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)	THE IMPORTANCE OF LIVING ARRANGEMENTS AND COPARENTING QUALITY FOR YOUNG CHILDREN'S MENTAL HEALTH AFTER PARENTAL DIVORCE-A CROSS-SECTIONAL PARENTAL SURVEY
AUTHORS	Bergstrom, Malin Salari, Raziye Hjern, Anders Hognäs, Robin Bergqvist, Kersti Fransson, Emma

VERSION 1 – REVIEW

REVIEWER	Reviewer name: Dr. William Fabricius Institution and Country: Arizona State University, Psychology, United States Competing interests: None
REVIEW RETURNED	10-Jun-2020

GENERAL COMMENTS	This study has potential for publication, but in my opinion the authors need to address some important issues. 1. The first is that the data clearly show an interaction between living arrangements and coparenting, but the authors do not make it clear to readers, and do not interpret it sufficiently. The means for living arrangements and coparenting quality in Table 2 (adjusted for the covariates) should be placed in a Figure so that readers can see the interaction. As in all cases where main effects are qualified by an interaction, the interaction is the effect that should be focused on, not the main effects. However, the authors do not discuss the interaction findings in the Discussion. Instead, they focus on the main effects; i.e., the finding that of the two main effects, coparenting accounts for more variance. The authors do not include a regression model with the interaction term, and do not offer a rationale for not doing so. That is unusual, and contributes to the downplaying of the interaction. Instead, they proceed directly in Table 4 to testing the pairwise comparisons among the eight means in the interaction. They do this in a separate regression model, using "intact/high coparenting" as the reference category, which of course is an arbitrary, if justifiable, choice. What those pairwise comparisons leave out, however, are the simple effects tests of living arrangements on child problems at high coparenting quality and at low coparenting quality, as well as tests of high versus low coparenting at each level of living arrangements other than intact families. It is clear that living arrangements have no relation to child problems when coparenting is low quality; all those children have

	similarly high levels of problems. But that is not the case when coparenting is high quality. Then joint physical custody children have as few problems as children in intact families, and problems increase as children live proportionally more time with one parent, up to the point where children living with only one parent have as many problems when their parents are cooperating well as when their parents are cooperating poorly! The authors' conclusion (p. 13) that "Once we accounted for coparenting quality, there were nothing but minimal differences in child mental health in different family arrangements" is based not on the interaction, but on the finding that of the two main effects, coparenting accounted for more variance. The interaction, however, shows the complete picture, including the fact that in one-parent families, there was no difference in child mental health in high versus low coparenting quality families, and the fact that in high quality coparenting, there were large differences in child mental health associated with different family arrangements. My advice would be to analyze the data with analysis of covariance. The independent variables are categorical, so there is no reason not to, and it would be much clearer to readers. The interaction would then be clearly tested, and could be followed up with simple-effects tests at each level of coparenting, as well as post-hoc tests at each level of living arrangements to give the complete picture. 2. It seems to me that we should worry about self-serving bias regarding the coparenting quality scale. At recruitment, parents were informed that the survey would be used to individualize their child's health visit, so therefore they knew their responses were not anonymous. Relatedly, the scale was not normally distributed, with most responses clustered at the high end, which also suggests self-serving bias. Is nonnormality unusual for a coparenting scale? Please explain to readers. The authors frankly discuss the limitations of this scale, which was shortened from the McHale and Kuersten-Hogan scale, but they should also mention that it needs to be validated with independent reports from both parents. Self-serving bias could probably account for the factor analysis finding of a single factor. The authors should compare the distributions obtained with the shortened and the full scale. They should tell the reader how the items were selected for the shortened scale. Given that only 20% were categorized as low quality, and all the rest as high quality, how are we to conceptualize them? If self-serving bias is at work, then the 20% should probably be seen as "very low," and the other 80% as a combination of the other four quintiles that would have appeared had self-serving bias not been at work (i.e., "poor," "moderate," "good," and "very good"). If self-serving bias is not at work, and the other 80% were really all similarly good coparents, then the bottom 20% might only be slightly worse at coparenting. I think the data make the former alternative more likely, because of the fact that in intact and joint custody families, those 20% of parents had children with a lot more problems. Thus, readers need to be reminded that the findings on child harm associated with low quality coparenting apply to only 20% of the population, and those

	are likely to be the families with the very lowest quality coparenting. This is important, because the effects of living arrangements apply to the remaining 80% of the population, which includes families at all levels of coparenting quality except the lowest level. 3. The authors should also explain to readers that this scale includes coparenting items as well as parent conflict items. The coparenting items are: ability to cooperate, support each other, and confide in/trust each other. The conflict item is: conflict related to children. Absence of cooperation is not conflict, but conflict related to their children surely is parent conflict. This relates to the point above about telling the reader how the items were selected. Minor points: 4. As I understand it, imputation of the scale mean is the least desirable way to handle missing data. Please justify. 5. p. 14 The first sentence of the first full paragraph, beginning, “For children living with…” doesn't make grammatical sense. 6. In Table 2, comparing intact and joint custody, how can the overall mean be higher in joint custody (7.26) than in intact (7.07), but the coparenting groups’ means both be lower than in intact families? 7.07 7.26 increase 6.69 6.39 decrease 8.78 8.42 decrease

