December 27, 2021

To: Prof. Cheri Shapiro & Prof. Anne Farrell,

Editors-in-Chief, *Journal of Child and Family Studies*

On April 20, 2021 we submitted a paper to the Journal of Child and Family Studies entitled "Mothers’ Perceptions on Children Screen Use During the COVID-19 Lockdown in Israel'' (JCFS-D-21-00345). On September 9, 2021, we submitted a revised manuscript of this article, and on December 13, 2021, we received your request to conduct another round of revisions.

My co-authors and I are happy to re-submit the improved manuscript.

Below, we describe the specific improvements made in the manuscript in relation to the comments made by the reviewers.

Thank you very much,

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**Revisions in detail**

**Reviewer #1**

1. Authors have improved a number of aspects of the paper, but I still see some problems that I believe should be corrected before publication. There are still a number of errors of English that are sufficient to distract if not confuse readers".

The manuscript was edited as requested and the language is much improved. Thank you for insisting on this improvement.

1. Authors are unfortunately correct to say that the use of parametric statistics with Likert scales is common, but that does not mean it is a good idea. I would suggest that they read Morris, S., Grice, J., & Cox, R. (2017). Scale imposition as quantitative alchemy. Studies on the transitivity of neuroticism ratings. Basic and Applied Social Psychology, 39(1), 1-18. If authors persist in using parametric statistics, I would suggest that they include a paragraph explaining why they have elected to do so and considering the possible effects of this choice on their results and conclusions.

We understand, and even share your concern. However, it will be highly confusing for readers to see such an apologetic paragraph considering the fact that most of the studies in the *Journal of Child and Family Studies* apply the same assumption. One day, the field (and perhaps the entire social sciences) might adopt better statistical conventions, but as long as it does not, we do not wish to make an example from our study.

1. The proposed experimental work needs to be discussed in more detail. What would be the instructions to the comparison group?

Thank you for encouraging us to expand on our proposal for future research. We now dedicate a full paragraph to describe our vision (pp. 17-18).

**Reviewer #2**

1. Thanks for added information on how the study was conducted. Under methodology (page 6 line 15), the sentence: The recruitment of participants has been conducted through a large survey panel in Israel on April 16, 2020….” Is this study from secondary data? If yes, please state clearly. Or is it a grammatical mistake where it should be had been conducted…. (still curious if it is secondary data) or do you mean was conducted….

We apologize for this confusion. The dataset was collected specifically for this study (i.e., it is not secondary). We clarified the description of the data in page 6.

1. Page 7: EFA: Thank you for the feedback on the factor analyses done. Such a table should be reported in the study because it is the basis of the measures you are reporting. Reviewing the factor analyses, it seems the 6 items load on 1 factor not two. Have you considered recoding the negative ones such that higher scores of all represent positive attitudes and lower scores would be less positive (negative) attitudes? Also in terms of meaningfulness of the two factors you have created it is not clear to me how “limiting screen time” loads on either negative or positive implications. Please review this measure once again. There is no theory to support how you split it too. You could also have split it by cognitive and emotional, but you did not.

We understand from this comment that our description of the factor analysis was insufficient. We now provide a detailed and clear description of the process, alongside the relevant factor-analysis tables (Tables 2 and 6) in the method sections of Study 1 and Study 2. We attach these tables here for your convenience.

Please notice that the two factors solution emerged from our bottom-up, exploratory approach; the two factors were extracted based on the conventional rule of eigenvalue > 1 (see in: Rosenthal & Rosnow, 2008, "Essentials of behavioral research: Methods and data analysis"). The classification of the items was based on item-loadings of at least 0.5 (in one case within the second factor analysis, an item, which was loaded on both factors was classified to the factor with which it was mostly correlated, in accordance with the guidelines provided by Rosenthal & Rosnow). Importantly, the test-retest reliability of the factor analysis was found to be good, since both analyses resulted in the same factorial pattern.

Please also notice that the items regarding the need to limit children's screen use are still considered attitudes of mothers (i.e., these are not behavioral items, but beliefs of mothers).





1. Page 8: Parental guilt and frustration: How about entering this variable as a predictor controlling for pre‐covid?

Thank you for this suggestion. We have now entered the pre-COVID guilt and frustration as a predictor. Notably, the pattern of the results remained the same. We have changed the statistical figures and the wording in the results section and the relevant tables (Tables 3 and 4) to match the modified analyses.

1. Results: Hypothesis 1‐Please report the analytic strategy you used for each hypothesis. You mentioned for the second hypothesis but not for the first.

