Author's response to reviews

Title: Comparison of Children's Depressive Symptoms among Different Family interactions Types in Northern Taiwan

Authors:

Wen-chi Wu (wenchiwu@nhri.org.tw)
Chi-Hsien Kao (arron_0704@hotamil.com)
Lee-Lan Yen (lan@ha.mc.ntu.edu.tw)
Szu-Hsien Tony Lee (tonylee@mail.ndmctsgh.edu.tw)

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Author's response to reviews: see over
Dear Reviewers:

Thank you for your many valuable comments that have helped us to improve our paper. Please refer to the underlined areas in the manuscript which represent the necessary revisions. Attached are our point by point responses.

Yours sincerely,

Lee-Lan Yen  
Sc. D  
Professor  
Institute of Health Policy and Management,  
College of Public Health,  
National Taiwan University.  
Room 623, No.17, Hsu-Chow Road, Taipei, Taiwan, R.O.C.  
Email: lan@ha.mc.ntu.edu.tw
Responses to the comments by Katherine Shelton

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Abstract:
1. The background should be re-written. The statements are too vague and over-reach the scope of the article. Two simple statements that make the case that family functioning is associated with children's depressive symptoms would suffice.

   The background section of the abstract has been revised in line with the above comments.

2. Methods: How old are the children? Why should the reader be concerned about the proportion of the sample that lives in Taipei or Hsinchu?

   The children in this dataset were 6th graders. The age range of 6th graders’ in Taiwan is 11 to 12 years. We have added this age range in the “methods” section of the abstract. We agree that the proportion in each residential area does not need to be mentioned in the abstract so this has been removed.

3. The results section doesn't adequately capture what was found. The frequencies for each item on the depression scale should not be described. Instead, focus on the types of interaction identified and how these relate to depressive symptoms.

   The frequencies for each item on the depression scale have been removed. The results section of the abstract has been revised.

4. Conclusions: Again, the statements made overreach the findings. This study doesn’t really tell us how best to prevent child and adolescent depression.

   We have revised the conclusions of the abstract.

Background:
1. The authors provide an overview of several different perspectives on family functioning. What is not clear to the reader is which perspective or framework is favoured by the authors. The conclusion that while previous research has conceptualised family interaction into types, family life is more complex than can
be captured by such an approach rather undermines the case for the analyses that follow. Thus, while the introduction is interesting, it really isn’t clear what the authors hypothesised regarding their approach to characterizing family interaction or which interaction styles would be associated with depressive symptoms.

We apologise that we did not make our point clear enough. We believe that family interaction can be categorized which is consistent with the approach of previous research [21-25]. However, we think that family interaction should be based on more than two components. We have made some revisions related to this idea in the underlined sections in the fourth to sixth paragraphs of the background.

2. In re-organising the introductory material, it might be helpful to first describe the theory that guides the present study and then present evidence that different types of interaction are linked to symptoms of depression in young people.

We have re-written the fourth to sixth paragraphs of the background in accordance with these suggestions.

Methods:
1. Participants: What proportion of the sample is from Taipei city vs. Hsinchu County? How old are the participating students? How many students were sampled (not just those with complete questionnaires)? Do the authors have any other information about these families that would help the reader get a sense of the data e.g. level of parental education, socioeconomic status, parent occupation, marital status etc? Do the authors regard the sample as representative of Taiwanese families?

In regards to the proportion of students from each location, 54.3% of participants were from Taipei and 45.7% were from Hsinchu. The students were 6th graders and hence were aged 11-12 years in 2003. The original sample consisted of 2499 students and 98% of the students completed the questionnaire. Approximately 57% of fathers had a high school level of education, and 28.9% had at least a college degree. About 65.9% of mothers had a high school level of education, and 18.9% had at least a college degree. In addition, 87.6% of parents were married. This sample only represents students studying in public elementary schools in Taipei City and Hsinchu County in northern Taiwan. We revised the section on study participants to help the reader get a sense of the data.

2. Measures: Some subscales have sample items, some don’t. I wonder if it might help
the reader to have a table or appendix in which the complete measure is presented.

Please refer to Appendix 1.

3. Data management and statistical analyses: Rather than frequencies and percentages, it would be more helpful to the reader to know if these measures are normally distributed or not (particularly depressive symptoms). It’s not at all clear why ANOVA and regression techniques are to be used. Given the aims of the study, only the ANOVA results appear relevant here.

