

PEER REVIEW HISTORY

BMJ Paediatrics Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Identifying Vulnerable Children's Stress Levels and Coping Measures during COVID-19 Pandemic in Japan: A Mixed Method Study
AUTHORS	Takeuchi, Hajime Napier-Raman, Sharanya Asemota, Osamagbe Raman, Shanti

VERSION 1 – REVIEW

REVIEWER	Reviewer name: Dr. Geir Gunnlaughsson Institution and Country: University of Iceland School of Social Sciences, Faculty of Sociology, Anthropology and Folkloristics Competing interests: None
REVIEW RETURNED	18-Oct-2021

GENERAL COMMENTS	<p>The authors are congratulated for embarking on this study to highlight children's experiences during a pandemic, here in Japan. Overall, the paper is well written, and the results well presented and supported with relevant other published work. Notably, the qualitative data presented is interesting and well summarised, and described.</p> <p>Before publication, I recommend the authors consider the following:</p> <ol style="list-style-type: none">1. Throughout the paper, presentation, and discussion of the results give the impression that they apply to all Japanese children even though study limitations are recognized in the paragraph of strengths and limitations. For example, in the Discussion on p16/24 (line 38): "We found that Japanese children had higher overall stress levels during the pandemic than a comparable sample of American children before the pandemic." Obviously and recognized by the authors, this is a small study, including children in a vulnerable situation, in a specific setting, and thus cannot be extrapolated to be valid for all Japanese children. The authors should revise the wording with this in mind all through the ms.2. In its current version, the paper lacks a good description of the work of the three non-profit organizations that helped reach out to the participants. Are these working on the national level or in other well-defined areas in Japan (district/city/urban area/...)? How many children/families are being served by their work. Are those drop-in services/referral services/services supported by the local social welfare system or ...? Are these day services/consultations/etc...? A minimum understanding of this work might help the reader to understand better the vulnerable context in which the children live and are recruited from.3. The methodology for the online sample is not described in any detail. How many children were potentially reached with an invitation to participate in the survey? Reminders?4. Paragraph beginning on p9/24 (lines 51) might be combined with the paragraph above beginning on line 42. Further, as this is a new paragraph, reference to Figure 1 might be appropriate once again.5. It is difficult to evaluate the impact of COVID on the score
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	<p>compared to a sample of average, American children. The participants in this study are to be expected to have higher stress scores before the pandemic, considering their vulnerable position. The authors should recognise this fact in the ms.</p> <p>6. The word "manga" on p15 (line 53) needs explanation.</p> <p>7. It might be a matter of taste, but the reference to the work of Yamaguchi et al. on p16/24 does not properly fit in when the authors summarize their results in the first paragraph of the Discussion. In my opinion, this would better fit as a separate paragraph with a discussion considering the results reported in the ms.</p>
REVIEWER	<p>Reviewer name: Dr. Praveen Unki</p> <p>Institution and Country: Adichunchanagiri Institute of Medical Sciences, pediatrics</p> <p>Competing interests: None</p>
REVIEW RETURNED	29-Oct-2021
GENERAL COMMENTS	<p>To the authors,</p> <p>First, I thank all the authors for this valuable work. It's important to study the effects of Covid-19 pandemic, as we are not completely aware of effects caused by it (on physical and mental health). It was great honour to review this study. This study has following pitfalls.</p> <ol style="list-style-type: none"> 1. Sample size: Too small to draw any inference and to generalize to public. As it is a questionnaire based study sample size collected using Google forms could have been more. 2. Correction of grammatical, spelling mistakes needs to be considered before proceeding further. Punctuation marks are missing. 3. On adding scores for questions 1, 2, 4, 5, 8, 9, 12 with questions scored reverse such as number 3, 6, 7, 10, 11, 13, and 14, the overall PSS-C score drawn will be low, masking the true total score and leading to misinterpretation of study. 4. Assent of children more than 7years of age has not been mentioned. 5. Results part: Qualitative data findings were described in too much detail including reply of the child for questions, which is not required as it will not contribute anything to overall study results. Mean (standard deviation), median for Stressor sensitivity, Emotional state, Security, Time pressure components have not been described in the results part. 6. 6 forms were partially filled out of 36 cases enrolled in the study, which will again alter the study result significantly because of small sample size. 7. References part: Page numbers are incomplete in few references (22, 26).

