Author's response to reviews

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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Author's response to reviews: see over
Dear Editor,

Thank you for the constructive review of our manuscript entitled ‘Direct and indirect exposure to violence and psychological distress among civil servants in Rio de Janeiro, Brazil: a prospective cohort study’. We appreciate the reviewers’ many positive comments on this manuscript and are pleased to have the opportunity to submit a revised version.

Please, find enclosed the revised manuscript, based on the suggestions made by the reviewers. Detailed responses for the reviewer’s comments are presented below.

Yours sincerely,

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

Reviewer #1. Wagner Ribeiro

1. General comments

This is a study on the association between "direct and indirect exposure to violence and psychological distress among civil servants in Rio de Janeiro, Brazil". The study's making strength is its longitudinal design, which makes it possible to establish a temporal – or cause-effect – relationship between exposure and outcome. Considering that most the evidence on the association between violence and mental health problems comes from cross-sectional surveys, the study helps to overcome the main limitation of such surveys, that is, the reverse causality issue.

On the other hand, the study included a sample of a very specific population, comprised by civil servants working in a university setting. This feature limits considerably the generalizability of the study’s results, as the sample seems to be quite different from the “Brazilian socioeconomic structure”.

Response:

We agree that studies based on occupational cohorts have potential limitations for generalizing their results to the general population, as this selection strategy limits investigation of those extremely poor and the unemployed. However, our study aims at estimating the association between certain exposure variables and an outcome and not at estimating prevalence of the study exposures or outcomes. Therefore, for this matter, high internal validity is the crucial aspect to consider. Likewise, seminal contributions to epidemiology and public health have been provided by studies of specific populations, i.e. not representative of any general population, e.g. British physicians (Doll and Hill’s studies on lung cancer) and civil servants (Whitehall Study on social determinants of health), French civil servants (GAZEL Study), US multidimensional Nurses’ Study, among many others. Moreover, occupational samples are known to minimize losses during follow-up, one of the main problems in longitudinal studies.
In order to address this issue, we expanded the paragraph of the discussion that describes and discusses the limitations of the study. The new text on page 17, lines 378-385 reads as follows:

“Another related issue is that studies based on occupational cohorts have potential limitations for generalizing their results to the general population as this selection strategy limits investigation of those extremely poor and the unemployed. However, our study aims at estimating the association between certain exposure variables and an outcome and not at estimating prevalence of the study exposures or outcomes. Therefore, for this matter, high internal validity is the crucial aspect to consider. Occupational samples, for instance, are known to minimize losses during follow-up, one of the main problems in longitudinal studies.”

2. Discretionary revision

2.1. Regarding the “design and study population”: The article poses clearly its research question, that is, to investigate the effects of individual exposure to and of contextual violence on psychological distress. It also presents a design and procedures that are suitable to address the research questions. Regarding the “design and study population” section, more information on the target population and sampling frame should be provided. It is described that the study is based on “a cohort of non-faculty civil servants” and that “4,030 men and women were enrolled”. However, none information is given regarding the total size of the target population (how many civil servants in total were working at the university at the time the survey was done?). Also, information on sampling procedures is missing (did all university’s civil servants participate on the study? If not, how those who participated were chosen? Were they randomly selected, or did they volunteer?)

Response:
In this study, the target population were 4,614 non-faculty civil servants aged 18–65 years and working at a university in Rio de Janeiro. Except for those transferred to another institution or who were on non-health leave (n=155), all employees were invited to participate, comprising the eligible population for the study (n=4,459
subjects). Thus, no sampling procedures were utilized. In phase 1, 4030 workers participated (90.4% of those eligible).

To address the issue raised, the description of the study population on page 7 was rewritten as follows (lines 149-156):

“The Pró-Saúde Study was set up in 1999 to investigate, in a prospective longitudinal study, the degree and causes of the social gradient in physical and mental morbidity. The target population were 4,614 non-faculty civil servants aged 18–65 years and working at a university in Rio de Janeiro. Except for those transferred to another institution or who were on non-health leave (n=155), all employees were invited to participate, comprising the eligible population for the study (n=4,459 subjects). With a participation rate of 90.4%, 4,030 men and women were enrolled and completed a self-administered questionnaire in 1999 (phase 1).”

2.2. (a) Regarding the “statistical analysis”: it is described that “the effect of contextual violence was assessed with a multilevel model”. More information on the statistical modelling should be provided (i.e., multilevel logistic regression analysis?).

