

Early parenting is associated with the development of neural reward responsiveness from childhood through adolescence

Corresponding Author: Dr Kaylin Hill

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Hill,

Thank you for your patience during the peer-review process. Your manuscript titled "Early parenting is associated with the development of neural reward responsiveness from childhood through adolescence" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest but raised some important points. We are interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file.

Editorially, we consider it important that the revision conducts the additional analyses requested by the reviewers, ensures the interpretations align with the evidence, and that limitations are well described.

As you revise the manuscript in response to these issues, please also implement all requests in the attached Mandatory Revision Requests document. All requirements listed in this document need to be fully met, or the work will be returned to you for further revisions without peer review. This workflow is in place to increase the likelihood that the paper will be accepted for publication. It reduces the number of rounds of revision (and review) and ensures that the reviewers vet a version of the article that is compliant with journal policies. If you have any questions regarding the required revisions, please contact the journal prior to resubmission to avoid a negative outcome.

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- Revised manuscript
- Point-by-point response to the referees' comments
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We hope to receive your revised paper within 8 weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Jennifer Bellingtier

Jennifer Bellingtier, PhD
Senior Editor
Communications Psychology

REVIEWER EXPERTISE:
developmental psychology, reward processing, EEG

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

The authors present the results of a study examining whether early environmental factors, specifically positive and negative parenting behaviors and early stress exposure at age 3, predict the developmental trajectory of neural reward responsiveness from late childhood to late adolescence (ages 9 – 18), measured by reward positivity (RewP to win). The study used a longitudinal design with RewP to win measured at four waves (age 9, 12, 15, 18). They found a quadratic trajectory of RewP to win, which increased from ages 9 to 15 and decreased from 15 to 18. Additionally, they found that positive parenting at age 3 predicted steeper increases in RewP to win trajectories whereas negative parenting predicted weaker increases. No association was found between stress exposure and RewP to win trajectory. The question is important and interesting, and a longitudinal design is powerful to ask such questions. The use of behavioral coding from lab-based interactions and interviews reflects a commitment to capture objective parenting behaviors and stress exposure. Despite these strengths, there are concerns that warrant further attention, as discussed below.

The manuscript focuses exclusively on the conditional ERP to win trials. In sensitivity analyses covarying for RewP to loss, only the negative parenting findings remained statistically significant, which raises questions whether the effects are reward specific. Examining parallel models with RewP to loss as the outcome variable could strengthen the manuscript and help with interpretation.

Additionally, a notable concern is that prior work using this sample has demonstrated associations between parental, particularly maternal, depression and offspring reward functioning (e.g., Kujawa et al., 2014). Given that maternal depression is also associated with some of the parenting behaviors reported in this study, it is important to clarify whether the reported effects are independent of maternal depression. The authors should adjust for maternal diagnoses or depressive symptoms, or at least discuss this potential confound as a limitation.

Another related limitation concerns the statistically significant correlation between RewP and child sex. The manuscript would benefit from additional interpretation of these correlations and adjusting for child sex.

It would improve transparency to provide a complete table (or other reporting method) of model estimates, including main linear and quadratic effects of time prior to inclusion of interaction terms.

Please include descriptive statistics for parenting measures (e.g., mean, standard deviation, and range). In the correlation table, values such as .04 and .02 are difficult to interpret—are these centered means? Clarifications are needed. The authors note that their stress exposure variable had limited range—did the parenting variables have more variability?

Please also report internal consistency for the RewP to win variable at each time point.

Additionally, the manuscript reports the use of a 500 ms baseline correction window for RewP. A 200 ms baseline is more typical in RewP research. The authors should justify this choice.

The manuscript indicates that outliers were deleted. Given increasing recommendations against deletion of outliers and in favor of robust statistical approaches, additional justification for this decision is needed (e.g., Karch, 2023). Alternatively, the authors could demonstrate that results are robust to analytic approaches less sensitive to non-normality or extreme values.

Generalizability should be discussed, as the sample is predominantly White.

Minor point:

A closing square bracket is missing on page 8, line 146.