VERSION 1 – AUTHOR RESPONSE

Professor Imti Choonara
Editor-in-Chief
BMJ Paediatrics Open

17 Nov 2021

Dear Professor Choonara

Re: bmjpo-2021-001310 - "Identifying Vulnerable Children's Stress Levels and Coping Measures during COVID-19 Pandemic in Japan: A Mixed Method Study"

Authors: Hajime Takeuchi, Sharanya Napier-Raman, Osamagbe Asemota, Shanti Raman

Thank you for the opportunity to respond to the reviewers comments and improve the strength of this paper. We have responded to each of the suggestions, changes are documented in the table below and highlighted in the revised paper. This paper is part of the special collection on 'Young Voices in the time of COVID-19' and we are proud to be showcasing less heard voices from Japanese children in this paper.

Editors comments	Responses
Editor in Chief	
Comparison with American children is NOT possible as you modified the PSS-C. Delete all mention of comparison with American children. This is NOT optional. Is reverse scoring the normal procedure for the PSS-C? Please explain exactly how your use differs from previous studies and justify your changes. If major, we may ask for removal of this section.	We have deleted any mention of comparison with American children in Results and Methods,. There was actually minimal modification to the scale, we only changed the time scale to participants' experiences "during the COVID-19 Pandemic", instead of the "past week". Which was done following discussion with the author.
	We have followed the procedure exactly as described by the author of the Perceived Stress Scale for Children. See "questions were developed with reverse wording and scoring in order to increase the likelihood of truthful responding." ¹
Associate Editor	
The background to the manuscript is quite well written, but it is not necessarily apparent what the evidence gap is that this research addresses. Could the authors please draw this out in a revision?	Given the word count limitations, we did not have much room to play with. We have added this sentence to make the evidence gap obvious: In Japan, detrimental impacts of pandemic measures on mental and physical health appear to be more severe among children from lower socio-economic backgrounds; however, there are few studies focusing on vulnerable CYP views and experiences.
I think a key thing missing from the background is an early definition of what is meant by 'vulnerable' – please could the authors add this into any revision of the manuscript.	We acknowledge that there are differences based on geopolitics, about who is considered "vulnerable", indeed a raging debate on whether we should use the term at all. Many of the key papers from child health advocates and global agencies have stressed the need to support vulnerable children during COVID-19. We have added this sentence, along with a good reference on the need to prioritise vulnerable children: ² While there are differences in which groups are considered 'vulnerable' depending on geography, CYP experiencing poverty, family dysfunction and neglect are recognised as priority populations if

	<p>inequities are to be addressed. In the Japanese context, “vulnerable” CYP are those experiencing socioeconomic difficulties, which includes children from single-parent families, and children experiencing abuse. This is already stated in the methods.</p>
<p>There seems to be a lack of justification for the use of the Perceived Stress Scale for Children – why was this the best measure to use? Could the authors include a justification in any revision?</p> <p>There also seems to be limited information about the adaptation – could the authors clarify if the translation, addition of the free text boxes, and a change in the timescale requesting the children to report on was the extent of the adaptation? Was the adapted questionnaire piloted in advance of this research? I have particular concerns about the timescale on which children were asked to report (during the covid-19 pandemic as opposed to a few weeks) – could the authors address this concern? Could children’s stress levels have varied over the pandemic? Would it have been more appropriate to only ask about recent weeks (as per the original questionnaire) to avoid recall bias and variation?</p>	<p>We chose the Perceived Stress Scale for Children (PSS-C), because it is brief, user-friendly for children and a validated tool. One of the key benefits of the International Society for Social Pediatrics & Child Health (ISSOP)’s COVID-19 Research Group, was the coming together of researchers, clinicians and child health advocates from across the world, sharing tools that could be readily used and modified rapidly in different settings to understand CYP’s experiences of COVID-19- we have mentioned this in acknowledgements. The PSS-C was a tool recommended by researchers within the group and several paediatric research groups in Africa, Latin America and Japan actually used this tool with their child populations. We have added: We chose the PSS-C, because it is an easy to use tool for children, brief, efficient, validated and ideally suited for online application.</p> <p>Yes, we have stated in the text: With White’s permission, the PSS-C was modified (Fig.1), and translated into Japanese. The modification made to the scale was asking about participants’ experiences “during the COVID-19 Pandemic” instead of “in the last week”. Additionally, open-ended ‘free description’ versions of each question were added to collect qualitative data on CYP’s experiences</p> <p>We address concerns about the timescale on which children were asked to report (during the COVID-19 pandemic, instead of the recent week) in Discussion, as a limitation. Given the need to access CYP’s views of COVID-19, in a timely manner, we proceeded with this modification of the questionnaire, which seemed justified.</p>
<p>Relatedly, it would also be useful to have further context about any variations in restrictions over the time-period that children were asked to report on. Were the pandemic-related restrictions static or did</p>	<p>We have provided more detail about this in the text. In Japan, the policy of school closure was conducted between March and May 2020. However, thereafter, according to the pandemic situations, partial closure or</p>

