Author's response to reviews

Title: Health-related quality of life of Palestinian preschoolers in the Gaza Strip: a cross-sectional study

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Author's response to reviews: see over
Dear Dr Joanne Reeve,

Thank you for your thoughtful review of our manuscript. Please, find enclosed a revised manuscript that takes into consideration your comments and suggestions. We are enclosing a revised version where the most significant changes (mostly in response to your suggestions) have been highlighted. We made additional editing of the manuscript to tighten up the language but these minor edits have not been marked in the enclosed version. Please, see below a point-by-point response (in **bold**) to each of your reviewers’ comments (in *italics*):

**REVIEWERS’ COMMENTS:**

1. **The Introduction should provide a more detailed review of the extant research literature on the effects of war on child adjustment. Currently, there is only one reference to PTSD.**?

   *We now provided more detailed review of the literature on the effects of war on child adjustment (pages 3-5).*

2. **A set of hypotheses should be generated at the end of the Introduction, and supported by the available research literature. These hypotheses should then guide the data analysis, particularly given the large number of statistical contracts across subgroups.**

   *As there are no studies of quality of life in children in war, we postulated associations based on research on factors associated with child mental health in war context (pages 5 and 6).*

3. **Tables 4, 5, and 6 should indicate the significant differences between the PedsQL™ scores across the contrast subgroups using superscripts (e.g., a, b, c). The footer on Table 4 should include Cohen’s values for small, medium, and large effect sizes. Thus, the tables should be self-explanatory, without having to refer to the Result section to understand the differences between subgroups.**

   *We now provided significant differences in Tables 4 and 5, and in Tables 6 and 7*
4. The references in the Discussion section should be double checked. For example, in the first paragraph, the reference to “children with cerebral palsy” is reference #10. However, reference #10 refers to PedsQL™ ADHD findings. Several other references are incorrectly cited.

   We reviewed the references. As to children with cerebral palsy, their PedsQL scores are mentioned in the PedsQL ADHD findings.

5. The reference list should be corrected for partial references. A more careful proof-reading is indicated, including checking whether the correct references are cited in the text throughout the manuscript.

   We reviewed the reference list in the revised file.

6. Although there is no evidence from other war zones, how do the findings contrast with similar (if any, even if not directly comparable) studies from low-income countries in relative stability?

   We compared the PedsQL scores with children in Iran and children in Argentina from low-income families (page 16).

7. What are the public health implications for policy and Gaza services, and other similar regions in the world?

   We provided public health implications for policy (pages 19 and 20).

8. The authors should more clearly state their purpose, and it should be in line with their methods. Was the primary purposed to test HRQOL as a tool and add to evidence for its use in diverse populations? or were they using the tool to conceptualize the effects of war on children beyond PTSD and the "other ... measures" they mention?

   The primary purpose of the paper is to examine impact of war on children beyond mortality, morbidity and PTSD. As a secondary outcome, this study aimed to examine applicability, floor and ceiling effect and reliability among children exposed to political violence, to add to the evidence for its relevance in diverse populations such as that under study. This is now clearly stated. (pages 5 and 6).

9. The regression analysis uses violence as a composite measure, but includes each item as separate variable in the same equation, as well. This methodology needs further amplification and perhaps correction (more details below).

   In Methodology (page 9), we explain that the exposure to violence was examined as both a summary measure and as individual exposures. In the regression models, we either included the summary score or the individual exposures (but not combined).
10. The lit review is far too contracted, which affected both the poor framing at the beginning and the conclusions.

We have now expanded the literature review (pages 3-6, 16-17, and 19-20).

11. The authors should list in a different order the conditions among US children that are associated with comparable scores in Palestinian children: Put ADHD after cerebral palsy. I can see in the manuscript why the order is the way it is 2(it’s in order of score), but ADHD just doesn’t have their impact as the others, and it should go last.

This has been addressed (page 15).

12. The sentence “Factors associated with lower HRQOL scores were…” should tell the author that lower is associated with worse.

This has been addressed (page 8).

13. When HRQOL is introduced, it should first be spelled out, and explained as a concept-how is this a different way of conceptualizing well-being? We’re not sure if this is the authors’ intent, but it appears from the way the background section is written that this paper is more about a specific tool (HRQOL) than an assessment of new measures of wellbeing among children in Palestine that have not been previously examined. If the intent is not to simply test a tool, we would suggest re-ordering the background section to first emphasize the ways well-being has been previously conceptualized and measured.

