Author's response to reviews

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

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Version: 3 Date: 21 February 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

EDITOR'S COMMENTS:

1. While the language in this manuscript is improved, there are still numerous expression errors that make it difficult to follow. Please seek further assistance with proof-reading this manuscript. Examples of some of these errors are below:

   > * "Also, Bonanno [15], reports too often operationalize resilience as the absence of PTSD symptoms or the presence of only a symptom of this disorder" - I'm not sure what this means
   > * "Nevertheless, some authors criticize this operationalization, reminding that resilience is more than the absence of PTSD, just as mental health is more than the absence of a mental disorder" - "reminding" is not the correct term here; perhaps use a term like "asserting"
   > * "They argue that it is inadequate to consider absence of PTSD as evidence of resilience, just as it would be difficult to interpret absence of fever (accurately measured using a thermometer) as evidence of good health in individuals who may well have other symptoms of a disease that cannot be measured by a thermometer" - should read "consider the absence"
   > * "Although numerous scales have been developed to investigate resilience, it is not clear whether a resiliency scale (based just on individuals, or also in familiar and social attributes) can truly measure improvement in resilience (e.g. good adaptation after a threat) [14]." - Do the authors mean "familial"?
   > * "Thus, rather than a vulnerability factor for adverse posttraumatic sequelae, trauma exposure may represent a protection, if it is associated with an increased sense of mastery or growth Thus, may enhance resilience to further adversities [24]." "protection" is not the correct term - do the authors mean "protective factor"

The manuscript was re-edited by EDANZ.

2. The authors expanded their background information on resilience well. They devote a considerable amount of space to the argument that resilience is NOT just the absence of psychopathology (as is appropriate) - however, resilience IS defined as the absence of psychopathology in this study. The authors should put some thought into how to reconcile this. In their response to reviewers, the authors stated that they considered "problems that cause clinically significant distress or impairment in social, academic, occupational, or other important areas of life" in their definition of resilience. While these areas are considered in other variables
included in the study, they were not considered in the definition of resilience, which was purely related to diagnostic category

We included further information clarifying these topics in the two last paragraphs on the item "Resilience" in section “Measures and Covariates”

> 3. There are some sections of the manuscript that require referencing (e.g., page 3, paragraph 3 - "there are distinct and complementary adaptive functions and physiological effects associated with positive or negative emotions".

In this particular case, the reference is [24]. All references were inserted in the text after the complete idea of the author was concluded.

> 4. The authors should provide more information in the method regarding which phobic and anxiety disorders and depressive disorders were considered to represent psychopathology in this study. I also wasn’t sure what paragraph 3 on page 6 meant "we also tried to avoid classifying someone erroneously....". Could the authors clarify?

1) All the phobic (social and specific), anxiety (panic, agoraphobia, OCD, PTSD, GAD) and depressive disorders (episodic, recurrent, dysthymia) not secondary to a medical condition or a substance use.

2) We included this information in the text.

> 5. In the method, the authors refer to the PANAS as measuring positive activation and negative activation - then continue to refer to affect throughout the paper. I would suggest either removing this argument, or making the language more consistent.

We removed the argument, as suggested.

> 6. The authors should specify if sum scores were used for the PANAS and the SUBI in this study..

For PANAS we added the items separately for positive and negative affect sub-scales. We included this information in the text.

For the well-being items extracted from SUBI, we did not produced a score but, instead, we presented the % of resilient participants for the three categories of each item of well-being (very much, to some extend and not very much).

> 7. The statistical analysis section would benefit from being restructured in the order in which the analyses were conducted. For the logistic regression analyses, it was not clear (a) which
variables were initially entered into the model before it was decided which to retain, (b) whether the model was conducted in a step-wise fashion, and at what levels the variables were entered.

In the statistical analysis section we wrote that: “Variables with $p$-values less than 0.20 were initially selected for inclusion in multivariate models”. Table 2 presents these variables.

We entered these variables in the multivariate logistic model using a stepwise procedure. This procedure was not automatic, but controlled by the authors. The order of entry of the variables was defined based on their $p$-values. Those with greater statistical significance (lower $p$-values) were entered first in the model. We included this information in the manuscript.

