Author's response to reviews

Title: The role of intimate partner violence and other health-related social factors on postpartum common mental disorders: A survey-based structural equation modeling analysis

Authors:

Michael E Reichenheim (michael@ims.uerj.br)
Claudia L de Moraes (clmoraes@ims.uerj.br)
Claudia S Lopes (cslopesims@gmail.com)
Gustavo Lobato (lobato@iff.fiocruz.br)

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Author's response to reviews: see over
April 8, 2014

Dear Editors,

We would like to thank both reviewers for their thorough assessments and BMC-PH for the opportunity of re-submitting our paper. As requested, below are our point-by-point responses to the reviewers’ concerns. Since many changes have been made (as requested), for consultations, we are submitting a marked (in blue) version of the revised paper along with the clean version.

We look forward hearing from you.

Kind regards,

Michael E. Reichenheim
(p/ Claudia L. Moraes, Claudia de S. Lopes and Gustavo Lobato)
Reply to reviewers’ comments

1. Reviewer #1 (Olufunmilayo Fawole)

1.1 STUDY OVERVIEW:

Thank you for asking me to review the above named manuscript. This study examined the study explored how socio-economic position, maternal age, household and marital arrangements, general stressors, alcohol misuse and illicit drug abuse, and especially psychological and physical IPV lead to postpartum common mental disorder (CMD). The study was carried out in five primary health care units of Rio de Janeiro, Brazil, and included 810 randomly selected mothers of children up to five postpartum months waiting for pediatric visits. The postulated pathways between exposures and outcome were based on literature evidence and were further examined using structural equation models. The paper is very well written and adds to the body of knowledge on intimate partner violence.

The following comments will help to improve the quality of the work.

1.2 Methods section:

1.2.1 The authors should provide some information on the health care system in Brazil and the study setting- the primary health care centres.

This information is provided in the revised version (page 6, paragraph 2).

1.2.2 There is also no information on the sampling technique used in the selection of the five primary health care centres and the assumptions on the selection of sample size.

Regarding the selection of the five primary health care centers, this information is now provided in the revised version (page 6, paragraph 2).

A prior sample size calculation was not performed since it is very difficult to project appropriate estimates (and covariances) for complex structural equation models using mediators and different variable distributions.

For a hint on the adequacy of our sample’s power, we carried out a full-fledged post hoc power study in Mplus v7.11 (using Monte Carlo simulation) for the untrimmed ‘propositional’ model shown in Figure 1. The power study is a projection for n=810 (our effective sample size), given the parameters we have effectively estimated (regression coefficients and residual variances). The simulation specified 10,000 replications, that is, the number of samples drawn from the specified population.

The table below (next page) shows part of the output. Column 1 and 2 provide the population parameter values specified —taken from the actual analysis of the ‘propositional’ model—and the average of the parameter estimates across the replications. There is close agreement.
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Note: values up to the mid-line relate to regression coefficients and residual variances thereafter.
Column 3 shows the proportion of replications for which the 95% confidence interval contains the population parameter value. The average is 0.948 (s.d. 0.0049), indicating that the parameters are fairly well estimated.

Finally and most importantly are the results shown in columns 4 and 5 of the above table. The focus is on the 38 significant p-values (< 0.05) obtained in the actual data analysis and shown in green in column 5. The related values in column 4 show that in 95% of the cases (36/38), the proportion of replication where H=0 is rejected at the 0.05 level is above 0.80 (also shown in green). Since these are the ‘correct’ probabilities of rejecting the null hypothesis when it is false, one may conclude that the power to reject that the model’s coefficient and variance estimations are zero exceeds the standard of 0.8 in most cases. In fact, the power of the study is higher than 0.8 since the majority of these proportions are above 0.90, tending to 1.0 in many cases. The average of all 38 ‘proportions rejecting H₀’ is 0.952 (s.d., 0.105). In addition, as expected, this ‘rejection average’ drops to 0.325 (s.d. 0.267) in relation to all 14 non-significant p-value found in the underlying analysis (values colored purple).

Note that these figures relate to the more complex ‘propositional’ model; coverage/power for the ensuing ‘trimmed’ models with less estimated parameters, including the ‘simplest’ model shown in Figure 2, would be even higher.

We have opted not touch on this power analysis in the paper for, as may be realized, this would entail expanding the text substantially in order to provide some insight of what was actually done. In any case, the reader may always have access to this information through the pre-publication history if s/he so wishes. We hope our decision is acceptable.