Reviewer #2 (Remarks to the Author):

In this manuscript, "Early parenting is associated with the development of neural reward responsiveness from childhood through late adolescence", the authors probe the relationship between parental parenting behavior and stress exposures for the child between 0-3 years on reward responsivity as measured with ERP, at ages 9, 12, 15, and 17 years. This is a powerful study looking at developmental trajectories of reward, and the impact of both parental behaviors and exposure to stressors in early childhood. While prior studies have looked at acute effects of stress exposures on reward processing, and early parenting impact on early reward, this longitudinal study allows for a developmental profile to be generated. This is a novel study, and the findings are quite powerful, the statistics are thorough, but there are a few minor concerns that dampen the overall enthusiasm for this specific study.

Concerns:

- 1) The authors draw a lot of attention in the introduction to prior studies showing an impact of parental affect (both positive and negative) on reward responsivity. However, the scales the authors are using don't directly measure parental affect. If the authors point of view in the introduction is to draw attention to the impact a parent may have, whether parenting style or affect, then, a little more direct statement of that would be helpful for the readers. As it is currently written, it's clear that this is a new, and important finding. But, how it integrates with existing literature is less clear given this study hasn't measured affect directly.
- 2) The authors have utilized statistical models to probe the role of either positive parenting or negative parenting on reward responsivity. However, the authors haven't justified why they didn't include both the positive and negative parenting scores for each parent in the statistical models. It's possible that the behaviors that comprise the negative parenting construct are important parental behaviors in specific situations. It may be possible that different combinations of negative and positive parenting across different tasks produce different reward responsivity. Similarly, a more comprehensive picture may include stress, and positive parenting, and negative parenting into the same statistical model. Ultimately, it is not understood what the justification is for running 3 different models, if they're potentially connected, and all impacting the child.
- 3) This is an incredibly time intensive longitudinal study that was quite successful. There is a gap in time for data though between 3 and 9 years of age, even though the data was collected when kids were 6yrs old. What was/is the justification for not collecting ERP data on those kids at 6 years old, especially since the authors provide references stating that reward response is evident from ages 4/5 through adulthood?

Methodology questions:

- 1) Were only moms answers used? Uncertain whether both moms and dads were asked to complete forms, or if it was just moms who completed surveys and performed parenting task
- 2) The authors provide different sample sizes that were used for statistical analyses throughout the paper, but it is difficult to follow where the drop outs occurred, and reasons for significantly different n's across tasks. N= 465 with data on stress and EEG at each time point, n= 458 who completed dyadic parenting tasks, 13 removed for ERP data- resulting in n=383, 319, and 284 at different time points. Where are the other several dozens (difference to n=458?). Perhaps a figure documenting sample size distribution across tasks would help here.

Recommendations:

- 1) While the demographic information is provided in the text, it would be better suited as a Table.
- 2) The information in the footnote on the bottom of page 14 really belongs in the main body of the text.

Reviewer #3 (Remarks to the Author):

This study addresses an important developmental question regarding how early caregiving environments may shape the development of neural reward systems from childhood to adolescence.

The work has several very notable strengths, including a longitudinal dataset and design, multimethod assessment of early environmental factors, and a large sample. The main findings provide suggestive and interesting evidence that early caregiving experiences may be associated with individual differences in trajectories of reward sensitivity from childhood to adolescence.

However, several conceptual and methodological issues currently weaken the introduction and discussion of the main findings. Addressing the concerns outlined above, particularly those related to the interpretation of environmental effects, the measurement of parenting, and conceptual clarity, would substantially improve the manuscript. I hereby provide a detailed report of all my recommendations and concerns below.

I therefore recommend overall major revision before the manuscript can be considered for publication.