REVIEWER	Reviewer name: Dr. Peter Flom Institution and Country: Peter Flom Consulting, United States Competing interests: None
REVIEW RETURNED	16-May-2020

GENERAL COMMENTS	I confine my remarks to statistical aspects of this paper. The basic approach is fine but I have some issues to resolve before I can recommend publication. The big issue is that independent variables should not be categorized. The coparenting scale and age should be left continuous and splines used to investigate nonlinearity. It is irrelevant whether the variables are normally distributed (OLS regression does not make assumptions about the distribution of the variables, it assumes that the errors are normally distributed and, if this assumption is violated, other methods (such as quantile regression) can be used. Other issues: p. 5 line 54 Insert "significant" between any and association p 9 Parents' age should be left continuous, but, in addition, you need to say how you are computing it. Is it the mother's age? Father's age? The average? The maximum? p. 9 Single imputation is not a good method. If there is very little missing data, then complete case analysis is fine, but if there is more, then multiple imputation should be used (at least if the data are missing at random or missing completely at random, which seems reasonable here).

	Peter Flom

REVIEWER	Reviewer name: Dr. Anja Steinbach Institution and Country: Univ Duisburg Essen, United Kingdom of Great Britain and Northern Ireland Competing interests: None
REVIEW RETURNED	02-Jun-2020

GENERAL COMMENTS	The objective of the paper was to investigate the association of living arrangements and co-parenting quality with mental health in preschool children after parental separation. The authors used data of a cross-sectional population-based study that included 12,845 3-year-old children in Sweden. As the authors point out, preschool children's well-being in joint physical custody is rarely studied. In addition, co-parenting quality was included, which also closes an important research gap. Thus, the paper adds important aspects to the existing literature. I have only some minor remarks. - Page 7: "12,845 children were included in the study, …" I got the impression that parents were included in the study, not children? - Page 7: I do not understand what the authors mean with this sentence: "We do not plan to have parents involved in the dissemination of the results of the study." - Page 8: If the parents were encouraged and obviously did complete the survey together: What are the methodological implications? Wouldn't it be likely that parents who are filling out the questionnaire together would give other answers than parents, who are filling out the questionnaire alone? For example, could it be possible that parents who filled out the questionnaire together state higher scores on the co-parenting scale? - Page 8: Are the authors convinced that the measurement of the living arrangement is exact enough? What, for example, does "living only with" mean? Do these children have any contact to the other parent? - Page 9: Why did the authors choose the cut-off at 20%? Are there any theoretical or empirical arguments? - Page 9: Why did the authors not use multiple imputation? - Page 10: 95% of the sample are nuclear families. Isn't the proportion of separated families too low for a Swedish sample? - Page 15: The authors only controlled for children's gender, parents' age, education, country of birth and coparenting quality. What about siblings, stepparents, working, hours, time since separation, and so on? - Page 22: The number of cases in some categories are rather small. Multivariate analysis with 2 cases are not possible or useful. The authors should consider deleting controls with extremely low number of cases in subgroups (like country of birth). - Page 24: The variable living arrangements should be moved to the first row. Why did the authors not display standardized coefficients and *** to indicate significant effects? This would be much easier to read.

VERSION 1 – AUTHOR RESPONSE

Answers to the reviewers regarding manuscript ID bmjpo-2020-000657
"The importance of living arrangements and coparenting quality for young children's mental health after parental divorce-a cross-sectional parental survey."

Please see our answers to the reviewers below

Reviewer: 1 Peter Flom, Peter Flom Consulting, USA

Comments to the Author
I confine my remarks to statistical aspects of this paper. The basic approach is fine but I have some issues to resolve before I can recommend publication.

The big issue is that independent variables should not be categorized. The coparenting scale and age should be left continuous and splines used to investigate nonlinearity. It is irrelevant whether the variables are normally distributed (OLS regression does not make assumptions about the distribution of the variables, it assumes that the errors are normally distributed and, if this assumption is violated, other methods (such as quantile regression) can be used.

Response: We are grateful for this comment, and have now in table 3 used coparenting as a continuous covariate. In the revised analysis we have dropped demographic variables with a large proportion of missing variables, including age. Covariates in the new analysis include gender, answering parent(s) and maternal education only, please see the new table 3. With regards to the analytic design we have chosen to follow the advice of reviewer 3 with ANCOVA as our main analytic method, which is presented as a figure in the revised manuscript.

Other issues:
p. 5 line 54 Insert "significant" between any and association

Response: Done

p 9 Parents' age should be left continuous, but, in addition, you need to say how you are computing it. Is it the mother's age? Father's age? The average? The maximum?

Response: As described above we excluded parents' age due to the high number of missing values.

p. 9 Single imputation is not a good method. If there is very little missing data, then complete case analysis is fine, but if there is more, then multiple imputation should be used (at least if the data are missing at random or missing completely at random, which seems reasonable here).