they change over the course of the time-period that children were asked to think about and during the period of recruitment?	staggered attendance was held depending on each municipality. We implemented the study between August and November in 2020.
Could the authors explain whether the sample has been impacted by recruitment through 3 organisations and online delivery of the questionnaire? For example, could it be that there children who are not being supported by such organisations who are therefore more vulnerable? How many children were approached and how many agreed to take part? Did online delivery create a digital divide in this study and does this have implications for the results?	We do not believe that the sample has been impacted by recruitment via these not profit organisations—in fact it is the only access to these CYP. Online delivery of the questionnaire in the Japanese context actually improved access and CYP agency in responding. The children receiving support from these organisations are amongst the most vulnerable groups in this province. However, even if children live in poverty, almost all of them have their own smart phones in these ages—therefore it did NOT create a digital divide in this study. The organisations introduced this questionnaire study to all the children attending in that period. But the number of children who chose to participate, versus the number who could have participated is not clear; since it was entirely voluntary.
Can the authors describe the recruitment process in more detail – as requested by reviewer 1. Were there any inclusion/exclusion criteria applied for example – for example relating to children’s reading age/ability?	We deliberately did not have inclusion and exclusion criteria, since we wanted to encourage full voluntary participation. No coercion or incentives were provided to push CYP to participate. The age range of children was 8-17 years and as you can see from figure 1, the tool uses very simple language with visual prompts.
Ethical approval is mentioned but there is no detail given as to providing potential participants with information or the procedure of assent/consent.	This is actually provided on the front page of the questionnaire. “Dear friends I want to know the current situation of your daily life and feelings during the COVID-19 pandemic. I want to make use of your comments for a better society after COVID-19. This is not a test. No one will know who you are. It will take about 30 minutes, but you can stop at any time. Cheer up everyone with your voice! If you accept to answer the questions, please proceed to the next step. Thank you.”
PPI: I think this section is quite limited and it raises concerns about that this research was not well-informed by vulnerable young children – or by children and young people at all.	With all due respect, this statement is quite insulting to the authors. We have stated in the PPI, that “The authors have extensive experience of working with vulnerable child and youth populations. The first author has strong links with the three non-profit organisations servicing children and young people experiencing social disadvantage, where this study was conducted. The research questions and outcome measures were based on the emerging literature on children’s experiences of