The paper is about assessment of a new measure of well-being rather testing PedsQL tool. The suggested edit has been done (pages 3-5).

14. There needs to be more background on well-being among children in Palestine, and the Gaza Strip in particular, on which a lot has been done (see for example the work of Gaza Community Mental Health, esp. for example Quota,2005, but others as well; Abdeen, 2008 Dubow 2009 and 2010; Punamaki 1997 and 2008) and then detailing what your choice of measure specifically adds to the body of literature what we are missing by merely measuring PTSD or “traditional measures of morbidity and mortality” (which should be more specific and fleshed-out, similarly to the PTSD section). In other words, please clarify that what you are doing is a conceptual issue (i.e. expanding our knowledge of well-being in children in Gaza through widening the conceptualization of well-being beyond PTSD and other concepts) RATHER than simply testing a tool for the sake of validation or curiosity. If your intent is to both test a tool and look at its predictors within your sample, be clear about those 2 objectives in your title, abstract, introduction, and methods.

We provided more data on well-being of Palestinian children (pages 3-5). The main study objective is to expand the knowledge of well-being in children exposed to political violence through widening the conceptualization of well-being beyond PTSD, morbidity, and mortality measures. As a secondary outcome, to examine applicability, content
validity, and reliability of PesdQL in our study sample. The suggested edits have been done (pages 3, 5-6).

15. In your variables section and your results, you discuss the sub-scales within the overall HRQOL concept. These should be stated as sub-objectives—you’re not just looking at overall QOL, you’re looking at specific facets of QOL.

Addressed in page 2.

16. What are “Traditional measures of morbidity and mortality?” It’s not clear what that sentence is trying to do.

This sentence is trying to point out that there are other important outcome measures on the impact of war on children, other than morbidity and mortality. This point is now more clearly made (pages 3 and 4).

17. The sentence on the criteria for stratification doesn’t tell us WHY you stratified, and how you used the stratification to structure the selection process. It would also be useful to know how many of each kindergarten class fit in each category of stratification (and how the categories fit together, i.e. how many in each locality were UNRWA? Were there one of each type of administration for each locality? Etc.); a table would be useful here. Also, please be clear that you randomized at the level of the school, not the child.

A more detailed description of the sampling strategy is now provided (pages 6 and 7).

18. The “reference population” you describe in the U.S. are California kids enrolled in SCHIP. These are mostly low income kids, aren’t they? Isn’t that worth stating? So we’re comparing traumatized kids in Palestine to low-income (not average) kids in the US? That needs to be amplified.

A more detailed description of the reference population is now provided (pages 7 and 8).

19. Is the reported date of birth from school records or the mother?

Based on mother’s report—now stated (page 8).

20. Nutritional status is unclear; did you obtain individual information on children (if so, what?) that you then used the WHO software on to compute z-scores?

We measured height of children and converted height standardized scores based on child age and gender (page 8).
21. What’s the justification for your cut-off for the SRQ? Is that a standard cut-off or based on your data’s distribution?

Cut off point is based on the lead author of the SDQ’s own recommendations (Goodman, 2001). We believe that using previously used categorizations strengthens the generalizability of our results and the potential for comparison with previous research.

22. How is “direct military confrontation area” defined?

Frequent fighting between Israeli soldiers and Palestinians and frequent military operations (page 9).

23. For deprivation, when you asked about not enough money, was that NOW or EVER OR HOW OFTEN? We would have thought the mother declaring her child as deprived would result in over-reporting, but the frequency counts don’t bear out that concern. Odd.

In the last 3 months (page 9).

24. The measures and the analysis need to be clearly linked and to have clearer explanations of what you are using as outcomes and what are co-variates (or controls). These also need to line up better with your hypotheses on page 4.

The suggested edits have been done (pages 11 and 12).

25. 350 observations: you mean children? Or mother/child pairs?

Mother/child pairs (page 10).

26. “We examined the feasibility of the PedsQL 4.0…” Feasibility isn’t exactly a clear concept. Could you elaborate?

Addressed and elaborated on the rationale (page 11)

27. “We examined Cronbach’s alpha…” of what variable/measure?