Thank you for noticing this gap. We have used paired-samples t-tests. This information now appears in page 8.

1. **Study 2**. It would be beneficial to state the min‐max scores for each measure used, in the descriptive table, to help the reader fill in the blanks on any questions they have because it was a bit difficult to understand how variables were coded. For example, how was religiosity recoded.

Min-max scores were added to the relevant tables (Tables 1 and 5), whenever the scales of the variables were at least ordinal. In nominal scales, such as religiosity, we report the exact percent of participants for each category. We added a note beneath the tables to describe this point.

1. Page 10, line12: this is a cross‐sectional study. Screen time use as a central predictor of mother’s guilt and frustration refers causality which is not true. They are significantly associated. Kindly rephrase.

Thank you for noticing this. We rephrased the sentence to avoid causal-related language (page 10).

1. It is not clear how ADHD was recoded as it is a nominal variable with at least 3 responses. Kindly expand on it. Again, the descriptive table with Min and max values would help the reader.

We are sorry that this was not written explicitly in the text. Since some mothers reported of having more than one child with ADHD, we recoded this variable into a binary variable (0/1), which indicated whether the mother had at least one child with ADHD (this variable was entered to the model as a between-subjects predictor with two levels – with and without ADHD). We now describe this operational definition in page 13.

1. Page 13‐ please report again here that you operationalized “COVID increase” as a change in the two scores (creating one variable versus using 2 separate variables).

In study 2, our goal was to explore factors that could have contributed to the increase in pleasure use. We therefore conducted a **repeated measures** ANCOVA, in which the DV was the variable of pleasure use itself. The repeated measures analysis replaces the need for a designated variable that indicates the change in screen use following the COVID-19 lockdown.

1. Page 14 lines 17‐32 and page 16, lines 39 ‐51: please make sure the interpretations are aligned. I am not sure it is. There are a lot of double negatives which makes the results and interpretation confusing.

We made sure that the interpretations of the findings align with the raw results. We also improved the language used in the entire manuscript, as mentioned in our first response to Review 1.

1. Also why use ANCOVA when your DV is a continuous variable, and you have multiple continuous level predictors and two categorical variables? Multiple regressions a much robust and simpler method/option. For the significant ANCOVA interactions, post hoc analyses were not reported. A figure is also required to show the direction of the interactions reported.

Thank you for the opportunity to discuss the statistical methodology of our study. Assuming that the two statistical models – the regression analysis and the ANOVA – would have produced the same results (because they rely on the same linear assumptions), we chose the model that is most appropriate to the presentation of our *variables of interest*.

Traditionally, the classic use of ANOVA is to demonstrate effects of categorical variables (e.g., a therapeutic intervention) on continuous variables. Our independent variable of interest is, of course, the categorical variable of the COVID-19. The use of the ANOVA gives the reader an intuitive tool to interpretate the results. To control for additional *continuous* predictors of less interests, we applied the ANCOVA, which is the preferred mixed method for considering continuous covariates, in traditional ANOVAs.

Since the categorical variable of interest (i.e., the COVID-19) had only two levels (i.e., before/during the COVID-19), post-hoc analyses are not needed here. Yet, detailed interpretations for the observed interactions are provided in the results and the discussion sections. Illustrative figures for these interactions are less recommended in cases where the interactions are between categorical and continuous variables since their interpretations are typically not intuitive to readers. We nevertheless made sure that the textual description of these interactions would be easy to read and highly informative.

1. Finally, include a table of results for study 2 like you did for Study 1 (Tables 2&3). It would make it clearer to the reader what predictors you used and which were significant. I think one overall challenge of two related studies with so many predictors, two age groups and limited clear theoretical framework in the introduction section is that, it is quite difficult to chart the story clearly.

Thank you for this suggestion. The write up of this research was indeed not trivial. We added the requested tables as suggested (Tables 7 and 8).

1. In addition, was a measure of pre‐COVID and during COVID for two groups. It seem your DV is continuous (unless it is unclear to me) and doing a multiple regression would simplify the many predictors you have. For example, while it is not clear how PAQ are scored as three subgroups (please give info on that), they each could be entered in the regression or used as two categorical variables. This is an important study and requires a well‐organized and clear presentation of it.

Please see our response to your previous comment (comment 10). Please notice that the three sub-scales of the PAQ are also based on continuous scales.

Upon completion of this review, we wish to thank the reviewers and the editor, once again for reading our article and providing us with great suggestions for improvements. As mentioned by Reviewer 2, this research involves multiple and complex relationships and the review process helped us consolidate them into a holistic and relatively easy-to-read text.

Thank you very much,

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