1.2.3 Who did the data collection? There is need to provide information on the data collection process and selection of participants.

This information is provided in the revised version (Page 6, paragraph 3).

1.2.4 References: The number of references is rather much, and can be reduced.

In our view, most of the cited references were necessary as a backup for building the propositional theoretical model underlying the statistical analyzes. Please note that analyses based on a structural equation models require strong theoretical models, which need as many ‘strong’ references as possible.

Please also note that the second reviewer, on the contrary, suggested the inclusion of additional references (see below), which is a request we agreed to comply with in the revised version of the manuscript.

2. Reviewer #2 (Karin Rhodes)

2.1 STUDY OVERVIEW:

2.1.1 This study attempts to address the gap in knowledge about how intimate partner violence (IPV) and “other psychosocial events” are specifically connected to postpartum common mental health disorders. It attempts to establish a framework that maps those relationships.
2.2 TITLE:

2.2.1 Change the words “life events” to “risks”. The study refers to “psychosocial life events” but addresses socio-economic position, number of children, relationship status, and other health-related social factors - which are not an “event” per se. I think “risks” or “health-related social factors” would be more accurate than “events”?

We agree with the reviewer’s suggestion. The title was modified, and the term “psychosocial life events” was avoided in the revised version of the manuscript.

The new title now reads:

“The role of intimate partner violence and other health-related social factors on postpartum common mental disorders: A survey-based structural equation modeling analysis”.

2.3 ABSTRACT:

2.3.1 Stating in results that “most events showed indirect relationships with postpartum CMD” is confusing, since you go on to list multiple variables that you detected direct pathways for. It might be less confusing to start with mentioning the variables with direct pathways to postpartum CMD before listing variables with indirect relationships.

The Abstract (Results) was modified in the revised version.

2.3.2 There should be consistency and/or clarity about findings of “relationships” vs. “pathways”, especially when considering that there is extensive literature about the existence of correlated risk factors. As you say in the abstract background, “literature lacks evidence about how these are effectively connected.” With addressing that gap in research knowledge being the primary aim, results reporting should be clearly connected to attempts to establish paths of connectivity. I would definitely stay away from any mention of causality.

Although we do not mention causality in our abstract, we are ‘forced’ to imply a causal framework since the modeling procedure and its underlying assumptions require putative ‘causal’ links by design [1-5]. Of course, it would be totally unwarranted to assert that our final model is the causal structure ‘flowing’ from macro-level determinants to the health outcome in focus. Yet, using SEM/DAG tacitly requires the notion of an explicative framework and thus our final model must suggest something along these lines even if just propositional and still as a provisional and potentially refutable instance to be ameliorated and refined.

Nevertheless, we concluded it would be wise take up the advice to refrain from explicitly mentioning causality —a term often understood and perceived in different (and sometimes conflicting) ways. There were three instances where the term ‘causal’ appeared in v1 and that have been now modified/omitted in v2.

- Modification on page 5, paragraph 2, citing Kendler (inter alia): “Within causal Along the pathways leading to mental disorders among women, some studies show …”
- Suppression on page 15, paragraph 1 (top): “… to the best of the authors’ knowledge, the present findings are the first to address this interconnectedness within a plausible and broad causal framework.
- Suppression on page 17, paragraph 2, which has been modified at the request addressed in § 2.4.9.
We hope that this decision meets the demands of the reviewer.

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2.4 BACKGROUND:

2.4.1 In terms of prior research in this area, it should be noted that some studies have addressed the concept of causal pathways in a variety of contexts that could be relevant and should be considered. [For example: Eynav Elgavish Accortt, Marlene P. Freeman, and John J.B. Allen. Women and Major Depressive Disorder: Clinical Perspectives on Causal Pathways. Journal of Women's Health. December 2008, 17(10): 1583-1590.]

Thank you for indicating this interesting reference. We have now included it in the revised version (page 4, paragraph 3).

2.4.2 You state that in addition to other psychosocial events, you are “especially” interested in psychological and physical IPV leading to postpartum CMD. If you’re exploring an overall framework for the relationship of multiple factors acting on CMD, perhaps provide more of a rationale for why you chose to emphasize IPV and psychological and physical IPV?

In accordance to the proposed title (singling out “intimate partner violence” from “other […] factors”), we believe that the Introduction progresses (as of paragraph 4) in such a way as to consistently provide prior evidence centered on links between the ‘other factors’ themselves, and above all, how these somehow connect to IPV.