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Version 1:

Decision Letter:

** Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your coauthors **

Dear Dr Hill,

Your manuscript titled "Early parenting is associated with the development of neural reward responsiveness from childhood through adolescence" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Psychology.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers and a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table". Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Jennifer Bellinger

Jennifer Bellinger, PhD
Senior Editor
Communications Psychology

REVIEWERS' EXPERTISE:

developmental psychology, reward processing, EEG

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for the opportunity to review the revised manuscript. The authors have made considerable effort to address the comments raised in the previous round of review, and I am mostly satisfied with the revisions. However, the revision and responses still have one major issue:

The inclusion of additional sensitivity analyses is appreciated. However, the results seem to suggest that the interaction effects are considerably less robust after covarying for RewP to loss or using the difference score, and the pattern of results predicting RewP to loss, despite being nonsignificant, appear similar to the results predicting RewP to win. These follow-up analyses suggest that the interaction effects are less specific to RewP to win than stated in the manuscript. The authors should soften the language in the abstract and discussion to better reflect the results found across analyses.

Reviewer #2 (Remarks to the Author):

The authors provide an excellent response to reviewers, and made substantial edits to the manuscript addressing concerns that were provided. Unfortunately, neither of the 2 new tables (Tables 1 and 2) that were created are visible to me, and I had specifically requested those tables. Please make those visible within the review file. There are a few minor issues that should be addressed:

1. The authors state that participants received \$400 per wave, which seems an astronomical compensation. If correct, please explain why payments were so high- otherwise, please revise.
2. In the new paragraph labeled "Maternal Lifetime Depression Diagnosis", the description of who administered the SCID should be similar in level of detail to the description for the PAPA. Currently, PAPA is succinct and is written, "trained interviewers", while the SCID says, master-level clinicians and upper-level doctoral students".
3. Additionally, there is a sentence in that same paragraph that "Interrater reliability was excellent". It's unclear what was being coded and scored for interrater reliability. Additionally, if there are reliability scores, those should be provided.
4. In the next to last paragraph of "EEG Data Collection and Processing", remove the sentence "See Table 2 for sample sizes across variables and waves", and insert (Table 2) at the end of the sentence that immediately precedes it, "after outlier removal.....variable and assessment waves (Table 2).
5. The fifth paragraph of the discussion section, consider the following revision, "...for example, home based daily assessments at multiple timepoints relate to reward response trajectories."

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Editor

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We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

RESPONSE: Thank you for your editorial review of our submission and for the opportunity to revise our work for publication in *Communications Psychology*. We are grateful for your and the reviewers' time and expertise and pleased that you saw the merit to this work. Please see specific responses to each of the reviewers' comments below.

Reviewer 1:

The authors present the results of a study examining whether early environmental factors, specifically positive and negative parenting behaviors and early stress exposure at age 3, predict the developmental trajectory of neural reward responsiveness from late childhood to late adolescence (ages 9 – 18), measured by reward positivity (RewP to win). The study used a longitudinal design with RewP to win measured at four waves (age 9, 12, 15, 18). They found a quadratic trajectory of RewP to win, which increased from ages 9 to 15 and decreased from 15 to 18. Additionally, they found that positive parenting at age 3 predicted steeper increases in RewP to win trajectories whereas negative parenting predicted weaker increases. No association was found between stress exposure and RewP to win trajectory. The question is important and interesting, and a longitudinal design is powerful to ask such questions. The use of behavioral coding from lab-based interactions and interviews reflects a commitment to capture

objective parenting behaviors and stress exposure. Despite these strengths, there are concerns that warrant further attention, as discussed below.

RESPONSE: Thank you for your time and expertise in reviewing our manuscript, and for your kind words regarding the importance of the aims, rigorous approach, and overall strength of the paper. We appreciate each of your comments and address each below.

The manuscript focuses exclusively on the conditional ERP to win trials. In sensitivity analyses covarying for RewP to loss, only the negative parenting findings remained statistically significant, which raises questions whether the effects are reward specific. Examining parallel models with RewP to loss as the outcome variable could strengthen the manuscript and help with interpretation.

RESPONSE: We appreciate the suggestion and now present these results in the supplemental materials. In summary, the RewP to loss demonstrated similar trajectories across time as the RewP to win (e.g., statistically significant main effects of linear and quadratic time); however, time did not interact with positive parenting, negative parenting, or early stress exposure when predicting the RewP to loss. Together, these results suggest that observed effects with early experiences are specific to the RewP to win.