Response: The method used to impute the SDQ missing values is based on the guideline provided by the developers. We used a similar method for the coparenting scale which resulted in imputing the values for only 93 cases (i.e., 0.7% of cases).

As far as we know, it is not yet possible to pool the ANCOVA's results on data imputed using multiple imputation. Thus, we used the same procedure for handling the missing values and included the result for the data imputed using multiple imputation as sensitivity analyses. The related text in the manuscript now reads:

"As sensitivity analyses, we used multiple imputation (MI) to impute missing values on SDQ and coparenting scale (imputed at item level), as well as mother education (compulsory and high school had to be combined to run the MI). The regression analyses and estimated marginal means in the ANCOVA model revealed the same pattern of results (not shown)."

We can include the detailed results as an appendix if the reviewers believe this will be helpful.

Peter Flom

Reviewer: 2 Anja Steinbach, University of Duisburg-Essen, Germany

Comments to the Author
The objective of the paper was to investigate the association of living arrangements and co-parenting quality with mental health in preschool children after parental separation. The authors used data of a

cross-sectional population-based study that included 12,845 3-year-old children in Sweden. As the authors point out, preschool children's well-being in joint physical custody is rarely studied. In addition, co-parenting quality was included, which also closes an important research gap. Thus, the paper adds important aspects to the existing literature. I have only some minor remarks.

- Page 7: "12,845 children were included in the study, …" I got the impression that parents were included in the study, not children?

Response: The study is based on a parental survey. Due to the children's young age the parents have provided the data, both regarding family type, coparenting quality and children's mental health.

- Page 7: I do not understand what the authors mean with this sentence: "We do not plan to have parents involved in the dissemination of the results of the study."

Response: The journal asks all authors to include patients (here parents) in their work, such as planning of the study, piloting whether questions are relevant etc and we have chosen not to do that.

- Page 8: If the parents were encouraged and obviously did complete the survey together: What are the methodological implications? Wouldn't it be likely that parents who are filling out the questionnaire together would give other answers than parents, who are filling out the questionnaire alone? For example, could it be possible that parents who filled out the questionnaire together state higher scores on the co-parenting scale?

Response: We agree and possibly also the reverse is likely-that parents who get along well choose to complete the questionnaire together. It is for clinical reasons that parents are asked to complete the survey together, to prepare for the visit at the child health care together. Here we chose to handle this by adjusting for answering parent/s in the analyses.

- Page 8: Are the authors convinced that the measurement of the living arrangement is exact enough? What, for example, does "living only with" mean? Do these children have any contact to the other parent?

Response: We cannot be completely sure about what type of contact these children have with their second parent. But since we included the "living mostly with mother/father" categories we think this gave parents a broad range of alternatives.

- Page 9: Why did the authors choose the cut-off at 20%? Are there any theoretical or empirical arguments?

Response: In the revised version we choose to change this cut-off to 16 instead of 17. Parents who choose to score a 3 (neither agree or disagree) for at least one of the four items are now dichotomized as reporting low coparenting quality.

- Page 9: Why did the authors not use multiple imputation?

Response: As explained in the response to the first reviewer, we chose to follow the SDQ guideline when imputing the missing values. We have now conducted additional analyses using multiple imputation which resulted in the same findings (the results are reported as sensitivity analyses in the manuscript). For details please see our response to the first reviewer.

- Page 10: 95% of the sample are nuclear families. Isn't the proportion of separated families too low for a Swedish sample?

Response: According to a report from Statistics Sweden in 2018 on evaluation of the measurements of parental separation the risks for measurement errors in the official statistics is highest for the youngest children. According to their adjusted calculations about 8-10 percent of the 3-year-olds have separated parents. Since our sample is slightly skewed towards highly educated parents this may explain the lower percentage here. Hypothetically parents may also be living together to a higher extent than is reported in the civil registers, there are economic incentives with regards to social welfare benefits to report separation. (Statistics Sweden. Background material about demography, children and family. 2018.1.)

- Page 15: The authors only controlled for children's gender, parents' age, education, country of birth and coparenting quality. What about siblings, stepparents, working, hours, time since separation, and so on?

Response: The clinical context of this study made it necessary for us to limit the collection of information. We choose covariates that in previous studies have been found to be the most important for children's mental health in different living arrangements in a Swedish context. Unfortunately, the number of missing values was high on some covariates. In our new analyses we therefor decided to keep only maternal education, gender of responding parent and child gender. We do not have data on step parents or working hours. Sibling is a difficult concept since number of siblings, type of siblings (biological/step/half) have different implications for children's wellbeing and is outside the main focus of this paper.

- Page 22: The number of cases in some categories are rather small. Multivariate analysis with 2 cases are not possible or useful. The authors should consider deleting controls with extremely low number of cases in subgroups (like country of birth).

Response: We choose to follow this advice and have now excluded covariates as described above.

