Reviewer's report

Title: Direct and indirect exposure to violence and psychological distress among civil servants in Rio de Janeiro, Brazil: a prospective cohort study

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Reviewer: Ana Flavia P L d'Oliveira

Reviewer's report:

The question posed is original and well defined and the methodology used is adequate to answer it. The methodology is well designed and described. The cohort study design allows the authors to approach the effect of violence exposure on psychological distress in a good time frame between exposure and effect. The use of different forms of exposure (direct, indirect and contextual) and the analysis of individual and contextual levels is also a strength of the study.

The relevant literature is presented. There is a scarcity of well designed studies on the effect of violence exposure in mental health, and this study helps to fill this gap. The writing is clear and the title and abstract are adequate to the work. Most limitations are stated. The comments below are suggestions to improve the text.

Major Compulsory Revisions

Methods

1. I missed more information in ethics. There is nothing about data storage and confidentiality protection besides informed consent. There are recommendations from WHO that we should have extra care in ethics when studying violence, because it is a sensitive issue, and I missed more information on the care with interviewers and interviewees. I would like to know more about if there were referrals of cases of interviewees with severe emotional distress by the researchers, and about training, debriefing and care with the interviewers. It should be an ethical commitment of the researchers to be aware and take care of this. Besides, it is a methodological issue, because training, supervision, debriefing and care may change the quality of the data that researchers can obtain.

2. I also would like to know more about the criteria of division of what was called “area of residence”. The use of multilevel analysis for the contextual level is strength of the study, but Rio is a very heterogeneous territory and the way each “area” is divided can make it more or less homogeneous in the characteristics studied.

3. The authors say, at third paragraph of the section “statistical analysis” Bivariate analysis were carried out in order to identify potential confounders. Therefore, age, sex, income, education, and alcohol consumption were initially considered, and only those with a p-value less than were included in the
multivariate logistic regression models.

But, in fact, both age and income were included in the final models presented in Table 2, in spite of having no statistically significant association in the bivariate analysis. I would like authors to explain this option and make it more coherent along the text.

4. I also would like to problematize alcohol consumption as a confounder, as it may be expression of psychological distress itself. Is alcohol consumption a confounder or is it part of the causal chain determination? I think it is something worth of discussion.

Discussion

5. I would like to see some discussion on the fact that occurrence of psychological distress is associated just with DIV and persistence of psychological distress was associated both with DV and ID but not with DIV. How do the authors explain this result? The exposure to violence in the last 12 months is not mentioned. So we know nothing about “persistence” or “occurrence” of violence in the last 12 months. Would this data be important to explain occurrence or persistence of psychological distress (obviously in relation with 6 years’ previous occurrences)?

6. I also missed a discussion on differences among men and women, framed as gender inequalities. The exposure to violence is usually very different between men and women, if not in general prevalence, in the type and chronicity of violence.

7. The last question is about the definition of violence the paper uses. The way exposure to violence was asked allows for report mainly about community violence, but some domestic violence may be reported as well. The author talks about exposure to violence in general, without specify which kind of violence. But in the discussion they talk about community violence and in the conclusions mention domestic violence. I did not understand quite well what this mention to domestic violence, in the second phrase of the conclusions, means. Does it mean that policy makers should consider this study and also add studies on domestic violence? I think that the lack of data especially on domestic violence (most of which would not be reported with the questions used) should be stated as a limitation of the study (mainly among women), and also that the distinction between domestic, community and general violence should be more clear.

Minor Essential Revisions

8. The sentence bellow is not clear for me. Murders are the leading cause of death in Rio among all causes? It is 33% of external causes? Of all deaths? Please make it more clear.

In Rio de Janeiro, these data seem even more alarming. Murders, the leading cause of death, increased from 33.4% in 1980 to 45.2% at the end of 1988.

9. Where the interviews were applied? At the work place or at the residence of the interviewees?
Discretionary Revisions

10. I suggest considering to include one other paper done in Brazil, Kiss L, Schraiber LB “Gender-based violence and socioeconomic inequalities: does living in more deprived neighbourhoods increase women’s risk of intimate partner violence?” Social Science and Medicine 2012

I understand the paper does not work directly with psychological distress, but it shows, using multilevel analysis, that characteristics of neighbourhood does not affect the prevalence of domestic violence.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**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.