Of PedsQL (page 11).

28. Tell us what concept the reader can understand by a measure of “effect size.”

More details about effect size are now provided (page 11).

29. The last paragraph of this section is unclear (esp. end of page 9); your analysis doesn’t seem to fit with your research questions. If you used regression analysis for all 3 of these outcomes (HRQOL, physical health and psychosocial health), then are these all your research questions? Also, the terms physical and psychosocial health are unclear-clarify that
these are your subscales from the children’s QOL measure as reported by the mother. Or are they something else?

A more detailed analysis plan is now provided (pages 5 and 6).

30. Also, you say you examine factors related to these outcomes-which factors? All 9 of your variables that you describe? And did you just throw these all into your regression equations at once and then do a deletion process? Please be clearer about how you established your regression models.

The regression models are now more explicitly described (pages 11 and 12).

31. Later, in the discussion section, you should address your thoughts on WHY stunting in Beit Hanoun was less than that of the overall study sample.

This is now addressed on page 16 [This may be explained by the higher percentage of the households in Beit Hanoun receiving food assistance on a regular basis compared to the overall study sample at the time of our study (91% versus 51%, respectively).]

32. Feasibility
Again, please be clear from the beginning of your objectives; if you’re testing feasibility of a specific tool, that should be stated from the beginning as one of your objectives.

Now added to study objectives listed on page 6.

33. Page 11: “Both exceeded the standard of 0.70 for group comparisons.” Please state what that means to a lay reader.

We replaced it now with: “Cronbach’s alpha of PedsQL 4.0 among toddlers and children 5-7 years of age were 0.91 and 0.85, respectively, similar to those reported by Varni for the younger group, but slightly below in the older group” (page 13).

34. For floor and ceiling effects, give us a brief description of why these concepts are important.

A description has been included (page 11).

35. Do you mean “The PedsQL total score was non-significantly higher for stunted children (?) with better maternal…” If not, higher than who? “,while those WHO (not that) rated their health as fair or poor…”

The suggested edits have been done (page 14).
36. Mean scores
2nd sentence: “The total score mean was 62”
3rd sentence: “Compared to the reference population, the mothers also reported significantly lower HRQOL...”
4th sentence: numbers cited in text don’t match the table.
Again, be clear what your measures are; HRQOL is your total composite scale, is that correct? And psychosocial health and emotional functioning are subscales of the total scale?

The suggested edits have been done and numbers have been checked (page 14).

37. Group differences
Before your first sub-head, tell us how tables 5 & 6 are organized. Also, both tables 5 & 6 need a total summary score for each dimension, so the categorical differences can be compared to the overall number for reference.

The suggested edits have been done.

38. Group differences reporting should include tests of mean differences for statistical significance, which should be reported in the table. Only statistically significant findings should be reported in the text, only brief highlights should be presented in the text, and the text should include the means and the significance level.

The suggested edits have been done (pages 14 and 15) and (Tables 5 and 6).

39. Household factors
Refer to the table where each of the findings are illustrated. This is confusing: “Those whose mothers reported any form of social support had lower emotional functioning score.” Please discuss in the discussion section, suggesting theories for this. Similarly, “Mothers with only elementary schooling reported lower scores...in all subscales in their children except for school functioning.” Why would that be? (offer an idea in the discussion section).

The suggested edits have been done (page 17).

40. Multivariate model
1st sentence: why not say “multivariate regression model” rather than “joint multivariable model?”

The suggested edits have been done (page 15).

41. page 13: do you mean “deprived of food, water, or electricity?”

We meant material deprivation and poverty (not deprived of food, water, and electricity) (page 14).
42. On your reporting of Table 7: Reporting the betas and p values in your text is preferable.

Now provided the p-value of the associated factors. I believe that adding both the betas and p-values will be redundant (page 15).

43. In your multivariate model, you included exposure to traumatic events as one variable and each of the items in the index as their own variables as well? All in the same model? This seems a little troubling-please justify in analysis section.

   We examined exposure to traumatic events both as a summary measure and individual exposures. Summary and individual measures were not included in the same model; if the summary measure wasn’t significant, it was dropped from the model, and then individual items were entered one at a time (page 9).