Following a ‘lead’ from the last passage in paragraph 4 (“...and features related to the prevailing marital relationship [Kendler et al.(2002)]”, the ensuing paragraph: (1) touched on the issue of social support and IPV; (2) how “early (youth/adolescent) pregnancy, unstable relationships, and the burden resulting from several young offspring to care for” are regarded not only risk factors for depression and anxiety, but also for IPV; and (3) how “along the pathways leading to mental disorders among women, some studies show that the effect of alcohol misuse or illicit drug abuse are at least partially mediated by IPV”.

Next, paragraph 5 focuses on IPV even more starkly. It suggests evidence from “a substantial literature focusing chiefly on violence between couples as a risk factor to CMD among women”, while pointing out several caveats and gaps requiring some light (and thus upholding the paper’s analytical gist).

Finally, once pointing out (1) “that research on the relationship between IPV and women’s poor mental health following childbirth still falls short on providing a comprehensive account” … since (2) little is still known regarding “the pathways by which [several other factors] and IPV relate to each other, and in tandem, to the lack of psychological well-being
in the postpartum period”, and (3) “that the focus has mostly been on specific relationships, failing therefore to offer a more inclusive and organic perspective”, the last paragraph of the section announces that the aim of the paper is to redress this/these gap(s), investigating how IPV during pregnancy leads to CMD among women in the postpartum period, within a complex pathway connected to ‘other factors’ as well.

Admittedly, this last argument —the actual focus of the paper— was somehow understated (or rather, turbid) in this last passage. We therefore opted to rephrase it thus (page 6 paragraph 1):

“In an attempt to redress this gap, the present study focuses on how IPV during pregnancy relates to CMD among women in the postpartum period, amid other biopsychosocial and health-related social covariates forming a complex interconnected framework. [The analysis …]”

2.4.3 “Common mental disorders” (“CMD”) itself requires a much more extensive description, since it is your outcome of interest. For example, what are the exact criteria for CMD? Is it a validated classification for a group of diagnosable disorders? Where does it come from and which disorders are included? (The study mentions “depression, anxiety, and some somatic complaints”—more specificity is needed. What is the rationale for grouping certain disorders together and excluding others? What are the “somatic complaints”?)

The term common mental disorder (or psychological distress) was built from the recognition that the most common presentation of psychological problems reported by the patients attended in the primary care does not fulfill criteria for a specific category in the existing mental health classifications, but represent an important burden for individual’s quality of life. These patients present a mixed of anxiety and depressive symptoms, with many associated somatic complaints. In epidemiological studies, they are all included in the CMD group, being detected by questionnaires such the SRQ-20 or the GHQ (General Health Questionnaire). It should be pointed out that the SRQ-20 has been used and cited in quite a number of studies carried out worldwide, including studies published in BMC Public Health.

We addressed the requested issues thus:

a) By providing more information throughout the Background section regarding the ‘origins’, the burden and the prevalence of CMD in population-based studies.

b) As to “which disorders are included” and “what is the rationale for grouping certain disorders together and excluding others”, the answer lies, on one side, on the very purpose of identifying frequent complaints (supposedly undetected outside the psychiatric care services), and on the other, on the set of symptoms that best sustained validity and reliability of the instruments developed to tap CMD (since the 1980’s when the concept of CMD was first suggested in the literature). In this respect and addressing the Reviewer’s concerns, we now offer a brief psychometric background of the SRQ-20 (page 10, paragraph 2), not only citing important international references, but also (and emphasizing) those concerning the Brazilian version.

c) Regarding ‘more specific’ information on “depression, anxiety, and some somatic complaints”, we opted to provide a description in the Methods section (page 10, paragraph 1) of the items actually comprising the SRQ-20 (evidently, other symptoms tapping CMD have been used in different instruments, but we feel they are outside the scope of the paper). Yet, we decided not to show another table with all 20 items, but to provide some examples. Note, however, that several references are provided in this and the previous paragraph wherein the reader will be able to see the entire set, including the actual wording.
We hope our choices are acceptable.

2.4.4 This study relies on previous work done on an international scale ("consistent evidence across different cultures and countries") but works with a very specific Brazilian population. The implications of the specificity of the sample should be discussed when being held up alongside international work.