- Page 24: The variable living arrangements should be moved to the first row. Why did the authors not display standardized coefficients and *** to indicate significant effects? This would be much easier to read.

Response: We have now changed the tables according to this suggestion.

Reviewer: 3
William Fabricius, Arizona State University, USA

Comments to the Author
This study has potential for publication, but in my opinion the authors need to address some important issues.

1. The first is that the data clearly show an interaction between living arrangements and coparenting, but the authors do not make it clear to readers, and do not interpret it sufficiently. The means for living arrangements and coparenting quality in Table 2 (adjusted for the covariates) should be placed in a Figure so that readers can see the interaction. As in all cases where main effects are qualified by an interaction, the interaction is the effect that should be focused on, not the main effects. However, the authors do not discuss the interaction findings in the Discussion. Instead, they focus on the main effects; i.e., the finding that of the two main effects, coparenting accounts for more variance.

The authors do not include a regression model with the interaction term, and do not offer a rationale for not doing so. That is unusual, and contributes to the downplaying of the interaction. Instead, they proceed directly in Table 4 to testing the pairwise comparisons among the eight means in the interaction. They do this in a separate regression model, using "intact/high coparenting" as the reference category, which of course is an arbitrary, if justifiable, choice. What those pairwise comparisons leave out, however, are the simple effects tests of living arrangements on child problems at high coparenting quality and at low coparenting quality, as well as tests of high versus low coparenting at each level of living arrangements other than intact families.

It is clear that living arrangements have no relation to child problems when coparenting is low quality; all those children have similarly high levels of problems. But that is not the case when coparenting is high quality. Then joint physical custody children have as few problems as children in intact families, and problems increase as children live proportionally more time with one parent, up to the point where children living with only one parent have as many problems when their parents are cooperating well as when their parents are cooperating poorly!

The authors' conclusion (p. 13) that "Once we accounted for coparenting quality, there were nothing but minimal differences in child mental health in different family arrangements" is based not on the interaction, but on the finding that of the two main effects, coparenting accounted for more variance. The interaction, however, shows the complete picture, including the fact that in one-parent families, there was no difference in child mental health in high versus low coparenting quality families, and the fact that in high quality coparenting, there were large differences in child mental health associated with different family arrangements.

My advice would be to analyze the data with analysis of covariance. The independent variables are categorical, so there is no reason not to, and it would be much clearer to readers. The interaction would then be clearly tested, and could be followed up with simple-effects tests at each level of coparenting, as well as post-hoc tests at each level of living arrangements to give the complete picture.

Response: Thank you for this very insight- and helpful comment! In the revised version we have now included a regression analyses (table 3) where we adjust for coparenting as a continuous variable in model 3 and, as shown in Figure 1, an ANCOVA analysis.

2. It seems to me that we should worry about self-serving bias regarding the coparenting quality scale. At recruitment, parents were informed that the survey would be used to individualize their child's health visit, so therefore they knew their responses were not anonymous. Relatedly, the scale was not normally distributed, with most responses clustered at the high end, which also suggests self-serving bias. Is nonnormality unusual for a coparenting scale? Please explain to readers.

The authors frankly discuss the limitations of this scale, which was shortened from the McHale and Kuersten-Hogan scale, but they should also mention that it needs to be validated with independent reports from both parents. Self-serving bias could probably account for the factor analysis finding of a single factor. The authors should compare the distributions obtained with the shortened and the full scale. They should tell the reader how the items were selected for the shortened scale.

Response: Thanks for this comment, we have accordingly revised the text under Methodological considerations in the discussion, please see lines 477-89, page 13.

Given that only 20% were categorized as low quality, and all the rest as high quality, how are we to conceptualize them? If self-serving bias is at work, then the 20% should probably be seen as "very low," and the other 80% as a combination of the other four quintiles that would have appeared had self-serving bias not been at work (i.e., "poor," "moderate," "good," and "very good"). If self-serving bias is not at work, and the other 80% were really all similarly good coparents, then the bottom 20% might only be slightly worse at coparenting.

I think the data make the former alternative more likely, because of the fact that in intact and joint custody families, those 20% of parents had children with a lot more problems. Thus, readers need to be reminded that the findings on child harm associated with low quality coparenting apply to only 20% of the population, and those are likely to be the families with the very lowest quality coparenting. This is important, because the effects of living arrangements apply to the remaining 80% of the population, which includes families at all levels of coparenting quality except the lowest level.

3. The authors should also explain to readers that this scale includes coparenting items as well as parent conflict items. The coparenting items are: ability to cooperate, support each other, and confide in/trust each other. The conflict item is: conflict related to children. Absence of cooperation is not conflict, but conflict related to their children surely is parent conflict. This relates to the point above about telling the reader how the items were selected.

Response: We have now explained how these aspects relate to child wellbeing on page 13, lines 479-84.

Minor points:
4. As I understand it, imputation of the scale mean is the least desirable way to handle missing data.

Please justify.