	<p>COVID-19, the authors' clinical and research experience and the relationship with the community-based sector." There is now a significant body of literature, including that published in BMJ Paediatrics Open -- chronicling COVID-19 pandemic related health and wellbeing onslaughts on vulnerable children and young people. And to state that this research which provided a platform for disadvantaged Japanese CYP to express themselves, who were proactively engaged by the organisations that supported them socially, was somehow "not well-informed by vulnerable young children – or by children and young people at all"- is uncalled for. We did not state that this research was carried out by CYP, but we are confident that it provides valuable insights into vulnerable CYP's views and experiences of COVID-19.</p>
I also did not understand the relevance of the paragraph here on recruitment and google forms – this information is perhaps more relevant elsewhere and should be moved accordingly.	We can't see any mention of google forms in the PPI. Perhaps it is the uploading process for publication in the BMJPO- it does not appear in our document.
Quantitative analysis: The quantitative analysis does not seem appropriate – for the research question or for the sample size.	We have provided simple descriptive statistics only. If Table 1, provides unnecessary detail on the parts of the PSS-C, it can be removed. We agree that it does not add much.
In terms of the research question, could the authors explain the rationale for comparison of stress scores with a pre-pandemic US sample of children? Could it be as reviewer 1 queries, that vulnerable children in Japan would have been more stressed than a US sample prior to the pandemic? Could it be that their scores have not changed with the pandemic or that they could potentially be lower during the pandemic as we do not have baseline data?	We have deleted any mention of comparison with American children in Results and Methods. We do need a baseline measure of children's stress levels using this tool, so we have suggested that this study will provide some baseline values of PSS-C in a non-English speaking Asian population.
In terms of the sample size, can the authors also explain why further efforts were not made to recruit more children to the study to increase the sample size?	We extended the period of recruitment to gather more replies. And the organisations did do their best. However, we were unable to get more responses.
Can the authors also comment on the 6 children for whom there is missing data – how much data was missing and what was the decision making process around inclusion? Were these 6 children spread across the age range of the sample?	By missing data, we are talking about children not filling out all the questions. The cases with missing data are 11 years old girl (missed 1 question), 14 years old boy (missed 2 questions), 8 years old boy (missed one question), 13 years old girl (6 questions missing), 11 years old girl (one question

	missing) and 12 years old boy (1 question missing). We analysed all the data available. We know for a fact that children in this study filled out most of the questions, compared to other research studies in different settings using the same tool.
I would query whether anything beyond presenting descriptive statistics for the questionnaire would be appropriate due to these limitations.	We have only presented simple descriptive statistics. The only extra detail is provided in table 1, which can be removed. However, we know that other research studies have this tool and having this published will help guide their own analysis.
Could the authors in their revision provide the rationale for their qualitative analysis. Can the authors also comment on whether they feel that they have achieved data saturation for the qualitative analysis? The authors state: "Many children only answered a few of the open responses and only one completed all questions." Of the children who did answer the open-ended questions, how much data did they provide? Is this sufficient data to draw conclusions from? I think this highlights the concerns of reviewer 2 about what we can learn from the qualitative results – the data is of interest (as noted by reviewer 1) but is it sufficient? How much evidence do we have for each of the identified stressors/protective factors?	We have provided more details about qualitative analysis in the revision. Data saturation is not an appropriate term to use here- as this was thematic content analysis of text responses, not in-depth interviews. Yes, the text-based data was more than sufficient to analyse-- qualitative data analysis relies on what is available and every bit of response is equally valid. We have rephrased to "Not all children answered free text responses to every question, only one completed all questions," because in fact there were a remarkable number and range of text responses. We do not draw 'conclusions' as one would in quantitative analysis, but get a richer understanding of what children's concerns and views are from qualitative analysis. Again the use of "evidence" for identified stressors and protective factors is misplaced- we are merely describing the themes that emerged from the data.
There are issues with spelling, punctuation and grammar as raised by reviewer 2 and these should be addressed in any revision.	We have reviewed spelling, punctuation and grammar in this revision
Reviewer 1	
The authors are congratulated for embarking on this study to highlight children's experiences during a pandemic, here in Japan. Overall, the paper is well written, and the results well presented and supported with relevant other published work. Notably, the qualitative data presented is interesting and well summarised, and described.	We thank you for your comments
Throughout the paper, presentation, and discussion of the results give the impression that they apply to all Japanese children even though study limitations are recognized in the paragraph of strengths and limitations.	Thank you for pointing this out. We have removed any comparison to the American sample and have taken care to point out this study pertains to a sample of vulnerable Japanese CYP.