One of the references used to support this statement (reference 17: Robertson et al. – page 4, paragraph 4) actually relates to a literature review, which suggests that these variables have a common representation in most contexts. Although we use a specific sample, we do believe that, on the assumption that internal validity holds, our findings pertain to an inferential domain interchangeable to others of similar contours. We cover this issue in the Methods section (‘Participants’, page 6, paragraph 2) and the Discussion (‘Strengths and limitations’, page 18, paragraph 2; and ‘Implications and future directions’, page 18, paragraph 3).

While debating ‘strengths and limitations’, before engaging on a debate about possible selection bias, we explicitly defend the external validity of our results conveying that "In view of the subjects’ characteristics, current findings seem applicable to populations commonly assisted in public primary health care units in Brazil and, possibly, other settings (countries) presenting similar socio-economic and cultural profiles". We based this statement on the belief that, to the best of our knowledge (including our readings of Robertson et al.), there is nothing sustaining that there might be some modification effect by domain, and thus that our findings should differ due to some facet (social, cultural, etc.) particular to our country. Yet, when assessing the ‘implications and future directions’, we do acknowledge the provisionality of our stance and caution the reader that our findings required corroboration in further studies. To reinforce this point, we added a sentence to paragraph 3 of page 18 (see § 2.7.2 as well).

We hope this is suitable.

2.4.5 The argument in favor of including both psychological and physical IPV is an asset to the study and the rationale is well argued, as is the acknowledgement of the difficulty navigating the nuances of considering the two forms separately and together. However, I think you do need to address why you did not include sexual IPV – after all these women got pregnant and there is an extensive literature around unintended pregnancies in IPV.

We considered studying sexual abuse as well, but eventually opted to leave it out of the present project/study. In retrospect, this may not have been the best of choices, because not only sexual IPV is an important issue per se, but also because its absence may have introduced some residual confounding to the exposure-outcome relations of interest (involving psychological/physical IPV and CMD). We now acknowledge this shortcoming in the ‘strength and limitations’ (sub)section of version 2 (page 17, paragraph 5 / page 18, paragraph 1).

(As a side note, acknowledging the importance of sexual IPV within the context of women’s mental disorders as a whole, we have since then engaged a new study that uses post-traumatic stress as the main outcome).

2.4.6 Paragraph 2: Clarify the relationship between gender differences and age differences in CMD prevalence.

An explanation regarding the influence of gender and age on CMD rates was expanded in the revised version (page 4, paragraph 2).
Paragraph 6: You state there is “not much evidence on the role played by escalating acts of psychological and physical partner abuse on CMD following childbirth”… however, this study does not address the gap in knowledge about the effects of IPV escalation, so I do not think this statement adds to the paper.

The referred to passage formerly read (in v1):

“Yet, there is not much evidence on the role played by escalating acts of psychological and physical partner abuse on CMD following childbirth. Some nuances of these intricate relationships remain unclear, as both types of IPV and maternal mental health have been commonly modeled through arbitrarily defined categorical or dichotomous variables. Furthermore, psychological and physical abuse have generally been treated separately in epidemiologic studies, therefore hindering a better understanding of how these two dimensions effectively associate amid a complex system holding other important events.”

On one hand, we still believe that (1) modelling IPV through arbitrarily defined categorical or dichotomous variables and (2) treating psychological and physical abuse in separate epidemiologic studies may have hindered shedding light on ‘escalation’, and that our methodological decisions resulting from this criticism (to use the full IPV scores thus enhancing scalability; and model both types in tandem thus striving for connection pathways) may have attempted to addressed the gap in knowledge about the effects of IPV escalation.

On the other hand, we appreciate that the link between these ideas are not easily understandable without further explanations, as became clear from the reviewer’s difficulty to make the necessary links. We therefore opted to simplify matters and maintain only the second argument in our introduction. This does not only seem less intricate —informing that that most (if not all) epidemiological studies have been treating psychological and physical abuse separately is easily graspable—, but is directly linked (as forewords) to a passage in the Discussion when we resumed this debate in the light of the corroborated path from psychological IPV to physical IPV (i.e., psychological ‘escalating’ to physical violence) (on page 15, paragraph 3).

The passage in the Background section of v2 now reads:

“Some nuances of these intricate relationships remain unclear, as psychological and physical abuse have been generally treated separately in epidemiologic studies, therefore hindering a better understanding of how these two dimensions effectively associate amid a complex system holding other important events.”

With a view to connecting this ‘introduction’ to the remark in the Discussion, we thus ask to keep the passage in the Background section, albeit modified.

Finally, since this was a study that built on the enrollment criteria used for other studies, give more background about the how and why of the broader project – and include references to those studies or reports, if they have been published.