Response: As explained in the response to the first reviewer, we chose to follow the SDQ guideline when imputing the missing values. We have now conducted additional analyses using multiple imputation which resulted in the same findings (the results are reported as sensitivity analyses in the manuscript). For details please see our response to the first reviewer.

5. p. 14 The first sentence of the first full paragraph, beginning, "For children living with…" doesn't make grammatical sense.

Response: We have now changed the wording on line 437, page 12.

6. In Table 2, comparing intact and joint custody, how can the overall mean be higher in joint custody (7.26) than in intact (7.07), but the coparenting groups' means both be lower than in intact families?
7.07 7.26 increase
6.69 6.39 decrease
8.78 8.42 decrease

Response: A new version of table 2 is now included.

Editor in Chief
Comments to the Author:
Title - add " a cross-sectional parental survey"

Response: We have changed the title.

Conclusion delete "For the first time, " as our journal style asks all authors to avoid describing their study as "the first".

We have omitted these words.

VERSION 2 – REVIEW

REVIEWER	Reviewer name: Dr. William Fabricius Institution and Country: Arizona State University, Psychology, United States Competing interests: None
REVIEW RETURNED	05-Oct-2020

GENERAL COMMENTS	Page numbers are manuscript page numbers in the revision. Results: 1. Results (pp. 9 - 10) The following sentences could be clarified by the addition of the edits in CAPS: "Table 3 shows the results from linear regression models, which suggest that, IN MODEL 1, WHEN CONTROLLING FOR CHILD GENDER AND PARENT RESPONDENT, 3-YEAR-OLDS living mostly or only with one parent, compared with those in JPC, had more mental health problems. However, 3-YEAR-OLDS in intact families were not significantly different from those in JPC in terms of mental health. When MATERNAL EDUCATION WAS ADDED TO Model 2, the differences between those living only and mostly with one parent and those in JPC were attenuated. Coefficients remained significant, however, indicating that only some of the differences were explained by the background characteristics." 2. Results (p. 10) The reader needs some guidance to understand the above findings from Model 2.

The authors need to point out that, as shown already in Table 1, the rates of low co-parenting show a “staircase” increase from 10% (Intact), 34% (Joint Physical Custody), 54% (Mostly with 1 Parent), to 66% (Only with 1 Parent). Thus, one would expect to find, in Model 2, that child problems (SDQ) would show something resembling a similar pattern of “staircase” increase. Surprisingly, however, Joint and Intact did not differ; i.e., 3-year-olds had similar low levels of problems in both. Problems increased in only the Mostly with 1 Parent and Only with 1 Parent living arrangements. Thus, it appears that the living arrangement of Joint Physical Custody buffered or protected 3-year-olds in some way from the effects of the higher rate (34%) of low co-parenting in Joint Physical Custody compared to Intact families (10%).
It might be sufficient to include only the above guidance about the Model 2 findings here (p. 10) in the Results. But then in the Discussion the authors will need to offer some explanation or alternate hypotheses for the Model 2 findings; i.e., what could it be about the family dynamics in Joint Physical Custody that meant that a higher rate (34%) of low co-parenting than in Intact families (10%) did not translate into more child problems among 3-year-olds in Joint Physical Custody?

3. Results (p. 10) The reader also needs some guidance to understand the findings from Model 3, in which co-parenting is controlled for (i.e., “Instead, differences between children in intact families and JPC became significant suggesting that children living in JPC showed fewer problems compared to children in intact families after controlling for coparenting quality.”)
This is a really surprising finding; i.e., that in Model 3, JPC showed fewer problems than intact. But the authors gloss over it, and move right on to the ANCOVA. Instead, the authors need to point out to the reader that Model 3 suggests that JPC does more than buffer or protect 3-year-olds from effects of low co-parenting (as suggested by Model 2); it actually appears to confer benefits in terms of lower behavioral problems in 3-year-olds in JPC than intact families, which are revealed when in Model 3 the authors take out the other effects on child problems due to low co-parenting, child gender, parent respondent, and maternal education.
As with Model 2, it might be sufficient to include only the above guidance about the Model 3 findings here (p. 10) in the Results. But then in the Discussion the authors will need to offer some explanation or alternate hypotheses for the Model 3 findings; i.e., what could it be about JPC that might lead to lower rates than in intact families of child problems among 3-year-olds? This is especially important to do, because the present study looks at 3-year-olds, and as the authors undoubtably know, there is substantial, current controversy about Joint Physical Custody for very young children. One side of that debate claims it is harmful to very young children, so the current findings need to be placed in that context and interpreted as carefully and as correctly as possible.

4. Results (pp. 10 -11) The findings of the ANCOVA could be presented more clearly for readers, and that would be helped by conducting the post hoc tests more conventionally.