Response:
Thanks for the comment. We included the following information in the text, lines 234-236:

“The effect of contextual violence was assessed by means of multilevel logistic regression models in which the rate of homicides at each weighting area was a second-level variable.”

(b) Also, besides collecting information on exposure to violence in the baseline, did the survey collect such information in the follow-up? If so, I wonder whether analysis should not consider measures of proximal (last year, collected in the follow-up) and distal (baseline) exposure to violence.
Response:
There was a mistake in the first paragraph of the results section that might have given the impression that exposure to violence was also measured in the follow-up, but violence exposure was evaluated only at the baseline. We did the correction and the text (lines 266-267) now reads as follows:

“The prevalence of exposure to violence at the baseline (2001) was 11.3% for DV, 3.8% for IV, and 1.8% for DIV.”

2.3. Regarding the “discussion” section: Considering the Odds Ratios presented in table two, it seems that “indirect violence” had a higher impact on the outcome than “direct violence”, which sounds counter-intuitive, as the literature on traumatic events suggests the opposite. How do authors interpret such results?

Response:
Our findings show a robust association between exposure to both direct and indirect violence and the outcome occurrence of psychological distress. However, the odds ratio for the association between exposure to indirect violence and the outcome persistence of psychological distress was higher than the odds ratio for exposure to direct violence. The literature supports that both indirect and direct violence might, independently, affect mental health [see, for instance, the following references listed below: Krug et al, 2002; Brewer-Smyth et al., 2015; Javdani et al., 2014; Yi et al., 2013; Clark et al., 2008]. What is not known is in which circumstances each different type of violence may be more important. It might be possible that exposure to both direct and indirect violence has a stronger effect on triggering mental health problems (as implied by the 6-fold increase for occurrence), but day-by-day experience of witnessing violence might play a more important role in maintaining such problems. Further studies should be carried out in order to clarify this issue.

The text below was included in the discussion section of the manuscript (lines 327-344). The references 41 through 44 (below) were included in the manuscript, since reference 40 was already cited.
“When we look at the results as a whole, some points draw attention and deserve to be discussed. Our findings show a robust association between exposure to both direct and indirect violence and the occurrence of psychological distress. However, the association between exposure to indirect violence (IV) and persistence of psychological distress was stronger than exposure to direct violence (DV). There is evidence that exposure to both indirect and direct violence may independently affect mental health [40-44]. It is possible that exposure to both direct and indirect violence (DIV) has a stronger effect on triggering mental health problems (as implied by the 6-fold increase in their occurrence), but day-by-day experience of witnessing violence may play a more important role in maintaining such problems. Concerning the lack of association between DIV and “persistence” whilst it was associated with DV and IV, we acknowledge that it is a counter-intuitive finding. Since violence was measured only at the baseline, violence events that may have occurred between 2001 and 2006 were not accounted for. For instance, it is possible that those exposed to DIV at the baseline have not experienced any violent event during the follow-up window. Hence, if persistence of psychological distress needs a continuous exposure to stressful life events, this might explain our findings. Further studies should be carried out in order to clarify these issues.”

References:
Reviewer #2: Ana Flavia P L d'Oliveira

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

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.

Response:
Thank you for your comment. To take care of this issue we provided additional information about ethical issues and included it in the methods section, as follows (lines 246-259):
Ethical considerations

“The research protocol was approved by the Ethics Committee of the Institute of Social Medicine of the State University of Rio de Janeiro, including questionnaires, recruitment protocol, and informed consents. The participation in the research was voluntary and written informed consent was obtained from all participants at all phases of the cohort study and their privacy assured. At all stages of data collection, the questionnaires were identified only by ID numbers, and their connection to the employee's names was confidential.”

Since questioning about exposure to violence in a survey is a sensitive issue, we took an additional series of precautions to minimize any risk for the participants. First, the questionnaire was self-administered, avoiding direct contact between interviewers and participants. Second, no specific questions on intimate partner violence were included in the questionnaire. Third, all field workers were trained to identify physical or mental distress situations and inform participants that, if they need, they could be immediately referred to a University health care facility.”

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.

Response:

In this study, contextual violence was assessed through the geocoding of address of study participants, the use of modified expansion areas and the rates of homicides in these regions in 2005.