For example, in the Discussion on p16/24 (line 38): “We found that Japanese children had higher overall stress levels during the pandemic than a comparable sample of American children before the pandemic.” Obviously and recognized by the authors, this is a small study, including children in a vulnerable situation, in a specific setting, and thus cannot be extrapolated to be valid for all Japanese children. The authors should revise the wording with this in mind all through the ms.	
In its current version, the paper lacks a good description of the work of the three non-profit organizations that helped reach out to the participants. Are these working on the national level or in other well-defined areas in Japan (district/city/urban area/...)? How many children/families are being served by their work. Are those drop-in services/referral services/services supported by the local social welfare system or ...? Are these day services/consultations/etc..? A minimum understanding of this work might help the reader to understand better the vulnerable context in which the children live and are recruited from.	We did not have the space to describe the three organisations in detail; we have provided a bit more in the text in the revision. Shinmama Osaka Ouendan: This organization supports more than 150 single-mother families with sending food daily necessities a month. And it has a shelter for single-mother families and young people, which is called the family home. It works locally and nationwide. Shinmama Ouendan Toyonaka: This organisation supports more than 30 families. Shinmama means single mother, but the organization supports not only single mother families but also both parents families with socioeconomic difficulties. The function as a food bank is main. And the house of this organization is an important place for vulnerable children to spend the twilight time there. CPAO: This organisation mainly supports children. The activities are daily food bank, and it works as a twilight space. And it has a foster function for children who cannot stay home safely.
The methodology for the online sample is not described in any detail. How many children were potentially reached with an invitation to participate in the survey? Reminders?	Staff members of the three organisations approached children who attended their services to participate in the project. But since they are community-based voluntary organisations, the actual number of children who attended regularly or were approached is not available.
Paragraph beginning on p9/24 (lines 51) might be combined with the paragraph above beginning on line 42. Further, as this is a new paragraph, reference to Figure 1 might be appropriate once again	Thank you for the suggestion, we have combined paragraphs for better flow.
It is difficult to evaluate the impact of COVID on the score compared to a sample of average, American children. The participants	We acknowledge this and are grateful for the reviewers pointing this out. We have deleted any reference to comparing to the US sample

in this study are to be expected to have higher stress scores before the pandemic, considering their vulnerable position. The authors should recognise this fact in the ms.	
The word “manga” on p15 (line 53) needs explanation.	“Manga” is a Japanese comic book. Explained in parenthesis in revised version
It might be a matter of taste, but the reference to the work of Yamaguchi et al. on p16/24 does not properly fit in when the authors summarize their results in the first paragraph of the Discussion. In my opinion, this would better fit as a separate paragraph with a discussion considering the results reported in the ms.	Thank you, we agree and have moved this reference to the following paragraph, describing all other studies in Japan
Reviewer 2	
Sample size: Too small to draw any inference and to generalize to public. As it is a questionnaire based study sample size collected using Google forms could have been more.	We agree it is a small sample size, and we have been careful not to generalise findings to all children. We have mentioned this in the Limitations. However recruitment was via the staff from the not-profit-organisations and we could not increase responses. We wanted it to be entirely voluntary and anonymous
Correction of grammatical, spelling mistakes needs to be considered before proceeding further. Punctuation marks are missing.	We have reviewed grammar and punctuation in the revised version
On adding scores for questions 1, 2, 4, 5, 8, 9, 12 with questions scored reverse such as number 3, 6, 7, 10, 11, 13, and 14, the overall PSS-C score drawn will be low, masking the true total score and leading to misinterpretation of study.	On the contrary, reverse scoring implies exactly that. We followed the authors instructions on reverse scoring as mentioned before: “questions were developed with reverse wording and scoring in order to increase the likelihood of truthful responding.” By reverse scoring positive questions, those with low scores would get high scores, increasing the total stress score.
Assent of children more than 7years of age has not been mentioned.	Children were approached by staff from the 3 organisations about their willingness to participate. The front page of the questionnaire has a child-friendly information and assent statement. Children only undertook to do the questionnaire if they wished.
Qualitative data findings were described in too much detail including reply of the child for questions, which is not required as it will not contribute anything to overall study results. Mean (standard deviation), median for Stressor sensitivity, Emotional state, Security, Time pressure components have not been described in the results part.	We have pared down the reporting of the qualitative data in the revised submission. We did not report in greater detail on the quantitative analysis of the PSS-C, because we are aware that the sample size is small and we do not have a comparable population to describe differences. The American study on which this tool was based only reported on total stress scores of children in a clinical and non-clinical sample.

6 forms were partially filled out of 36 cases enrolled in the study, which will again alter the study result significantly because of small sample size.	We acknowledge that, however as we have mentioned before of the 6 children most of them only missed 1 or 2 questions.
References part: Page numbers are incomplete in few references (22, 26).	Thanks you we have fixed these.

