limitations section which indicates that, relative to the results obtained by objective methods, sleep difficulties are overreported.

b. Who designed the ACE questions/phrasing and were the answers provided to participants (e.g. never, once, > once)? They are leading questions: are there any data on how the phrasing of these questions changes responses? Thank you for this suggestion. We now provide further detail about how the ACE questionnaire was designed in the Methods section. Regarding the concern you raise about phrasing of the questions, we assume you are referring to the ACE abuse questions, as the other questions were phrased differently. Alternative question responses were provided by the interviewer to the participant, and this is now noted in the Methods section. However, we were unable to find any data addressing how the phrasing used could affect our results. We would welcome the Reviewer’s suggestions regarding any prior research addressing this issue.

c. You mention that the proportions of ACEs could be conservative. Is there any literature investigating whether ACE self-report (e.g. an individual’s disclosure or ability to recall etc) is affected by sleep problems or other covariates?
3. To assess the generalisability of your findings, please describe:

a. The number of participants approached to take part in the BRFSS, the proportion who did, and how these two groups differ. In the five states investigated, cooperation rates – the proportion of all respondents interviewed – ranged from 70.1% (Washington) to 78.8% (Arkansas). This is now explicitly described in the text. As no data were collected on the remaining subjects, we do not have any basis for comparison, but appreciate the suggestion.

b. Comparison data describing how US individuals with landlines differ from those without landlines, so the reader can judge the generalisability of the findings. We thank the Reviewer for this suggestion and now describe characteristics recently identified which distinguish landline users from cell phone only users.

Minor Essential Revisions:

4. Title: Please state the type of study in the title, e.g. retrospective/prospective, cohort/cross-sectional. It is a retrospective cohort study and this is stated in the title. Thanks for the suggestion.

5. Methods: What are the dates for data collection, i.e. were interviews administered each month of 2009 or a selection of months? Data were collected continuously throughout 2009 and were not categorized by month.

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

Reviewer's report

Title: Adverse Childhood Experiences and Frequent Insufficient Sleep in 5 U.S. States, 2009

Version: 2 Date: 22 October 2012

Reviewer: Jon Quach

Reviewer's report:
- Major Compulsory Revisions
- Minor Essential Revisions

1. It would be helpful to include information about the states in which participants were recruited. Either by explicitly stating or referencing. The five states are now explicitly listed throughout the manuscript. In the five states investigated, cooperation rates – the proportion of all respondents interviewed – ranged from 70.1% (Washington) to 78.8% (Arkansas). This is now explicitly described in the text. Thanks for the suggestion.

2. When presenting p-values in the text, can authors please list the actual p-value instead of p<0.05 to enable the reader to make a judgement on the strength of association. We were seeking to explain if the relationship between ACEs and frequent insufficient sleep was attributable to residual variation or solely to the ACEs, so we were not examining which ACEs were more important to insufficient sleep. Moreover, all of the individual ACEs were statistically significantly associated with frequent insufficient sleep. While the odds ratio of the association of “parental separation/divorce” is the lowest of all of the ACEs examined, the resulting confidence interval overlaps with that of several of the other ACEs. For these reasons, we prefer not to discuss potential differences in the strength of association between individual ACEs and frequent insufficient sleep. We would welcome any further suggestions if we have misunderstood the Reviewer’s comments.

- Discretionary Revisions

3. If available, it would be helpful to describe the type of sleep problems these adults are experiencing. For example, are people who experience ACE experiencing more insomnia or nightmare problems? While we agree that this would be helpful information, the current survey data set does not permit its assessment.

4. Is there information about whether the participants felt that their sleep problem was related to their ACE? This might also prove interesting, but the current survey data set did not collect such information.

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