> 8. The explanation of the linear regression analyses was unclear. How many analyses were done? Were they just linear regression analyses or were t-tests also conducted? Which variables were included as IVs and DVs in the regression models? Similarly, the reporting of these analyses was inadequate. Please provide standard deviations for the means reported. Also, there needs to be greater information reported on the linear regressions - what were the $R$/ $R^2$ squared, $B$ and beta weights for each IV? This should either be reported in text or be made into a table. As it stands, there are currently no data reported on these analyses.

Concerning the number of analyses, and dependent and independent variables

In the last paragraph of statistical analysis section it is written that “To explore the role of positive and negative affect on resilience, we conducted two additional analyses including either PA or NA variables as dependent variables in linear regression models. The first one included two independent variables related to trauma...”

We changed the end of the same paragraph to avoid ambiguity:

“The second analysis included the number of different types of trauma, resilience and an interaction term including both factors as independent variables”.

We included the requested tables (4-7).

> 9. In relation to the graphs, it is unclear how the authors calculated the average scores for positive and negative affect, controlling for multiple variables. It is also inadequate to just include graphs without any numbers relating to the means that were calculated. Also, it would be useful if group differences could be tested for significance.

The means were calculated using the coefficients provided in the tables 4 to 7, included in this new version. For example: for PANAS positive the mean for the category “no trauma” is the constant $\beta_0$ of the model. To get the mean for the category “moderate trauma without resilience”
we added to the constant ($\beta_0 = 27.24$) the value of the $\beta$ coefficient obtained in the regression model for this category (1.02) and so on. P values are presented in the new tables.

> 10. The discussion is clear and reads well - but the authors should take care when intimating causality - e.g. "suggesting a possible causal association", "PA seems to function as a resilience factor, as a moderator factor that is activated by trauma and buffers NA". As the authors have noted, this is a cross-sectional study that does not warrant conclusions about causality.

We do agree that sectional studies have limitations for causal inference. Nevertheless, we limited our analysis to variables less affected to reverse causality as stated in the methods’ section. Moreover, we believed that we were careful when we referred to the observed associations by the use of expressions like “suggesting possible”, “seems to” to avoid strong statements. We also highlighted this issue in the last paragraph of the discussion section: “Causal inference in cross-sectional design can be affected by reverse causality…”

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> EDITOR'S REQUEST:
> 1. Title page: please include a title page at the front of your manuscript file. It should contain, at minimum, the names, institutions, countries and email addresses of all authors, and the full postal address of the submitting author.

We included the title page.

> 2. Tables: please place your tables in the main manuscript file after the figure legends and references. You should remove the copy from the additional files / figure files.

Thank you. We fixed this problem.

> 3. Competing interests: Please include a ‘Competing interests’ section between the Conclusions and Authors’ contributions. If there are none to declare, please write ‘The authors declare that they have no competing interests’. Please consider the following questions and include a declaration of competing interests in your manuscript:

> Financial competing interests

> ? In the past five years have you received reimbursements, fees, funding, or salary from an organization that may in any way gain or lose financially from the publication of this manuscript, either now or in the future? Is such an organization financing this manuscript (including the article-processing charge)? If so, please specify.
> ? Do you hold any stocks or shares in an organization that may in any way gain or lose financially from the publication of this manuscript, either now or in the future? If so, please specify.
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> ? Do you have any other financial competing interests? If so, please specify.
> Non-financial competing interests
> ? Are there any non-financial competing interests (political, personal, religious, ideological, academic, intellectual, commercial or any other) to declare in relation to this manuscript? If so, please specify.

We included this statement in the text.

> 4. Requesting copy editing:

> After reading through your manuscript, we feel that the quality of written English needs to be improved before the manuscript can be considered further.

> We advise you to seek the assistance of a fluent English speaking colleague, or to have a professional editing service correct your language. Please ensure that particular attention is paid to the abstract.

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The last version we sent had already been edited by EDANZ as you suggested. We sent you your comments to EDANZ in order to deal with this problem, and another edition by EDANZ was made.

> We would be grateful if you could address the comments in a revised manuscript and provide a cover letter giving a point-by-point response to the concerns.

> Please also ensure that your revised manuscript conforms to the journal style (http://www.biomedcentral.com/info/ifora/medicine_journals). It is important that your files are correctly formatted.
We look forward to receiving your revised manuscript by 11 January 2014. If you imagine that it will take longer to prepare please give us some estimate of when we can expect it.

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Please don't hesitate to contact me if you have any problems or questions regarding your manuscript.

With best wishes,

Mr. Carlo Rye Chua
on behalf of Dr. Angela Nickerson