The authors don’t present the tests for the two main effects, but only for the interaction (“The factorial ANCOVA analysis revealed a significant interaction effect for children’s living arrangement by

	coparenting quality F(3, 12 834) = 3.770, p < .01.”), and that is appropriate because it is the interaction that should be interpreted (and main effects are tested in the regressions, anyway). However, the statement on pp. 10-11, “Post hoc comparisons yielded no significant differences between children’s living arrangement, while low coparenting quality was associated with more mental health problems,” is actually a statement of the two main effects, not of the post hoc comparisons. This statement is therefore confusing to readers and should be deleted. The most conventional and clearest way of exploring an interaction such as this (recommended by Keppel, G. (1991). Design and analysis: A researcher's handbook (3rd ed.). Prentice-Hall, Inc.) includes two steps. The first step is to conduct two one-way ANCOVAs, one on the low coparenting group, and one on the high coparenting group. This allows the authors to look for an overall effect of the four family types at each level of coparenting. If there is an effect of family type, then the authors can do post-hoc t-tests to see which groups differ, and the advantage here is that the error term for these t-tests is the pooled error term of the four groups, which is better than doing individual t-tests with the error term coming only from the two groups involved in the test (which appears to be what the authors have done). The authors have determined beforehand, shown in their regressions, that they are interested in the comparison between the JPC group and each of the other three groups, so within each of these simple effects ANCOVAs they will be doing only 3 post hoc t-tests. Given what the authors report on p. 11, and what is shown in Figure 1, I assume that the simple effects ANCOVA at low coparenting will reveal an effect of family type, and that the post hoc t-tests will show lower child problems in JPC than in intact. The authors imply that their other t-tests showed no significant differences between JPC and the other two living arrangement groups, and that might still be the case using the pooled error term from the simple effects ANCOVA. If so, I would worry that those t-tests might lack power, given that the Ns for the Mostly with 1 Parent and Only with 1 Parent groups are each half of the N for JPC, and the variances are higher. In that case, I would recommend taking the extra, exploratory step, and of course clearly justifying it to the reader, of combining the Mostly with 1 Parent and Only with 1 Parent groups (in a new ANCOVA) and re-doing the post hoc t-test between JPC and the combined group. I think this would be well-warranted given that none of the other analyses suggested any differences between these two groups, and given the importance of getting the best understanding we can of this interaction. I assume that the simple effects ANCOVA at high coparenting will also reveal an effect of family type, and that the post hoc t-tests will show no difference in child problems between JPC and intact. This will clarify the finding in Model 3, by showing that the benefit in terms of lower behavioral problems in 3-year-olds in JPC than in intact families is confined to the low coparenting group. That should help the authors in offering, in the Discussion, some explanation or alternate hypotheses for this more focused finding of benefit of JPC. Next, if t-tests show no significant differences between JPC and the other two living arrangement groups here at

	high coparenting, then I would have the same recommendations as before, to re-run the ANCOVA and t-tests using the two other two groups combined. With these two simple effects ANCOVAs and post hoc tests, the reader will easily be able to see the pattern of 3-year-olds' behavioral problems across the different family types at both levels of coparenting. It does appear that at both levels, children in JPC fare best. The second step is to conduct post hoc t-tests between high and low coparenting within each family type. This shows that the benefits of coparenting, seen in intact and JPC groups, appear to be lost in the Mostly with 1 Parent and Only with 1 Parent groups. It is lost because even with high coparenting in the Mostly with 1 Parent and Only with 1 Parent groups, 3-year-olds still have high levels of problems. So it appears that high coparenting does not buffer or protect children in the Mostly with 1 Parent and Only with 1 Parent group. The authors could also combine these two groups for this post hoc test, to be sure the lack of statistical significance is not due to low power. Discussion: I have read the Discussion several times, and I'm pretty sure that readers will find it confusing or unnecessarily difficult. I think this is simply because the Discussion was written before the ANCOVA test of the interaction was done. The Discussion does need a major re-write in light of the interaction findings portrayed in Figure 1. In addition to the suggestions above for explaining the regression findings and the ANCOVA tests, the Discussion should correct the following statements: 1. p. 11, "Regardless of family type, children with parents with low quality coparenting relationships had more mental health problems than their counterparts in families where parents reported high coparenting quality." The interaction now shows this is not true – there is no significant difference between high and low coparenting in the Mostly with 1 Parent and Only with 1 Parent groups. 2. p. 11 "Schoppe-Sullivan & Weldon (24) have previously shown that coparenting quality may moderate the association between children's socioemotional development (such as self-control) and later externalizing behaviours." The relation between self-control and externalizing is so tangentially related to the current study that this sentence will be confusing and potentially misleading to readers. It does not serve to explain the current findings. 3. p. 11 "The impact of parental conflict on children's wellbeing is well established in the literature." Why bring up parent conflict, when you are studying coparenting, which you have explained in the Introduction is a different construct? This will also be confusing and distracting to readers. Focus on coparenting. 4. p. 12 "… coparenting quality may be more important for young children's mental health than their family form per se." If what you mean here is simply that coparenting is also important in intact families, then be clear and just say that; you don't need to say that