To make this clearer, the term “area of residence” was removed, a new paragraph was written, and a new reference was included, as follows (see lines 196-204 in the new version):

“In this study, contextual violence was assessed through the geocoding of residential addresses of study participants and the rates of homicides in 2005 at the
corresponding weighting area. Weighting area is a geographical unity formed by contiguous and mutually exclusive census tracts used to calibrate the sampling weights. Weighting areas representing more homogeneous units, in which census sectors considered subnormal – favela areas – were grouped in specific units, nonetheless representing the boundaries of neighbourhoods and administrative regions in the municipality. This grouping strategy is particularly important for Rio de Janeiro, given the heterogeneity of this city’s social space [31].”


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.

Response:
Thanks for your comment. In fact, the multivariate model was constructed according to the literature on the theme and all covariates were maintained in the model regardless of statistical significance. We rewrote this passage to avoid ambiguity. We changed the text as follows (lines 232-234):

“Based on previous literature, the potential confounders age, sex, income, education, and alcohol consumption were included in the multivariate logistic regression models.”

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.
Response:
In fact, the relationship of alcohol consumption with psychological distress is often bidirectional, and cross-sectional studies have difficulty in establishing the direction of this association. However, in this study, for “incidence of psychological distress”, the temporal sequence of events is clear, since alcohol consumption was assessed retrospectively in the baseline and prevalent cases of psychological stress have been removed from the analysis. We agree that for the analysis of “persistence of psychological distress”, the problem is more complex and we cannot rule out the possibility that alcohol is not a confounder but just an expression of psychological distress. Nevertheless, we chose to keep the consumption of alcohol as a potential confounder in this analysis, since many studies show that alcohol is an independent risk factor for the chronicity of mental disorders, although we cannot rule out the possibility of mental disorders can also lead to initiation and chronicity of alcohol consumption. In addition, this study does not intend to work with the abuse and/or addiction to alcohol, which would make the use of such variables more complex.

Another related issue is that, theoretically, alcohol consumption may be considered both a confounder and an intermediate variable (i.e., in the causal pathway between violence and psychological distress). However, results from multivariate analysis do not support the hypothesis of mediation, since the inclusion of alcohol consumption in the models did not reduce the association between exposure to violence and psychological distress. Conversely, when alcohol consumption is included, the association was strengthened, suggesting that this covariate was a confounder in our study.

In order to make it clearer, we introduced the following paragraphs in the discussion (lines 409-427):

“The option for the inclusion of alcohol consumption as a confounding variable in the analysis should be seen in light of the longitudinal study design of this study. In fact, the relationship of alcohol consumption with psychological distress is often bidirectional, and cross-sectional studies have difficulty in establishing the direction of this association. However, in this study, for “incidence of psychological distress”, the temporal sequence of events is clear, since alcohol consumption was assessed retrospectively in the baseline and prevalent cases of psychological stress have been
removed from the analysis. For the analysis of “persistence of psychological distress”, we cannot rule out the possibility of mental disorders can also lead to initiation and chronicity of alcohol. Nevertheless, we chose to keep the consumption of alcohol as a potential confounding, since many studies show that alcohol is an independent risk factor for the chronicity of mental disorders.

Another related issue is that, theoretically, alcohol consumption might be considered both a confounder and an intermediate variable (i.e., in the causal pathway between violence and psychological distress). However, results from multivariate analysis do not support the hypothesis of mediation, since the inclusion of alcohol consumption in the models did not reduce the effect of violence on psychological distress. Conversely, when alcohol consumption is included, the association increased, suggesting that this covariate is a confounder in this context.”

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)?

Response:

Our findings show a robust association between exposure to both direct and indirect violence and the outcome occurrence of psychological distress. However, the odds ratio for the association between exposure to indirect violence and the outcome persistence of psychological distress was higher than the odds ratio for exposure to direct violence. The literature supports that both indirect and direct violence might, independently, affect mental health [see, for instance, the following references listed below: Krug et al, 2002; Brewer-Smyth et al., 2015; Javdani et al., 2014; Yi et al., 2013; Clark et al., 2008]. What is not known is in which circumstances each different type of violence may be more important. It might be possible that exposure to both direct and indirect violence has a stronger effect on triggering mental health problems (as implied by the 6-fold increase for occurrence), but day-by-day
experience of witnessing violence might play a more important role in maintaining such problems. Concerning the lack of association between DIV and “persistence” whilst it was associated with DV and IV, we acknowledge that it is a counter-intuitive finding. Since violence was measured only at the baseline, violence events that may have occurred between 2001 and 2006 were not accounted for. For instance, it might be that those exposed to DIV at the baseline have not experienced any violent event during the follow-up window. Hence, if persistence of psychological distress needs a continuous exposure of stressful life events it might explain this finding. Further studies should be carried out in order to clarify these issues.