This particular study (paper) arises from a broader Project, which also attempted to tap into breast-feeding patterns and how biopsychosocial and health service related issues might interfere on the prevalence of IPV, health care performance, and specifically on postnatal depression.

Although some studies/papers have issues in common with this one, none have focused on CMD in particular, nor attempted to dwell into the mediating processes underlying the IPVs and their connection to CMD. We did try to insert some of the papers’ contents into the Background section, but without success since their focus added little to the literature review we envisaged as a ‘building up’ to the theoretical model that was eventually proposed and
further analyzed. In fact, when planning this paper, we thought of including a comment in the Discussion on how the present findings added to the knowledge on the ‘consequences of violence/IPV and take the opportunity to cite our articles (form this and previous Projects); but we soon realized that this would open up a vast literature—far beyond the thematic scope of our study—and decided not to go down this line.

Specifically on the “enrollment criteria”, we have now expanded the text on the ‘Participants’ (as also request by Reviewer 1). At this reviewer’s request, it was here (page 7, paragraph 1)—“[g]iven some shared research purposes […] women were considered ineligible when […]”—that we opted to add several published studies of ours. We believe that (at least) scrutinizing the titles, the reader will be able to identify the other ‘shared research purposes’ of the study.

2.4.9 Very important – either in the background – or early in the methods section – I think you need a good description of survey-based structural equation modeling analysis. When is it used? What is it good for? What are its strengths and weaknesses in terms of understanding the strength of association between multiple risk factors, when working with cross sectional data? You really need more introduction to help motivate the paper so that readers who are not experts in the analytic technique can understand the benefits of using this methodology.

We addressed these issues by (1) adding an explanation to the last paragraph of the Background section (page 6, paragraph 1) and adding content to the Discussion when ‘debating’ the statistical approach (page 17, paragraph 2). Besides the positive appraisals of SEM (referring back to the Introduction), we draw the attention to the reservations one should bear with using cross-sectional data. At this point, we turn to the ‘ethical issue’ already argued in version 1.

2.5 METHODS:

2.5.1 Participants: Again it is critical to provide more detail surrounding the broader study – or “other shared research purposes” for which women were enrolled. Specifically, what was the rationale for the inclusion and exclusion criteria?

This has been dealt with in § 2.4.8.

2.5.2 How did you define an “intimate relationship”?

In the context of interpersonal violence, the term ‘intimate partner violence’ describes physical, sexual, or psychological harm by a current or former partner or spouse. This type of violence can occur among heterosexual or same-sex couples and does not require sexual intimacy.

Please refer to a CDC page on Injury Prevention & Control:


2.5.3 How did women who refused to participate differ from women who participated?

There is no statistical differences between those who participated or not concerning maternal age and schooling, and child age. These were the only pieces of information that could be collected (via service logs) about the few eligible women who were missed (2.9%). However, and most importantly, there is no reason to believe that the frequency (incidence/prevalence) of CMD among those missing women with and without IPV, alcohol misuse, drug abuse
(etc.) should differ from those effectively interviewed. If this is sustainable — i.e., the outcomes among non-missing and missing women are interchangeable in regards to the ‘exposure’ variables—, NMAR (non-missing-at-random) pattern is unlikely and thus missingness may be ignorable. Moreover, from a practical point of view, given the small amount of missing data, we believe that the bias would have been minimal anyway, even if NMAR were defensible. On this account, we ask not to extend the text.

2.5.4 Setting: A little bit of background about the health care system in Brazil could be helpful for the audience to understand the choice of “public primary health care facilities” as a recruitment setting.

This information is now provided in the revised version (page 6, paragraph 2).

2.5.5 Also describe the socioeconomic status of the clinic’s population.

This information is also provided in the revised version (page 6, paragraph 2).

2.5.6 Specify what do the various levels of the Brazilian Criterion of Economic Classification (BCEC) mean?

Besides in the new Table 1 (c.f. § 2.5.9 and § 2.6.3), information on the CCEB is provided, now on page 8, paragraph 1 of v2:

“The BCEC is a composite index comprised of a selected basket of available household assets and the educational status of the main breadwinner [52]. As recommended, the index was categorized into five strata, ranging from the richest (stratus A) to the most disadvantaged economic group (stratus E).”

2.5.7 Variables and measurements: Why were these particular domains (socio-economic position, stressors, substance (mis)use, IPV, and CMD) selected?