Yours sincerely

Professor Hajime Takeuchi

(Corresponding author on behalf of all co-authors)

References

1. White BP. The Perceived Stress Scale for Children: A Pilot Study in a Sample of 153 Children. *International Journal of Pediatrics and Child Health* 2014;2:45-52.
2. Joshua P, Zwi K, Moran P, et al. Prioritizing vulnerable children: why should we address inequity? *Child: Care, Health and Development* 2015 doi: doi:10.1111/cch.12264

VERSION 2 – REVIEW

REVIEWER	Reviewer name: Dr. Geir Gunnlaughsson Institution and Country: University of Iceland School of Social Sciences, Faculty of Sociology, Anthropology and Folkloristics Competing interests: None
REVIEW RETURNED	17-Dec-2021

GENERAL COMMENTS	<p>The authors are to be congratulated for the major revision they have undertaken on the ms. The paper has improved, and the issues raised have been addressed. As before, the strength of this paper lies in analysing the voices of a group of children living in a socially disadvantaged position in a specific setting in Japan, better described in the revised ms. Yet, there are still issues that the authors need to address before publication.</p> <p>Abstract Page 2, line 28: Change to "... focusing on a group of socially disadvantaged children."</p> <p>Methods In the revised ms, the work of the three organisations that provide the recruitment basis for the study is well described and improves understanding of the situation of the children who participated in the study, obviously a hard-to-reach group. It would strengthen the section to shortly describe how the children accessed the online questionnaire, i.e., did all the children who were approached by the workers of the three organizations get a link to use to answer the questionnaire or only those who agreed to participate? Did they respond in the facilities of the respective organizations, were they alone when they answered, and did they need help from an adult</p>
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	<p>(e.g., their parents?) as some were young as 8 years of age. In the authors' comments, it is indicated that the children used their own mobiles, and in that case, this fact needs to be highlighted in the ms.</p> <p>Results. Overall, I find the results section an interesting reading with important perspectives of study children on the Covid pandemic and its impact on their daily lives. Yet, I would recommend the authors to play down the repeated reference to "children" in the text, when they are obviously referring to the children who participated in the study. This is, e.g., exemplified in the first sentence (p. 13/29, line 33): "School was mentioned in responses to questions about what made children feel rushed (Q2), busy (Q3), scared (Q8) and angry (Q9)." These questions specifically address respondents' feelings, not children generally, as evidenced in the supplemental material provided. This should be given attention to in all the results section and revised accordingly, as found appropriate.</p> <p>The legend of Table 1 still gives reference to Japanese children. I suggest a revision of the legend to indicate more precisely who these Japanese children are, for example: "Stress scores in a group of socially disadvantaged Japanese children aged 8-17 years (n=36), as assessed by the Perceived Stress Scale for children." This would facilitate the reading for those who might initially only scan the article before digging deep into the results.</p> <p>Discussion Page 19/29, line 40: I recommend that the authors emphasize more that they are reporting results of a study of a specific group of children in Japan who are in a socioeconomically difficult situation.</p> <p>Page 21/29, line 37. Here the reference is given to American children for stress scores that have not been introduced earlier. What is this "American sample"? This needs clarification. It is also doubtful the stress score results in this study group can serve as a baseline for non-speaking Asian CYP, and that should be toned down.</p> <p>Page 21/29, line 50. Here the reference is given to a safe space for children to answer the questionnaire. This "safe space" needs better explanation, as highlighted in comments on the Methods section.</p>
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REVIEWER	<p>Reviewer name: Dr. Praveen Unki Institution and Country: Adichunchanagiri Institute of Medical Sciences, pediatrics Competing interests: None</p>
REVIEW RETURNED	16-Dec-2021

GENERAL COMMENTS	<p>To the authors, Authors' interest in research is well appreciated and they have made good efforts to answer all the queries raised by reviewers. However this study has following drawbacks which were not explained satisfactorily.</p> <ol style="list-style-type: none"> 1. Sample size: Too small to draw any inference and to generalize the same. As it is a questionnaire based study sample size collected using Google forms could have been more to get more valid result. 2. 6 forms were partially filled out of 36 cases enrolled in the study, which will again alter the study result significantly because of small sample size.
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VERSION 2 – AUTHOR RESPONSE