The text below was included in the discussion section of the manuscript (lines 327-344). The references 41 through 44 (below) were included in the manuscript, since reference 40 was already cited.

“When we look at the results as a whole, some points draw attention and deserve to be discussed. Our findings show a robust association between exposure to both direct and indirect violence and the outcome occurrence of psychological distress. However, the odds ratio for the association between exposure to indirect violence (IV) and the outcome persistence of psychological distress was higher than the odds ratio for exposure to direct violence (DV). The literature supports that both indirect and direct violence might, independently, affect mental health [40-44]. What is not known is in which circumstances each different type of violence may be more important. It might be possible that exposure to both direct and indirect violence (DIV) has a stronger effect on triggering mental health problems (as implied by the 6-fold increase for occurrence), but day-by-day experience of witnessing violence might play a more important role in maintaining such problems. Concerning the lack of association between DIV and “persistence” whilst it was associated with DV and IV, we acknowledge that it is a counter-intuitive finding. Since violence was measured only at the baseline, violence events that may have occurred between 2001 and 2006 were not accounted for. For instance, it might be that those exposed to DIV at the baseline have not experienced any violent event during the follow-up window. Hence, if persistence of psychological distress needs a continuous exposure of stressful life events it might explain this finding. Further studies should be carried out in order to clarify these issues.”
References:


In relation to the other point mentioned (exposure to violence in the last 12 months), there was a mistake in the first paragraph of the results section that might have given the impression that exposure to violence was also measured in the follow-up, but violence exposure was evaluated only at the baseline (regarding the last 12 months as specified in the methods section). We did the correction and the text (lines 266-267) now reads as follows:

“The prevalence of exposure to violence at the baseline (2001) was 11.3% for DV, 3.8% for IV, and 1.8% for DIV.”

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.

Response:

We agree with the reviewer in that exposure to violence usually differs among men and women, especially in the type and chronicity of violence. In general, men are
usually more exposed to acute community and women to chronic domestic violence. Unfortunately, as mentioned in our manuscript, our study variables did not distinguish between these two forms of violence.

In order to make this limitation clearer, we added the following to the text (lines 401-405):

“This issue has a close relationship with another limitation of the study, namely the absence of distinction between men’s and women’s exposure to violence. In general, men are usually more exposed to acute community and women to chronic domestic violence [41,43,50]. However, our study variables did not allow us to distinguish these two forms 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 specifically 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.

Response:
Thanks for the comments. We added a paragraph in the discussion section pointing out the lack of data specifically on domestic violence as an important limitation of the study; we also modified the sentence about domestic violence in the conclusion section.

The new text (lines 395-398 and lines 431-437) reads as follows, respectively:

“The measurement of individual’s exposure to violence is another study limitation. Violence exposure was assessed by means of three separate questions, which allowed
the distinction between direct and indirect violence, but could not capture other important features, such as the occurrence of domestic violence.”

**Conclusion**

“In this study, exposure to both direct and indirect violence was associated to the occurrence and persistence of psychological distress. Unfortunately, it was not possible to investigate the effect of different types of direct victimization, like community and domestic violence. Future studies should overcome this gap, including more refined instruments on domestic, community and other types of violence, helping policymakers appreciate the evidence of more complex relationships among different types of violence and their outcomes.”

**Minor Essential Revisions**

8. The sentence below 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 clearer.

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.

**Response:**

*We thank the reviewer for this comment. In fact, the sentence was not clear and has been modified (please, see below the new sentence in lines 68-69). In Rio de Janeiro, homicide is the leading cause of death among external causes.*

“In Rio de Janeiro, these data seem even more alarming. The proportion of homicides among external causes of death increased from 33.4%, in 1980, to 45.2% in 1988.”

**Discretionary Revisions**

10. Where the interviews were applied? At the workplace or at the residence of the interviewees?

**Response:**

*The self-administered questionnaires were applied at the workplace. This information was included in the sentence (lines 163-168) as below:*
“Self-administered questionnaires were completed at the workplace to collect information on socio-demographic characteristics, family history, work environment, history of stressful life events including exposure to violence and discrimination, medical history, self-reported health, psychosocial and lifestyle factors, social network and support, alcohol consumption, and psychological distress.”

11. 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 neighborhoods 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, those characteristics of neighborhood does not affect the prevalence of domestic violence.

Response:

We thank for the suggestion. The reference was included in the revised manuscript.