We believe we have provided information on this in the Introduction, offering a quite substantial literature review to back up the model build up processes (the theory to support the initial Direct Acyclic Graph). Of course, as in any complex modelling procedure such as this, several factors (‘knots’ in DAG parlor) could have been used/studied as well (or instead), depending on the public health perspective taken. Yet we understand this is a dynamic process to be improved and refined in future research.

Literature has identified other variables of interest- for example, nutrition (Brenda M.Y. Leung, Bonnie J. Kaplan, Perinatal Depression: Prevalence, Risks, and the Nutrition Link—A Review of the Literature, Journal of the American Dietetic Association, Volume 109, Issue 9, September 2009, Pages 1566-1575). Of note, the literature has also identified protective factors that might mediate the impact of IPV on CMD: http://uknowledge.uky.edu/cgi/viewcontent.cgi?article=1114&context=crvaw_facpub. Why weren’t these types of measures included?

We agree that there are nutritional factors involved with IPV and mental health disorders, but these were not the focus of our project at the time (as they are now —our research group is currently involved in a longitudinal study addressing these factors). Then, we opted to concentrate on more ‘distal’ exposures, rather than ‘proximal’ effectors (mediators) at the nutritional level.

The explanation about the absence of 'social support' is of a different order, however. Information on this variable was collected, but we decided not to include social support in the
putative model for methodological reasons. Given the cross-sectional nature of the study, we concluded that the outcome could have an influence on the recall of some perception-driven antecedents and bias the results.

Addressing the reviewer’s concerns, we are now inserting a passage (page 17, paragraph 3) where we discuss the potential problems arising from the cross-sectional approach. Please notice how we attempt to distinguish the ‘potential biasing’ variables that were measured with instruments tapping factual events/items (and thus allowed into the model), from social support that is based on the subject's perception (and, therefore, excluded). Please note also that we use two references ‘on social support’ that are more recent than the one suggested by the reviewer.

2.5.8 Why were the variables that make up each domain selected? In particular, why does the category of stressful life events during pregnancy include the number of children under age 5? I do not doubt that having young children is stressful but is this a validated measure? Why is relationship stability assessed specifically in a spousal context?

The selection of variables to compose the different domains was based on an extensive literature review (Background and Methods/Variables). For example, the support for the inclusion of the ‘number of children under 5’ as a potential burden is detailed in the Background section (page 5, paragraph 2). In passing, ‘number of children under age 5’ was not understood as a ‘category of stressful life events during pregnancy’, but as a separate variable (proxy to the stressor ‘maternal burden’).

2.5.9 The measurement instrument narrative descriptions are extensive and helpful, but a table that specifies each instrument’s name, population, delivery method, length, reliability, validity, scaling/scoring, etc. would be an asset to the audience and allow for an easier read.

Although informative, this table is not customary in epidemiological studies. We believe that a review such as this would require an enormous literature review to cover all the instruments’ histories of development and cross-cultural adaptation processes. These histories could/would of course include papers (peer-reviewed or from government documents) addressing reliability and validity, both ‘internal’ (structural/dimensional/factorial validity) and ‘external’ (via hypothesis testing) (c.f., http://www.cosmin.nl/). Furthermore, these studies would likely explore/involve various delivery methods, scales/scores and populations, and therefore, conveying all of these issues to the reader would be an awesome task, even in a stand-alone article covering all the proposed instruments at once.

We thus plead not to engage in such an endeavor, but instead, provide a summary table of the variables’ instruments and coding systems. See § 2.6.3 for more details.

2.5.10 Substance use: The study uses the words “alcohol (mis)use”, “(mis)abuse”, “use”, and “misuse”. Choose the most appropriate term and maintain consistency.

In the revised version of the manuscript, the term ‘misuse’ is now consistently used for alcohol, whereas ‘abuse’ is used for illicit drugs.

2.5.11 Provide a description of the specifics of the total survey instrument used. How many total questions were included to given an idea of respondent burden?

The multi-dimensional questionnaire included approximately 368 questions and lasted about 45 minutes, including items used for evaluating eligibility and the main questionnaire. This information is now on page 6, paragraph 3.
2.5.12 Did these women receive additional care based on study findings (for example, were referrals made for those reporting IPV or CMDs?) Did they receive standard of care? What is standard of care?

Besides the scheduled childcare visit and/or vaccination rounds, women/mothers were informed about health care units providing special services and hostels for cases of domestic violence in various parts of the city of Rio de Janeiro, regardless of whether they were identified as victims or not in the study. One of the offshoots of this study has been to provide information and promote discussion within the health units and municipal and state health departments, aiming at improving the care of women and children, especially in relation to maternal mental health.