Professor Imti Choonara
Editor-in-Chief

6 Jan 2022

Dear Professor Choonara

Re: bmjpo-2021-001310 - "Identifying Vulnerable Children's Stress Levels and Coping Measures during COVID-19 Pandemic in Japan: A Mixed Method Study"

Authors: Hajime Takeuchi, Sharanya Napier-Raman, Osamagbe Asemota, Shanti Raman

Thank you again for the opportunity to respond to the reviewers' comments and improve the strength of this paper. We have responded to each of the suggestions. Changes are documented in the table below and highlighted in the revised article. This paper is part of the special collection on 'Young Voices in the time of COVID-19', and we are proud to be showcasing less heard voices from Japanese children in this paper. Editors comments

1. Editor in Chief

1) Abstract Results avoid use of the word "average". Replace with "mean" or "median."

We have replaced "mean" following the comment.

2) Abstract Conclusions 1st sentence. Replace the word "Japanese" with "The"

We have deleted "Japanese" and added, "The" following the comment.

3) What this study adds section (NOT what this study hopes to add) 1st statement add "The small cohort of" at the start of the sentence

We have replaced "What this study hopes to add?" with "What this study adds" following the comment, and added "The small cohort of" at the 1st sentence.

4) What this study adds section 2nd sentence delete "Japanese"

We have deleted "Japanese" following the comment.

5) Methods Analysis delete "We used Stat ViewTM for statistical analysis of quantitative data."

We have deleted this sentence, replaced it with: Simple descriptive analysis on the quantitative data was carried out using Stat ViewTM, since we have to document how the analysis was carried out.

6) Discussion 2nd sentence add "a group of" before "socioeconomically vulnerable Japanese children"

We have added "a specific group of" before "socioeconomically vulnerable Japanese children" following this and reviewer 2 comments.

7) Discussion page lines 30-52 delete the paragraph "The tool we used was a modified version of the PSS-C, the average score in our study was 14.8 ± 4.6 , with a median of 14. While we do not seek to compare this score with those of the American sample, we would suggest that our study provides a baseline score of a non-English speaking Asian CYP sample, which could be used in future comparisons. As Figure 1 illustrates, this simple tool was easy to deliver online and the majority of children completed the survey. A particular innovation was adapting this simple tool to the COVID-19 context, and allowing children to freely express themselves at every stage. Thus, the tool facilitated CYP's voices in a safe space.

We have deleted this paragraph following the comment.

8) Conclusions 1st sentence add "a small cohort of" before "Japanese CYP"

We have added "a small cohort of" following the comment.

1. Associate Editor

1) Please can the authors address the issues raised by reviewer 1. In particular, we would request that the authors are more cautious in their conclusions and be clear that the results pertain to this specific population.

We have added "a small cohort of" following the comment. And we have added, "Acknowledging that our study sample was small, our results nevertheless show that the pandemic placed a great deal of stress on this cohort of vulnerable CYP."

2) Please can the authors also address, the comments of reviewer 2. In particular we seek further clarifications around the method. Further, we would ask that the authors provide information around whether parental consent was required/sought for this study.

All studies involving children must include parental consent before ethical approval is given in addition to assent by children old enough to respond to the questions. Parental consent is mandatory and was obtained before children were recruited to give responses.

3) Thank you to the authors for removing comparisons with US samples, using the original version of the questionnaire from sections of the manuscript, we would ask that the authors to remove comparisons from the discussion.

We have deleted the sentence of the comparisons from the discussion.

3. Reviewer 1

1) Sample size: Too small to draw any inference and to generalize the same. As it is a questionnaire based study sample size collected using Google forms could have been more to get more valid result. Thank you for the comments. We have already mentioned it in the major limitation of the discussion. We extended the period but could not gather more replies. The nonprofit organisations did their best even though their daily activities were hectic. If we had set out to look at the general population of Japanese children there would have been ample room for a larger sample size- but that was not our target population.

2) 6 forms were partially filled out of 36 cases enrolled in the study, which will again alter the study result significantly because of small sample size.