Additionally, a notable concern is that prior work using this sample has demonstrated associations between parental, particularly maternal, depression and offspring reward functioning (e.g., Kujawa et al., 2014). Given that maternal depression is also associated with some of the parenting behaviors reported in this study, it is important to clarify whether the reported effects are independent of maternal depression. The authors should adjust for maternal diagnoses or depressive symptoms, or at least discuss this potential confound as a limitation.

RESPONSE: Thank you for this important point and opportunity to investigate the role of maternal diagnoses. To address this point and the next, we now present models covarying for maternal lifetime depression diagnosis and child sex. All of the results remained consistent with our original submission, with the exception of the preliminary model estimated without interaction terms for positive parenting. Specifically, results from the preliminary model covarying for maternal lifetime depression and child sex showed that greater positive parenting at baseline was associated with higher average RewP to win at each time point. These results are described on page 16:

“In a preliminary model estimated without interaction terms, such that the only predictors of the RewP to win were the linear effect of time ($\pi_{1j} = 4.58$, 95% CI [3.56, 5.61], $z = 8.78$, $p < .001$), the quadratic effect of time ($\pi_{2j} = -1.36$, 95% CI [-1.69, -1.03], $z = -8.01$, $p < .001$), child sex, maternal depression lifetime diagnosis, and positive parenting, positive parenting was a statistically significant predictor of the reward responsiveness intercept ($\beta_{01} = 0.30$, 95% CI [0.004, 0.60], $z = 1.98$, $p = .047$). Greater positive parenting at baseline was associated with higher average RewP to win overall. With interactions added between positive parenting and the linear and quadratic effects of time, a statistically significant interaction was observed between positive parenting and both the linear ($\beta_{11} = 0.51$, 95% CI [0.09, 0.93], $z = 2.39$, $p = .017$) and quadratic trends of time ($\beta_{21} = -0.17$, 95% CI [-0.32, -0.03], $z = -2.40$, $p = .016$).”

To maintain transparency to our original approach, we now present our original analyses, without these covariates, and their results in the supplement.

Another related limitation concerns the statistically significant correlation between RewP and child sex. The manuscript would benefit from additional interpretation of these correlations and adjusting for child sex.

RESPONSE: To address this point, we now present models covarying for maternal lifetime depression diagnosis and child sex. All of the results remained consistent with our original submission, with the exception of the preliminary model estimated without interaction terms for positive parenting. Specifically, results from the preliminary model covarying for maternal lifetime depression and child sex showed that greater positive parenting at baseline was associated with higher average RewP to win at each time point. These results are described on page 16:

“In a preliminary model estimated without interaction terms, such that the only predictors of the RewP to win were the linear effect of time ($\pi_{1j} = 4.58$, 95% CI [3.56, 5.61], $z = 8.78$, $p < .001$), the quadratic effect of time ($\pi_{2j} = -1.36$, 95% CI [-1.69, -1.03], $z = -8.01$, $p < .001$), child sex, maternal depression lifetime diagnosis, and positive parenting, positive parenting was a statistically significant predictor of the reward responsiveness intercept ($\beta_{01} = 0.30$, 95% CI [0.004, 0.60], $z = 1.98$, $p = .047$). Greater positive parenting at baseline was associated with higher average RewP to win overall. With interactions added between positive parenting and the linear and quadratic effects of time, a statistically significant interaction was observed between positive parenting and both the linear ($\beta_{11} = 0.51$, 95% CI [0.09, 0.93], $z = 2.39$, $p = .017$) and quadratic trends of time ($\beta_{21} = -0.17$, 95% CI [-0.32, -0.03], $z = -2.40$, $p = .016$).”

To maintain transparency to our original approach, we now present our original analyses, without these covariates, and their results in the supplement.