We have decided to remove the part about the regression analysis in this study. In addition, information about the kurtosis and skewness of depressive symptoms were added in a note under Table 1, as the total scores of depressive symptoms were not normally distributed. The natural logarithm of the depressive symptom score was used when conducting the ANOVA to calculate the F value. The new results are shown in Table 3 and a note is provided under Table 3 to explain the transformation.

Results:
1. It is very hard for the reader to interpret the description of depressive symptoms. It would be more interesting to know if there were sex differences in levels of depressive symptoms and the association between symptoms and age.

We revised table 1 to show the differences in depressive symptoms between genders. In this sample, the age range was very small, so the association between age and symptoms was not analyzed. The results shown in Table 1 are described in the section “Depressive symptoms in study participants”.

2. What is the pattern of associations between the derived subscales? This should be included as a separate table.

When carrying out factor analysis, we applied varimax rotations of factor loadings. The pattern of loadings was such that each variable loaded highly on a single factor and has small to moderate loadings on the remaining factors. Hence, the correlations between each factor score (derived subscale) were very small. Is it necessary to include a table showing the correlation coefficients, considering that the majority of them are zero?
3. **There was insufficient information about the cluster analyses and how the results should be interpreted.**

   We have added additional information in the section “clustering of family interactions”.

4. **The last section, which is really the crux of the paper, appears rather rushed. No rationale was presented in the introduction for conducting analyses separately by gender and location and no differences are found. If there was no a priori reason to expect differences, than the analyses and presentation of results could be simplified. The regression analyses are not clear. Why was this done and what was the independent variable? The ANOVA already tests for differences in symptom levels as a function of interaction style.**

   We agree with these comments. Since the relationships among gender, residential area and the types of family interaction are not our primary interest, we have simplified the results by removing Table 4 and adding a row on Table 3. As the ANOVA results are sufficient to show the differences, the part on regression was also removed. This part of the revision is in the section “Relationship between family interaction types and depressive symptoms”.

**Discussion:**

1. **The text needs sharpening here. Avoid expressing the findings in the context of the 'development of depression'. These results relate to symptom levels assessed at the same time as family interaction so the language of 'development' and prevention needs revision.**

   We have revised the use of the terms ‘development’ or ‘prevention’ in the discussion section.

2. **The section that describes Taiwanese parents applies to all parents, irrespective of nationality! (pg. 14). There also needs to be consistency in reference to the sample as not all readers will be familiar with the cultural context. It would be clearer to refer to the sample as Taiwanese rather than switch between 'Taiwanese parents' and 'Chinese parents'.**

   Thank you for mentioning this point. We have changed the wording ‘Chinese’ to ‘Taiwanese’.
3. The discussion introduces a lot of new material without reference back to perspectives described in the introduction. There needs to be a more streamlined presentation of the literature that orients the paper.

We have reorganized the references in the revised manuscript.

4. The section on sex differences and geographical belongs in the introduction and will require revision based on the age group under study here (i.e. children vs. adolescents). Again, why was location (County sampled) of interest here? What was hypothesized? The results testing differences between groups in depression level were not presented and there is no discussion of why such a difference might exist.

As the relationships among gender, residential area and depressive symptoms were not the key point of this study, the section ‘Relationship between gender, geographical location, family interactions and childhood depression’ was removed.

Study limitations:
1. This seemed rather vague and there are other limitations that should be acknowledged (e.g. single informant across all measures).

We have revised this section and added the limitation of using a single informant.

Conclusions:
1. A rather strong final statement that should be revised in line with the study findings.
   Thank you for your advice. We have revised the conclusion.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct).

1. Title: Suggest replacing Depression with depressive symptoms as no diagnostic criteria is used.

   Thank you for your suggestion. We agree that “depressive symptoms” is more appropriate in this study. We have changed the title to “Comparison of Children’s Depressive Symptoms among Different Family Interaction Types in Northern
Taiwan”.

2. There are some other minor essential revisions that should be made. Some of these relate to sentence structure and spelling (e.g. ‘researches’ appears throughout the document).

We have tried to correct these errors with the assistance of a native English speaker.
To reviewer: Michele Berk

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Introduction:
1. Several of the references are quite old. Please specify if newer research exists or not and include it if it does exist.

   Apart from several classical references, we have replaced old references with newer ones.

2. Please elaborate on the mechanisms by which family interaction styles may lead to depressive symptoms.

   We have added some statements on the relationship between family interaction styles and depressive symptoms in the fourth and fifth paragraphs of the background.

