Author's response to reviews

Title: Resilience to trauma in the two largest cities of Brazil: a cross-sectional study

Authors:

Liliane Vilete (lilianevilete@hotmail.com)
Evandro S F Coutinho (esfcoutinho@ensp.fiocruz.br)
Ivan Figueira (ifigueira@uol.com.br)
Wagner Ribeiro (wagner.ribeiro@me.com)
Maria Ines Quintana (quintana@psiquiatria.epm.br)
Sergio B Andreoli (sergio.b.andreoli@gmail.com)
Jair J Mari (jamari17@gmail.com)

Version: 6 Date: 17 August 2014

Author's response to reviews: see over
Dear Dr. Angela Nickerson,

Thank you for the opportunity to revise our manuscript Resilience to trauma in the two largest cities of Brazil: a cross-sectional study. We changed our text in order to address the problems mentioned. We present below (in red) the comments of the issues raised in your letter.

Best regards,

Liliane Vilete

MS: 1412941410102635
Resilience to trauma in the two largest cities of Brazil: a cross-sectional study
Liliane Vilete, Evandro S F Coutinho, Ivan Figueira, Wagner Ribeiro, Maria Ines Quintana, Sergio B Andreoli and Jair J Mari

Dear Prof Vilete,

Editorial assessment of your manuscript (above) is now complete, and we are delighted, in principle, to accept the manuscript for publication in BMC Psychiatry.

However, before acceptance, the Associate Editor has requested some final changes to be made to the text in your manuscript. These are mainly grammatical changes to improve the readability of the text, given that there have been some difficulties in improving the quality of English so far. We appreciate that English is not your first language and we understand that you have worked with the copy editing service we recommended to improve the text. We hope that you find the further comments from the Associate Editor helpful in improving the English further.

As you will see, they are satisfied overall with your manuscript and the changes that you have made so far. The remaining changes, although it may seem that there are many, are just some minor wording rearrangements and essential clarifications. Please see the comments below:

---
Associate Editor comments:

I have re-reviewed the manuscript "Resilience to trauma in the two largest cities of Brazil: a cross-sectional study. I found it much improved but recommend the following revisions before it can be accepted for publication. In particular, as noted previously, there are some errors of expression remaining. I have suggested corrections to some of these below.

Abstract
1. I suggest the authors remove the sentences "We restricted the independent variables..... predictors of this adaptation". The authors did a good job of explaining this elsewhere, and I don’t think it is necessary in the abstract. They could move some of this explanation to the statistical analysis section.

We removed the sentences.

2. I suggest the authors reword the sentence "Logistic regression models were then fitted for resilience and independent variables to
estimate the incidence density ratio. A more correct wording would be something like this "Logistic regression models predicting resilience were used to estimate the incidence density ratio".

We rewrote the sentence.

3. I suggest the authors reword the sentence "This measure expresses how much the rate of resilience.... In the non-exposed one" so that it reads something like "This measure expresses the extent to which the rate of resilience differs from the exposed group to the non-exposed group".

We rewrote the sentence as suggested.

4. I suggest the authors reword the sentence "Moreover, we explored the relationship of positive/negative..." to read "Moreover, we explored the relationship between positive/negative...."

We changed the word as suggested.

5. The authors should include a statistical test in the abstract to substantiate the statement "There was an inverse linear relationship between childhood violence and resilience".

We included the results.

6. The authors should reword the sentence "Furthermore, resilience might depend on the buffering effect of positive affect against negative affect" to reflect the statistical tests used to evaluate this statement; or they should put this in the conclusions section. The sentence was removed from the abstract.

7. I suggest the authors reword the sentence "This study provides results that help...... in the presence of traumatic experiences" to "This study provides results that help...... following traumatic experiences".

We changed the word as suggested.

Introduction

8. "Empidemiologic" should read "epidemiological" (paragraph 2, page 3)

Thank you! We corrected this mistake.

9. I suggest the authors remove the word "may" from the sentence "PTSD is a syndrome..... hyperarousal that persist for at least a month". This is definitional.

We removed the word "may".

10. Page 3, Paragraph 3, should read "Fortunately, not all trauma-exposed people"

We changed it to trauma-exposed.