We also interpret the correlations between the RewP and child sex in the Results on page 16:

“Child sex shared small, statistically significant associations with the RewP to win at two time points. At child age 9, the RewP to win was negatively associated with child sex such that boys demonstrated a larger RewP to win; at age 18, the RewP to win was positively associated with child sex such that girls demonstrated a larger RewP to win.”

It would improve transparency to provide a complete table (or other reporting method) of model estimates, including main linear and quadratic effects of time prior to inclusion of interaction terms.

RESPONSE: We agree and have updated the manuscript and supplement for increased transparency in reporting all effects. We report the statistics for the main effects of time. Of note, however, we do not report the coefficients for explicit covariates in the models, as this can lead to the Table 2 Fallacy (Westreich & Greenland, 2013). In specific regard to the reviewer's point, we present the main linear and quadratic effects of time prior to inclusion of interaction terms for each model.

Positive parenting, page 16:

“In a preliminary model estimated without interaction terms, such that the only predictors of the RewP to win were the linear effect of time ($\pi_{1j} = 4.58$, 95% CI [3.56, 5.61], $z = 8.78$, $p < .001$), the quadratic effect of time ($\pi_{2j} = -1.36$, 95% CI [-1.69, -1.03], $z = -8.01$, $p < .001$), child sex, maternal depression lifetime diagnosis, and positive parenting, positive parenting was a statistically significant predictor of the reward responsiveness intercept ($\beta_{01} = 0.30$, 95% CI [0.004, 0.60], $z = 1.98$, $p = .047$). Greater positive parenting at baseline was associated with higher average RewP to win overall. With interactions added between positive parenting and the linear and quadratic effects of time, a statistically significant interaction was observed between positive parenting and both the linear ($\beta_{11} = 0.51$, 95% CI [0.09, 0.93], $z = 2.39$, $p = .017$) and quadratic trends of time ($\beta_{21} = -0.17$, 95% CI [-0.32, -0.03], $z = -2.40$, $p = .016$).”

Negative parenting, page 17:

“In contrast to positive parenting, negative parenting was not a statistically significant predictor of the reward responsiveness intercept in the model without interactions ($\beta_{01} = -0.10$, 95% CI [-0.54, 0.34], $z = -0.45$, $p = .653$), where the linear effect of time was $\pi_{1j} = 4.59$, 95% CI [3.56, 5.61], $z = 8.78$, $p < .001$ and the quadratic effect of time was $\pi_{2j} = -1.36$, 95% CI [-1.69, -1.03], $z = -7.99$, $p < .001$. However, the addition of interactions between negative parenting and time trends revealed that higher negative parenting scores predicted a weaker linear trend ($\beta_{11} = -0.77$, 95% CI [-1.38, -0.16], $z = -2.46$, $p = .014$) and a weaker quadratic trend ($\beta_{21} = 0.27$, 95% CI [0.07, 0.48], $z = 2.62$, $p = .009$) across time.”

Early stress exposure, page 17:

“Stress exposure also did not significantly predict the reward responsiveness intercept in the preliminary model without interaction terms ($\beta_{01} = -0.14$, 95% CI [-0.5, 0.28], $z = -0.64$, $p = .523$), where the linear effect of time was $\pi_{1j} = 4.58$, 95% CI [3.56, 5.60], $z = 8.82$, $p < .001$ and the quadratic effect of time was $\pi_{2j} = -1.36$, 95% CI [-1.69, -1.02], $z = -8.01$, $p < .001$. In contrast with the parenting predictors, we did not observe a statistically significant interaction between stress exposure and either the linear ($\beta_{11} = -0.13$, 95% CI [-0.71, 0.45], $z = -0.44$, $p = .663$) or quadratic trends of time ($\beta_{21} = 0.05$, 95% CI [-0.14, 0.23], $z = 0.47$, $p = .639$); see Figure 5.”

Westreich, D., & Greenland, S. (2013). The table 2 fallacy: Presenting and interpreting confounder and modifier coefficients. *American Journal of Epidemiology*, 177(4), 292-298.