3. Several studies are presented in terms of how researchers grouped family interactions into types, but the results of these studies are not presented. These studies need to be discussed in greater detail. In its current version, the introduction does not give the reader a good understanding of prior research, nor how the present research contributes to the literature.

   The results of related studies have been added in the fifth paragraph of the background. We have added a statement in the last paragraph of the background to differentiate the contribution of this study from previous research.

4. Discussion of prior relevant studies conducted in Taiwan or other countries besides the United States and the similarities and differences between these findings should be added and would greatly strengthen the contribution and purpose of this manuscript.

   Some statements related to Taiwanese experiences have been added in the second and fourth paragraphs of the background. Further, we have added some statements about Taiwanese culture in the second paragraph of the discussion.

Method/Results:
1. Why were 6th graders chosen?

As we were interested in children’s subjective perspectives of depressive symptoms and family interactions, we required measurements to be reported by the students themselves. We chose 6th graders as they have enough maturity and ability to express their perceptions.

2. Is there reliability or validity data for the measure of depressive symptoms used? How was the measure scored? Are the cutoff scores for mild/moderate/severe depression? The fact that the average score was 10, in a range of 7 to 21, suggests that depressive symptoms in this sample were very low. Even in the “disciplined” group, the average depression score appears to be 11.94. Please address this and how it impacts the results. Your findings appear to be accounting for mild depression at best.

The internal reliability of depressive symptoms was assessed using Cronbach’s α (0.75) and the content validity was assessed by five psychological and behavioral professionals. The total score for depressive symptoms was obtained by the addition of scores from the seven items and used as an interval variable. A higher score indicated that children expressed more depressive symptoms. The participants in this study expressed mild depressive symptoms as they came from the general population. From a health promotion perspective, we would like to know how to prevent children’s depressive symptoms progressing from mild to severe depression. Hence, the results are still important for the promotion of children’s mental health. We have added some statements to this effect in the last paragraph of the results.

3. Were the dependent measures completed only by children or also by their parents? If so, please present results separately for adult and child data. Also, if measures were only completed by children, please comment on the reliability of self-report data in this age group.

The measures were all completed by the students. We believe that 6th graders can understand and answer the questions by themselves and can also express their perceptions correctly. We believe this to be the case because child development theory supports that 11-12 year old children (late childhood) have their own values and attitudes, and also because the interval reliability and validity were adequate.
4. Why were the particular categories of family interaction styles chosen? They appear to mirror the American literature presented, but how do these relate to Chinese culture? I would expect family interaction styles and values to be quite different in each culture.

The development of the CABLE Family interactions Scale was based on relevant literature and also included some culturally specific questions, for example: ‘parents tell you that children don’t understand things’ and ‘parents make you feel that they are always right’. In Taiwan there is a proverb: ‘children only have ears and don’t have a mouth’. This means that Taiwanese parents do not wish their children to talk too much and prefer them to listen. We took Taiwanese culture into consideration when developing our questionnaire.

Discussion:

1. Again, I think this manuscript would be greatly strengthened by making cultural issues the major focus of this discussion.

Thank you for your advice. We have added some statements regarding Taiwanese culture to the discussion.

2. A more detailed discussion of how high levels of discipline in families in this sample may lead to depressive symptoms in children would be helpful. Also some context for understanding the findings would be helpful – for example, what is the typical level of discipline in Chinese families?

This is a good point. However, in this study, we cannot measure how high levels of discipline in families will increase children’s depressive symptoms because ‘discipline’ is one of the four types of family interactions and it was not measured on an interval scale. We suggest that future research should focus on this issue. Little previous research has quantified the level of discipline in Taiwan. As a result, it is difficult for us to know the typical level of discipline in Taiwanese families. In our opinion, Taiwanese families are more strict than western families, due to the impact of traditional values. We have added some statements regarding this issue in the first paragraph of the discussion.

3. The finding that girls have a higher rate of depression in this sample than boys should be mentioned in the Results section as well as the discussion. What might be the reason for this gender difference?
Table 1 has been revised to show the differences in depressive symptoms between genders. A new paragraph has been added in the last part of the discussion.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Introduction:
Page 4: Please specify what the Disability Adjusted Life Year is.

The definition of Disability Adjusted Life Year has been added to the first paragraph of the introduction.

General:
There are several misuses of the English language. I assume this is due to English not being the authors’ primary language.

We have tried to correct these errors with the assistance of a native English speaker.