11. "Masten states that two judgments" should be re-worded. The authors should reword this to something like "Masten states that two factors" (page 4, paragraph 2).

We reworded the sentence: Masten states that two criteria are required to identify a process of resilience.

12. "There is also little consensus..... defining what positive adjustment means" should be rephrased to state "There is also little consensus.....the definition of positive adjustment".
We rephrased the sentence.

13. There remains a contradiction in the introduction whereby the authors state that resilience should not be defined as the absence of disorder; yet this is how they define resilience in the paper. At the very least, this should be acknowledged and explained. We do agree with the editor's position. For this reason we discussed this issue further in the paper: "Despite these limitations, a higher proportion of "resilient" individuals among participants with the highest scores on items related to well-being was found, suggesting that the criteria for classifying positive adaptation used in this study achieved a degree of convergent validity" (page 19, second paragraph).

14. Page 2, paragraph 5 "In a study with firefighters about job-related traumas" should read something like "In a study of firefighters investigating job-related traumas"
Thank you. We rephrased the sentence.

15. I suggest the authors temper the language of the final sentence in paragraph 2, page 5, referring to childhood abuse, to state that "the continuous exposure to it is one of the most deleterious environmental risks that exists". There are arguably other experiences (i.e., torture) that also represent strong environmental risks. We changed the sentence as suggested.

16. Page 6, paragraph 2. The authors should provide references for the following statements: "Negative emotions focus and narrow thoughts and actions to prepare the body for fight or flight" and "Positive emotions, however may..... resources."
We provided the reference as recommended.

Method
17. The authors should clarify what they mean by "sample size", as when they refer to a sample size of 1,500 interviews in Rio de Janeiro and 3,000 in Sao Paulo it is somewhat confusing. Perhaps they mean initial sample size before refusals/drop-outs?
This refers to the number of individuals provided by the sample size calculation. Due to the refusals, the number of people actually interviewed was 2,159 in São Paulo and 1,072 in Rio de Janeiro. We included the expression estimated sample size to make clear that 1,500 and 3,000 were the numbers obtained from the sample size calculation.

18. Page 9, paragraph 3, the authors should consider rephrasing "PA and NA items..... reflecting proneness to which individuals experience...." to read "PA and NA items are...... reflecting an individual's proneness to experience...."
We rephrased the sentence.

19. Page 10, paragraph 2. I believe the title "exposure variables " is misleading, as this group of variables represent both participant characteristics and exposure variables. I suggest the authors rename this
We renamed it as independent variables.

20. Page 10, paragraph 3. I suggest the authors reword "The following covariates were used in the analyses..... exposure variables" to state "the following covariates were used in the analyses to adjust for the net effects of [participant characteristics] and exposure variables on
resilience"

We rewrote the sentence as: “In order to control for potential confounding effect, the following covariates were included in the regression models...”

21. It is unclear why the authors decided to look at some of the participant characteristics as covariates, and some as potentially important predictor variables. This should be clarified, and a rationale provided. We believe that the reason was stated on page 10: “Because the main objective of this study was to investigate etiological hypotheses from cross-sectional data, and because causal inference cannot be made inter alia in the presence of reverse causal relationships, our independent variables were restricted to events likely to have occurred before the onset of the mental disorder of those receiving a psychiatric diagnosis.”

22. The authors should clarify how they defined "moderate trauma" and "severe trauma" This definition can be found on page 10, last paragraph: “self-perception of intensity of trauma (evaluated with a five-point Likert scale and stratified as 1-3: low/moderate; 4-5: severe).”

23. When considering childhood trauma (none, physical abuse, sexual abuse), how were individuals who had experienced both physical and sexual abuse coded? They were coded as sexual abuse. We tried to clarify this issue: “childhood traumatic event (occurring before 13 years of age; with three categories: 1) ‘childhood trauma’ - with no physical or sexual abuse –; 2) physical abuse – if at least one physical abuse and no sexual abuse; and 3) ‘sexual abuse’ – if at least one sexual abuse).”

Results

24. On page 12, paragraph 1, the authors should specify which "interaction term" they are referring to. We added this information, specifying the interaction term: “The second analysis included the number of different types of trauma, resilience as independent variables and an interaction term (number of different types of trauma X resilience).”