44. Page 14: “As expected, physical health was least affected by deprivation and violence.” Why would we expect that?

   An explanation is now provided (page 16).

45. Discussion, 2nd paragraph: “Older children in our sample had lower HRQOL, as rated by their mothers. This is in alignment with a study…” (similarly, use “aligns” not “agreement” in the line 2nd from the bottom of page 14

   The suggested edits have been done –“consistent” has been used in the second instance (pages 16 and 17).

46. 3rd paragraph: Except for poor child mental health and living in direct military confrontation area, each form of adversity has different associations with PedsQL..”

   The suggested edits have been done (page 17).

47. “Furthermore, the biased mother may report both child exposure to traumatic events and lower HRQOL.”

   The suggested edits have been done (page 18).

48. And this is the paragraph where you would discuss why the SCHIP comparison population is actually already a lower standard than a completely stress-free population.

   Details now provided (page 18).

49. 3rd line from bottom: “effects of war” (not impact). And 2nd from the bottom: “relationship between maternal mental health” (not impact of)

   The suggested edits have been done (page 18).
50. The conclusion about older children is too simplistic. Could it be that this is also an indication of cumulative stress? Simply that it’s in agreement with a study means little.

This alternative possible explanation is now provided (pages 16 and 17).

51. End of page 14-the part of social support is not consistent, though, with many other studies-see Khamis in Palestine and other literature on social support.

The suggested edits have been done (page 17).

52. Both the conclusion and the introduction need considerable help with the lit reviews. The authors are missing many studies, done specifically in Palestine, on the question of political violence and mental health, overall and among children. In addition to the ones listed above, I would be sure to look at the special issue of the International Journal of Behavioral Development 2008 32(4) on war and children by Sagi-Schwartz and others. Sagi-Schwartz’s article in that issue reviews more than 20 studies done in Palestine with children.

The literature review has been expanded (page 3-5, 17, 19-20).

53. Tables
The footnotes in the tables might do better with numerals rather than funny squiggle symbols.
(Then you can remove “Gaza, 2007” from the titles)

The tables’ captions have been revised.

54. Table 1:
I would state raw numbers with percentages in parentheses afterwards. For example, FEMALES 173 (49%) Maternal age is in YEARS Average household income in US$ (and then skip the footnote) Number of persons in household above 18 years

The suggested edits have been done (page 26).

55. Table 2: Please state that these characteristics are as reported by mothers on behalf of their children (if that’s the case) Can you distinguish between the overall proportion and those of Beit Hanoun? (add another column) “injured on TV” do you mean “injured on local TV news?” Because seeing it on a cop show isn’t quite the same is it? “Witnessed firing on houses” (no need for “of”) “Detained in house during incursion” (no need for “his”) Perhaps use dividers or shading to help trace the numbers to the experience? What instrument did you use to capture these experiences? Name it in the caption.

The suggested edits have been done (page 27).
56. Table 3--Can you distinguish between the overall proportion and those of Beit Hanoun? (add another column)

Addressed in a separate Table (page 28).

57. Table 4--The title is not good—make it clearer and less cluttered with jargon. Put the instrument source in the caption.
Use a footnote to explain what is the meaning of “effect size.”
In footnote, you could insert “low-income” ahead of “American children” (unless you think that’s not true—but I think it is)

The suggested edits have been done (page 28).

58. Tables 5 and 6--You should note in footnotes which instruments are used to calculate each factor. Need total summary score for each column in both tables for comparison purposes.
State what the denominator is for the score; is it out of 100? If you’re comparing means, include test of mean differences for statistical significance and report these

The suggested edits have been done (pages 29 and 30).

59. Table 7--Same advice as for table 4 with regard to title. What are you not illustrating ALL data for all rows? Show all data, and put the significant p-values in BOLD.
Make good use of footnotes to tell your reader how to interpret the coefficient. (child age (month)* where the *says, “for each additional month of age, child’s score drops (worsens) by .26 points”) And for gender: “girls have average score 3.16 more than boys”)
And for traumatic events: “for each additional traumatic event, score drops by 1.31 points”
“and” electricity? Or “or”? 

The suggested edits have been done (page 31).

We sincerely appreciate your consideration of our manuscript. Please, let us know if you have any additional questions or concerns.

Yours truly,
Salwa Massad, MCPH, PhD