We added some of this information to the last paragraph of the section on ‘Participants’ (page 7). The whole passage now reads:

“… The effective study sample thus included 810 women who were interviewed in a reserved area without the presence of anyone but the interviewer, after signing an informed consent. Anonymity and confidentiality were completely assured. Women were informed about available health care units providing special services and hostels for cases of domestic violence in various parts of the city of Rio de Janeiro, regardless of whether they were identified as victims or not. The study was approved by the Research Ethics Committee of Rio de Janeiro Municipal Health Department in conformity with the principles embodied in the declaration of Helsinki.”

2.6 RESULTS

2.6.1 Throughout the text and in the figures – I would avoid use of your analytic short hand e.g. “SLEV” “AGEPR” “HHCND” “WSCHL” that require the reader to continually look back in the list of abbreviations to see what the term refers to. Instead use more understandable short hand terms. For example if “WSCHL” refers to “Women’s schooling achievements” – use the shorthand “EDUC”.

The suggestion was incorporated in the revised version. New shorthand may be readily viewed in the new versions of Figures 1 and 2.

2.6.2 Likewise, if “SLEV” stands for “Stressful Life Events during pregnancy”, note this seems like a poor term when the text describes this variable as being an amalgam of age at pregnancy, marital status, and having children under 5 years – none of which are “events” per se! So perhaps use the term “STRESSORS”? NSDUQ – use “DRUGS”. for HHCND (which is also very poorly described) use “HOUSING”.

Please see § 2.6.1.
2.6.3 Table 1 and each of the figures each need an extensive legend. While the authors define terms in the text, the table and figures should be understandable without having to go back to the text to clarify whether Socio-economic strata “A” or “E” is the highest strata or the lowest strata. Likewise for “household condition score”, which I could not sort out even when looking back in the text. Is a 0 worse or better than 7 for having water, sewage disposal, etc. these terms and their scores need to be fully described, in the text and legends of the table and figures. Each table and figure should alone. I really think the paper would benefit from a Measures Table that lays out each of the measures in the model (with references and number of items that comprise the measure) and the scaling and scoring of each.

We addressed these issues by introducing a new table providing a summary of each variables’ underlying composition/instrument, along with details of their scaling/scoring (as ‘strongly’ suggested by the reviewer). We opted to show only the coding used in main SEM models, but the way variables are categorized in Table 2 (former table 1 in v1 with added bivariate analysis; see § 2.6.7) may be easily figured out. Table 1 should be anchored towards the end of paragraph 1 of the ‘Variables and measurements’ (sub)section.

In our view, this comprehensive table allows circumvent the problem of legends since the reader can always refer to it. Without the legends describing each variable, the other tables become (remain) less cluttered.

The titles of tables and figures have been edited (improved).

2.6.4 Using terms like “loose conjugal relationships” is confusing if the metrics haven’t been described completely. (In this case, for example, the stratification of intimate relationships has not been precisely defined, so “loose” is a puzzling descriptive.)

This point may be addressed now with the new Table 1 (see § 2.6.3). We also changed loose to unstable in the text.

2.6.5 Information about the substance use of both partners is helpful, but results reporting here is in the context of the couple as a unit. You never mentioned that the male partner also completed the risk assessment – or are you relying on the women’s report of their partners substance abuse? This needs clarification in the methods section.

Information on how risk assessment of partners was obtained is provided for both alcohol and illicit drugs intake (page 9, paragraph 1).

2.6.6 There is very little detail surrounding findings of maternal age overall, other than references to teenage pregnancy. Was teen pregnancy the real topic of interest in the broader study? If so, that should be described more specifically in the methods section. If, in fact, the focus of the broader study was on teen mothers then perhaps some discussion of the role of the different age groups and whether some were more at risk than others in the bivariate analysis? Were there risks or mediating/moderating factors that were specific to the teenage or older population? There must be some rationale as to why age group (presumably the younger age group?) was treated as a stressful life event?

No, teenage pregnancy was not a topic of main interest in the broader study. This characteristic was included as a covariate, since a consistent literature has been showing that pregnancy among teenagers may be particularly stressful (page 5, paragraph 2). Moreover, teenage pregnancy potentially qualified as a confounder (although it turned out not to be part of the ‘minimal sufficient adjustment set’ as diagnosed in a formal interim analysis of our structural equation models using DAGitty [1]).