25. On page 12, paragraph 3, the authors should state "Bivariate analyses of the relationship between demographic and psychological variables, and resilient outcomes" rather than "Bivariate analyses of the demographic and psychological variables with resilient outcomes" We rephrased this sentence as suggested.

26. Treating age group and education as categorical variables (Table 2) does not allow any conclusions to be drawn about their relationship to resilience, as the statistical test does not specify which categories are significantly different. Is it possible for the authors to treat these as continuous variables and compare their means between the resilient and non-resilient groups? We believe that this analysis is not in accordance to the type of information provided by table 2. In this table we are presenting the proportion of resilient within the categories of the variables of interest. In case of treating these variables as continuous, we found:
**AGE**

Two-sample t test with equal variances

<table>
<thead>
<tr>
<th>Group</th>
<th>Obs</th>
<th>Mean</th>
<th>Std. Err.</th>
<th>Std. Dev.</th>
<th>[95% Conf. Interval]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Not-resil</td>
<td>1866</td>
<td>38.91586</td>
<td>.3371012</td>
<td>14.56</td>
<td>38.25473 39.577</td>
</tr>
<tr>
<td>Resilient</td>
<td>1365</td>
<td>40.45788</td>
<td>.4368361</td>
<td>16.1</td>
<td>39.09386 40.82182</td>
</tr>
<tr>
<td>combined</td>
<td>3231</td>
<td>39.56732</td>
<td>.268548</td>
<td>15.26</td>
<td>39.04077 40.09386</td>
</tr>
<tr>
<td>diff</td>
<td>-1.542013</td>
<td>.5430783</td>
<td>-2.606826</td>
<td>.4771995</td>
<td></td>
</tr>
</tbody>
</table>

\[ \text{diff} = \text{mean(não-resil)} - \text{mean(resilient)} \]

\[ t = -2.8394 \]

\[ \text{degrees of freedom} = 3229 \]

Ho: diff = 0
Ha: diff < 0
Ha: diff ≠ 0
Ha: diff > 0

Pr(T < t) = 0.0023
Pr(|T| > |t|) = 0.0046
Pr(T > t) = 0.9977

**EDUCATION IN YEARS**

Two-sample t test with equal variances

<table>
<thead>
<tr>
<th>Group</th>
<th>Obs</th>
<th>Mean</th>
<th>Std. Err.</th>
<th>Std. Dev.</th>
<th>[95% Conf. Interval]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Not-resil</td>
<td>1866</td>
<td>8.829582</td>
<td>.0978372</td>
<td>4.226</td>
<td>8.6377 9.021464</td>
</tr>
<tr>
<td>combined</td>
<td>3231</td>
<td>9.013928</td>
<td>.0754542</td>
<td>4.288</td>
<td>8.865985 9.16187</td>
</tr>
<tr>
<td>diff</td>
<td>- .4363521</td>
<td>.1525865</td>
<td>-.7355283</td>
<td>.137159</td>
<td></td>
</tr>
</tbody>
</table>

\[ \text{diff} = \text{mean(não-resil)} - \text{mean(resilient)} \]

\[ t = -2.8597 \]

\[ \text{degrees of freedom} = 3229 \]

Ho: diff = 0
Ha: diff < 0
Ha: diff ≠ 0
Ha: diff > 0

Pr(T < t) = 0.0021
Pr(|T| > |t|) = 0.0043
Pr(T > t) = 0.9979

These data shows the means of age and years of education in each group instead of the proportion of resilient according to the categories of the independent variables and covariates, which is the main purpose of the table 2.

27. The description of the logistic regression needs further clarification (page 13, paragraph 1). Where all variables entered at the same step?

No. Please see page 11, second paragraph: “We entered these variables in the multivariate logistic model using a stepwise strategy. This procedure was not automatic, but was controlled by the authors. The order of entry of the variables was defined based on their p-values. Variables with greater statistical significance (lower p-values) were entered first in the model.”

The authors should reword the sentence "when the four exposure variables were entered" to reflect the fact that these variables were all significant at the univariate level.