Thank you for the comments. That is true. Some of the children did not fill out all questions. However, as it was an anonymous we didn't gather their e-mail addresses. So, it was difficult to chase children.

4. Reviewer 2

1) Abstract

Page 2, line 28: Change to "... focusing on a group of socially disadvantaged children."

We have added "a group of" before "socially disadvantaged children" following the comment.

2) Methods

In the revised ms, the work of the three organisations that provide the recruitment basis for the study is well described and improves understanding of the situation of the children who participated in the study, obviously a hard-to-reach group. It would strengthen the section to shortly describe how the children accessed the online questionnaire, i.e., did all the children who were approached by the workers of the three organizations get a link to use to answer the questionnaire or only those who agreed to participate? Did they respond in the facilities of the respective organizations, were they alone when they answered, and did they need help from an adult (e.g., their parents?) as some were young as 8 years of age. In the authors' comments, it is indicated that the children used their own mobiles, and in that case, this fact needs to be highlighted in the ms.

We have clarified this as follows: Children were given the QR code by the staff after parental consent was obtained and the children assented. Children without mobile phones were helped by parents to access the online questionnaire. Parents were encouraged to record their children's responses and not to interfere with the filling of the questionnaire by offering suggestions or commenting on their choices. Some responses were completed on site in the not-for-profit building while others were completed at home

3) Results.

Overall, I find the results section an interesting reading with important perspectives of study children on the Covid pandemic and its impact on their daily lives. Yet, I would recommend the authors to play down the repeated reference to "children" in the text, when they are obviously referring to the children who participated in the study. This is, e.g., exemplified in the first sentence (p. 13/29, line 33): "School was mentioned in responses to questions about what made children feel rushed (Q2), busy (Q3), scared (Q8) and angry (Q9)." These questions specifically address respondents' feelings, not children generally, as evidenced in the supplemental material provided. This should be given attention to in all the results section and revised accordingly, as found appropriate.

Thank you for the comments. The children involved in this study are not the typical exemplars of Japanese children. However, the children in this study described their feelings, which we have faithfully reported on. In the results section, we report on what the children in this study have said. We have however reduced use of the term "children" as suggested in the Discussion.

4) The legend of Table 1 still gives reference to Japanese children. I suggest a revision of the legend to indicate more precisely who these Japanese children are, for example: "Stress scores in a group of socially disadvantaged Japanese children aged 8-17 years (n=36), as assessed by the Perceived Stress Scale for children." This would facilitate the reading for those who might initially only scan the article before digging deep into the results.

We have replaced "Stress Scores of Japanese Children, as assessed by the Perceived Stress Scale for Children" with "Stress scores in a group of socially disadvantaged Japanese children aged 8-17 years (n=36), as assessed by the Perceived Stress Scale for children"

5) Discussion

Page 19/29, line 40: I recommend that the authors emphasize more that they are reporting results of a study of a specific group of children in Japan who are in a socioeconomically difficult situation. We have added several words as "Our study documents responses of a specific group of socioeconomically

vulnerable Japanese children to the COVID-19 pandemic and related public-health measures."

6) Page 21/29, line 37. Here the reference is given to American children for stress scores that have not been introduced earlier. What is this "American sample"? This needs clarification. It is also doubtful the stress score results in this study group can serve as a baseline for non-speaking Asian CYP, and that should be toned down.

We have deleted the sentence of the comparisons from the discussion; "The tool we used was a modified version of the PSS-C, the average score in our study was 14.8 ± 4.6 , with a median of 14. While we do not seek to compare this score with those of the American sample, we would suggest that our study provides a baseline score of a non-English speaking Asian CYP sample, which could be used in future comparisons. As Figure 1 illustrates, this simple tool was easy to deliver online and the majority of children completed the survey. A particular innovation was adapting this simple tool to the COVID-19 context, and allowing children to freely express themselves at every stage. Thus, the tool facilitated CYP's voices in a safe space."

7) Page 21/29, line 50. Here the reference is given to a safe space for children to answer the questionnaire. This "safe space" needs better explanation, as highlighted in comments on the Methods section.

We have added this sentence; A "safe space" is where children can spend their time peacefully and comfortably."

Yours sincerely

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