2.6.7 Very important: It would improve the manuscript to include a Table 2, that includes the bivariate analysis between each of the risk factors and the outcome of CMD. Currently it is unclear how many women in the study had the outcome of interest and how that outcome varied across the various risk factors. That might address place a lot of the questions raised by the manuscript. It is very hard to evaluate the study findings without this information.

Information on the bivariate analyses has been added to Table 2 (former table 1) at the reviewer’s request and is referred to on page 12 of version 2. We only show the conditional probability (as %) of positive CMD according to variable status, but the frequency (%) of CMD(−ve) in each level may be easily figured out from the table; for instance, the % of CMD(−ve) in BCEC_A is \( \frac{15−2}{15} = 0.8666 \times 100 = 86.7\% \). We hope this addition elucidates most (perhaps all) issues raised by the reviewer. (c.f., § 2.6.3)

2.6.8 I will specifically ask the statistical reviewer whether the final model (figure 2) would benefit from including the coefficients on each of the connecting lines?

We plead that this is not implemented, not only to avoid cluttering the figure, but also because we provide all the coefficients in Table 3 (of v2), including their 95% CI. In addition to these “aesthetic considerations”, the two displays would also contain too many redundancies.

2.7 DISCUSSION

2.7.1 Strengths and limitations: It is mentioned that these measurement tools are “well known and comprehensive… already adapted for use in Brazil”. Was any work done to assess whether these tools are culturally generalizable? If not, that would be a potential limitation that should be mentioned.

While there is always the possibility that an adapted instrument lacks cross-cultural generalization due to problems of cultural identity, unresolved semantic idiosyncrasies and/or psychometric impediments (e.g., differential item functioning), contrary to what the reviewer suggests and converging with most authors (like us) who have been carrying out cross-cultural adaptation studies, we see this issue —that the instruments went through a thorough process of adaptation— as a virtue and not as a limitation. In fact, we could engage in a discussion on the theoretical and operational limits involved and their implications (in general and in relation to the instruments used here in particular), but we believe this would unduly lengthen the article. Indicating that many considerations can be found in the cited articles (and others therein), we thus ask not to extend the text further.

2.7.2 How generalizable are these findings? It might be more appropriate for the background section – and in the limitation section to use and identify relevant research literature pertaining to Brazil.

Consistent with the vision exposed in § 2.4.4, despite the sample being Brazilian, we believe the study concerns the relationship between events ‘in human populations’ (in general). Thus, in accepting the internal validity of the study (in an epidemiological sense), we understand that the evidence transcends what happens specifically in Brazil and thus is generalizable (in the limit that any epidemiological study carried out in a certain locality and population so allows). As conveyed in § 2.4.4, although there is always a possibility of some modification
effect by domain, and thus that the findings differ due to some social, cultural or other factors specific to our country, we implicitly assert ‘universality’ (generalizability) since we could not think of anything sustaining this proposition. Yet, we do recognize that the findings are provisional (as in any study) and warn the reader that our findings require corroboration in further studies. As explained in § 2.4.4, we added a passage to the section on the ‘implications and future directions’ (page 18, paragraph 3) to reinforce this debate.

Please note that, despite our ‘non-particularistic’ stance, there are 13 Brazilian articles on ‘mental health’ in the paper (15%).

2.7.3 Only 2.9% of the women invited to participate in the study declined. To what do you attribute this level of enrollment success? Was the initial survey part of their routine care?

Although mothers and children were approached during routine visits to the health services, the operationalization of this study was completely separate. This involved a logistics very much focused on avoiding losses. The thorough training of the data collection team was particularly important in this strategy.

2.7.4 Were there any interesting characteristics in participants that you had not anticipated that could affect results?

No, we cannot think of any ‘interesting characteristics’ that may have affected the results.

2.8 IMPLICATIONS AND FUTURE DIRECTIONS

2.8.1 You do not acknowledge that there are other risk factors that could be explored. This connects back to the need for a rationale for the risk factor selection for the broader study. While the scope of this study’s findings is necessarily limited to the variables explored, it is worthwhile to point out what other factors (unmeasured in the larger study) might also be salient and worth investigating – for example the woman’s level of social support.

This has been addressed in § 2.4.2 and especially in § 2.5.7.

2.8.2 I also think the issue of how findings should inform interventions is sorely neglected – but I acknowledge that this is beyond the scope of the current work and will address this issue in my on line commentary, should the paper be accepted for publication.

Thank you in advance for your future comment.