	it may be more important. You don't really have any reason to speculate that. 5. p. 12 "Coparenting quality differed between parents in different post-divorce family types and among those still cohabiting, which may explain differences in how children fare across family forms." No, you controlled for coparenting in Model 3, and JPC had LESS child problems than intact. In addition, the simple effects ANCOVAs show differences among groups at each level of coparenting. Your finding of an interaction between coparenting and family types shows that there are effects associated with both. 6. p. 12 "An interesting finding was that fewer child mental health problems were reported in low coparenting quality families when parents shared custody in two separate homes than as an intact family." Yes, this is one of the important aspects of the interaction, but the authors say nothing more about it, rather they immediately bring up references to parent conflict, divorce (not living arrangements), and economic hardship, all of which are off the point, and do not help the reader understand the current findings. Implications: 1. p. 14 "This study does not support claims that JPC is psychologically harmful for preschool children." Cite those claims for the reader. Conclusion: 1. p. 14 "This study shows that coparenting quality may explain a large part of the differences in child psychological problems between young children in different living arrangements, and explains more than the actual living arrangement." No, it doesn't explain more than the living arrangement; you have found an interaction between the living arrangements and coparenting. Please explain clearly to your readers what Figure 1 tells them. When you consider what might explain the apparent advantage of JPC that Figure 1 shows at both low and high coparenting, you might want to consider what these 3-year-olds' experiences might have been like in their living arrangements in the three years prior to their having participated in this study. Did JPC in those early years help each parent engage in better parenting, due to less stress of having to be the sole or primary caregiver, for example?

VERSION 2 – AUTHOR RESPONSE

Dear editor.

We are very grateful for this very helpful review and have chosen to follow the suggestions both regarding the ANCOVA analyses and the revision of the discussion, see below for detailed comments:

Results:
1. Results (pp. 9 - 10) The following sentences could be clarified by the addition of the edits in CAPS: "Table 3 shows the results from linear regression models, which suggest that, IN MODEL 1, WHEN CONTROLLING FOR CHILD GENDER AND PARENT RESPONDENT, 3-YEAR-OLDS living mostly or only with one parent, compared with those in JPC, had more mental health problems. However, 3-YEAR-OLDS in intact families were not significantly different from those in JPC in terms of mental health. When MATERNAL EDUCATION WAS ADDED TO Model 2, the differences between those living only and mostly with one parent and those in JPC were attenuated. Coefficients remained significant, however, indicating that only some of the differences were explained by the background characteristics."

Thanks, we have changed the wording accordingly.

2. Results (p. 10) The reader needs some guidance to understand the above findings from Model 2. The authors need to point out that, as shown already in Table 1, the rates of low co-parenting show a “staircase” increase from 10% (Intact), 34% (Joint Physical Custody), 54% (Mostly with 1 Parent), to 66% (Only with 1 Parent). Thus, one would expect to find, in Model 2, that child problems (SDQ) would show something resembling a similar pattern of “staircase” increase. Surprisingly, however, Joint and Intact did not differ; i.e., 3-year-olds had similar low levels of problems in both. Problems increased in only the Mostly with 1 Parent and Only with 1 Parent living arrangements. Thus, it appears that the living arrangement of Joint Physical Custody buffered or protected 3-year-olds in some way from the effects of the higher rate (34%) of low co-parenting in Joint Physical Custody compared to Intact families (10%).
It might be sufficient to include only the above guidance about the Model 2 findings here (p. 10) in the Results. But then in the Discussion the authors will need to offer some explanation or alternate hypotheses for the Model 2 findings; i.e., what could it be about the family dynamics in Joint Physical Custody that meant that a higher rate (34%) of low co-parenting than in Intact families (10%) did not translate into more child problems among 3-year-olds in Joint Physical Custody?

We prefer to only present the actual results in the result section and have instead chosen to comment on this in the discussion.

3. Results (p. 10) The reader also needs some guidance to understand the findings from Model 3, in which co-parenting is controlled for (i.e., “Instead, differences between children in intact families and JPC became significant suggesting that children living in JPC showed fewer problems compared to children in intact families after controlling for coparenting quality.”)
This is a really surprising finding; i.e., that in Model 3, JPC showed fewer problems than intact. But the authors gloss over it, and move right on to the ANCOVA. Instead, the authors need to point out to the reader that Model 3 suggests that JPC does more than buffer or protect 3-year-olds from effects of low co-parenting (as suggested by Model 2); it actually appears to confer benefits in terms of lower behavioral problems in 3-year-olds in JPC than intact families, which are revealed when in Model 3 the authors take out the other effects on child problems due to low co-parenting, child gender, parent respondent, and maternal education.
As with Model 2, it might be sufficient to include only the above guidance about the Model 3 findings here (p. 10) in the Results. But then in the Discussion the authors will need to offer some explanation or alternate hypotheses for the Model 3 findings; i.e., what could it be about JPC that might lead to lower rates than in intact families of child problems among 3-year-olds? This is especially important to do, because the present study looks at 3-year-olds, and as the authors undoubtably know, there is substantial, current controversy about Joint Physical Custody for very young children. One side of that debate claims it is harmful to very young children, so the current findings need to be placed in that context and interpreted as carefully and as correctly as possible.

We are grateful for these helpful comments and have now extended the discussion accordingly.