We rephrased the sentence as: “When the four independent variables, presenting p-values less than 0.20 in bivariate analysis, were entered one by one in a multivariate logistic regression model, only ethnicity lost its association with resilience”.

I also wasn't clear on the statement that "only ethnicity lost its association with resilience". At a univariate level, wasn't it the
case that only the indigenous vs. other categorization was significant?
Yes, “indigenous” is one of the six categories of the variable “ethnicity” and it was inversely associated with resilience in the univariate (or “bivariate” i.e. ethnicity and resilience) analysis.

If so, was this classification of the ethnicity variable tested in the logistic regression?
Yes, it was tested and, at this level of the analysis (multivariate analysis), the category “indigenous” presented a borderline statistical significance (please see table 3). As mentioned in page 15, 3rd paragraph, “As the number of individuals in this ethnic group was small (n=34), we cannot rule out that this finding was caused by the lack of statistical power. Other studies have found a risk of adverse mental health outcomes in minority ethnic groups, but some have suggested that this risk is more likely to occur in areas where this minority is smaller”

28. The authors state that "The incidence rate among men was 34% higher than among women, and men were still more resilient even after controlling for type of trauma (31% higher)". This suggests that the analyses reported in Table 3 did not control for severity of trauma. Is this the case? If so, were another set of analyses conducted controlling for other variables? This should also be detailed in the statistical analyses section.
No, we do not have an objective measure of severity of trauma. But we think that this was indirectly investigated when we adjusted for “type of trauma”.

29. The authors state that "Those individuals who had childhood trauma were less likely to adapt positively if the trauma was physical abuse, and even less if the major trauma was sexual abuse. In this last case, resilience was reduced to 81%”. Shouldn't this be 19%? Also, what do the authors mean by "the precision of this estimate is not very high"?
No. It is right in the text, as the reduction is calculated as: \[(100 - 19)*100\], that is, 81% of reduction.

30. The authors state that "no interactions were found between parental mental health and any variable related to the traumatic event." What exactly was tested here? Why was this interaction term specifically tested?
We tested if the positive effect of parental mental health on resilience was modified by the presence of repetitive or different types of trauma. This hypothesis was supported by studies as mentioned in the last paragraph of page 16. Our data did not support this idea.

31. In relation to Tables 4 to 7, the regression results should also be mentioned in the text.
In page 14, 2nd paragraph, we present the results obtained from the analyses showed in tables 4-7. These findings were originally presented using graphics (figures 1 and 2). The tables were added in our last version of this manuscript as they were requested by a previous editor just to show the data used to build the figures. So, we don’t think we should expand the description as our main interest is to show the trends instead of the numbers.

Also, the titles of these tables are a bit misleading, as these do not reflect "Average scores", but rather regression analyses.
The beta coefficients provided by the regression analyses showed in tables 4-7 represent the average scores of PA and NA.

32. Further, the explanation of Tables 6 and 7 should be expanded. Please see comment of item 31.

For example, define what "resilient" means.
Thank you. We changed the term to "resilience", in tables 6 and 7. Resilience was defined in page 8, item 1 of Measures and Covariates Section.

33. Paragraph 1, page 14, the authors should state "As some protective effects, ...... we investigated" rather than "we tried to investigate".
We reworded the sentence as suggested.

Discussion

34. I suggest the authors reword the first sentence to state "Our findings support the hypotheses that resilience may be influenced..." We rephrased the sentence as suggested.

35. Page 16, paragraph 2, the authors should state "The occurrence of positive adaptation to a traumatic event increased by 48%"
Thank you. We reword the sentence.

36. The authors should clarify the first sentence in paragraph 2, page 17. Perhaps they mean "when the child may develop the confidence that he or she will be able to manage problems in the environment". The text is correct, and it is in accordance to what is stated by the authors of reference 55.

37. On page 18, paragraph 3, I suggest the authors reword the sentence to state "Our results also suggest that resilience may be influenced by the amount of positive..."
Thank you. We reword the sentence.

38. There appears to be a word missing on page 19, paragraph 3: "Although focus" should presumably read "Although our focus"
Thank you. We corrected it.

39. I suggest the authors replace the phrase "child's disease" with "child's disorder".
We reword the sentence as suggested.

---