4. Results (pp. 10 -11) The findings of the ANCOVA could be presented more clearly for readers, and that would be helped by conducting the post hoc tests more conventionally.

The authors don’t present the tests for the two main effects, but only for the interaction (“The factorial ANCOVA analysis revealed a significant interaction effect for children’s living arrangement by coparenting quality $F(3, 12\,834) = 3.770$, $p < .01$.”), and that is appropriate because it is the interaction that should be interpreted (and main effects are tested in the regressions, anyway).

However, the statement on pp. 10-11, “Post hoc comparisons yielded no significant differences between children’s living arrangement, while low coparenting quality was associated with more mental health problems,” is actually a statement of the two main effects, not of the post hoc comparisons. This statement is therefore confusing to readers and should be deleted.

The most conventional and clearest way of exploring an interaction such as this (recommended by

Keppel, G. (1991). Design and analysis: A researcher's handbook (3rd ed.). Prentice-Hall, Inc.) includes two steps.
The first step is to conduct two one-way ANCOVAs, one on the low coparenting group, and one on the high coparenting group. This allows the authors to look for an overall effect of the four family types at each level of coparenting. If there is an effect of family type, then the authors can do post-hoc t-tests to see which groups differ, and the advantage here is that the error term for these t-tests is the pooled error term of the four groups, which is better than doing individual t-tests with the error term coming only from the two groups involved in the test (which appears to be what the authors have done). The authors have determined beforehand, shown in their regressions, that they are interested in the comparison between the JPC group and each of the other three groups, so within each of these simple effects ANCOVAs they will be doing only 3 post hoc t-tests.

Given what the authors report on p. 11, and what is shown in Figure 1, I assume that the simple effects ANCOVA at low coparenting will reveal an effect of family type, and that the post hoc t-tests will show lower child problems in JPC than in intact. The authors imply that their other t-tests showed no significant differences between JPC and the other two living arrangement groups, and that might still be the case using the pooled error term from the simple effects ANCOVA. If so, I would worry that those t-tests might lack power, given that the Ns for the Mostly with 1 Parent and Only with 1 Parent groups are each half of the N for JPC, and the variances are higher. In that case, I would recommend taking the extra, exploratory step, and of course clearly justifying it to the reader, of combining the Mostly with 1 Parent and Only with 1 Parent groups (in a new ANCOVA) and re-doing the post hoc t-test between JPC and the combined group. I think this would be well-warranted given that none of the other analyses suggested any differences between these two groups, and given the importance of getting the best understanding we can of this interaction.

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The second step is to conduct post hoc t-tests between high and low coparenting within each family type. This shows that the benefits of coparenting, seen in intact and JPC groups, appear to be lost in the Mostly with 1 Parent and Only with 1 Parent groups. It is lost because even with high coparenting in the Mostly with 1 Parent and Only with 1 Parent groups, 3-year-olds still have high levels of problems. So it appears that high coparenting does not buffer or protect children in the Mostly with 1 Parent and Only with 1 Parent group. The authors could also combine these two groups for this post hoc test, to be sure the lack of statistical significance is not due to low power.

We have re-analysed our results according to these fruitful suggestions.

Discussion:
I have read the Discussion several times, and I'm pretty sure that readers will find it confusing or unnecessarily difficult. I think this is simply because the Discussion was written before the ANCOVA test of the interaction was done. The Discussion does need a major re-write in light of the interaction findings portrayed in Figure 1.

The discussion is now completely rewritten with the inclusion of all the suggested changes. We thank the reviewer for being so helpful!

In addition to the suggestions above for explaining the regression findings and the ANCOVA tests, the

Discussion should correct the following statements:

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1. p. 14 “This study does not support claims that JPC is psychologically harmful for preschool children.” Cite those claims for the reader.

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Editor in Chief
Comments to the Author:
Detailed helpful comments from the reviewer
Rename "What this study hopes to add" to "What this study adds"

This is now altered.

Yours sincerely,

Anders Hjern

VERSION 3 – REVIEW

REVIEWER	Reviewer name: Dr. William Fabricius Institution and Country: Arizona State University, Psychology, United States Competing interests: None
REVIEW RETURNED	02-Mar-2021

GENERAL COMMENTS	The authors have successfully responded to my concerns and suggestions. I only note 3 typos: p. 13/44 line 261 “families with those” should read “families and those” p. 15/44 line 330 “quality for both for” should read “quality for both” p. 15/44 line 339 shouldn’t “with divorced parents” instead say “with JPC parents” because there was no effect of co-parenting among the mostly/only one parent groups?

VERSION 3 – AUTHOR RESPONSE

Dear editor.

We have made the minor revisions asked for in this revised manuscript. We hope that you now found this manuscript fit to publish. I have been in touch with Inti about how to proceed with the article so that it is presented with two main authors, since this is not accepted by the website of the journal. Inti said that this is something to be taken care of after the article has been accepted for publication. I would very much appreciate advice on how to get this done.

Yours sincerely, Anders Hjern