Please include descriptive statistics for parenting measures (e.g., mean, standard deviation, and range). In the correlation table, values such as .04 and .02 are difficult to interpret—are these centered means? Clarifications are needed. The authors note that their stress exposure variable had limited range—did the parenting variables have more variability?

RESPONSE: Descriptive statistics for parenting measures are included in Table 3. These values reflect the average of standardized scores across the included coded behaviors. Looking at the standard deviation of these scores, observed positive parenting had greater variability relative to observed negative parenting, which was similar to early stress exposure.

Please also report internal consistency for the RewP to win variable at each time point.

RESPONSE: Thank you for the opportunity to provide this information. We have now added the internal consistency of the RewP to win at each wave of data collection to the Method section. On page 12:

“The RewP to win demonstrated acceptable split-half reliability at each wave of data collection (Age 9: $r_{SB}(414) = .63$, Age 12: $r_{SB}(396) = .83$, Age 15: $r_{SB}(372) = .87$, Age 18: $r_{SB}(283) = .87$).”

Additionally, the manuscript reports the use of a 500 ms baseline correction window for RewP. A 200 ms baseline is more typical in RewP research. The authors should justify this choice.

RESPONSE: While papers focused on the RewP have used baseline correction windows ranging from 100 ms (e.g., Kujawa et al., 2019) to 500 ms (e.g., Goldstein et al., 2019) and we are unaware of research systematically examining the effects of the specific baseline correction window on RewP measures, we agree with the reviewer that the 200 ms baseline correction window seems to be the most common (e.g., Brown & Cavanagh, 2018; Foti et al., 2014; Hennefield et al., 2022; Heydari & Holroyd, 2016; Hoang et al., 2025; Kujawa et al., 2015; Kujawa et al., 2019). The present manuscript used the 500 ms window in line with previous publications using this data across the assessment waves (e.g., Kujawa et al., 2019). In contrast to other event-related potentials, the RewP begins post stimulus presentation and so data remains largely unchanged regardless of 200 ms or 500 ms baseline correction window. To demonstrate this point, we reprocessed the data at each wave and calculated the bivariate correlations between the RewP with 200 ms baseline correction and RewP with 500 ms baseline correction. These values were all $r > .97$, $p < .001$.

Brown, D. R., & Cavanagh, J. F. (2018). Rewarding images do not invoke the reward positivity: They inflate it. *International Journal of Psychophysiology*, 132, 226-235.

Foti, D., Carlson, J. M., Sauder, C. L., & Proudfit, G. H. (2014). Reward dysfunction in major depression: Multimodal neuroimaging evidence for refining the melancholic phenotype. *NeuroImage*, 101, 50-58.

Goldstein, B. L., Kessel, E. M., Kujawa, A., Finsaas, M. C., Davila, J., Hajcak, G., & Klein, D. N. (2020). Stressful life events moderate the effect of neural reward responsiveness in childhood on depressive symptoms in adolescence. *Psychological medicine*, 50(9), 1548-1555.

Hennefield, L., Gilbert, K., Whalen, D., Giorio, C., Camacho, L. E. Q., Kelly, D., ... & Hajcak, G. (2022). The reward positivity shows increased amplitude and decreased latency with increasing age in early childhood. *Developmental science*, 25(3), e13196.

Heydari, S., & Holroyd, C. B. (2016). Reward positivity: Reward prediction error or salience prediction error?. *Psychophysiology*, 53(8), 1185-1192.

Hoang, G. H. L., Hecker, K. G., Maxey, C., Burles, F., Krigolson, O. E., & Kopala-Sibley, D. C. (2025). The reward positivity as a predictor of first-lifetime onsets of depression, anxiety, and suicidal ideation in high-risk adolescents. *Biological Psychiatry: Cognitive Neuroscience and Neuroimaging*, 10(2), 148-157.

Kujawa, A., Burkhouse, K. L., Karich, S. R., Fitzgerald, K. D., Monk, C. S., & Phan, K. L. (2019). Reduced reward responsiveness predicts change in depressive symptoms in anxious children and adolescents following treatment. *Journal of Child and Adolescent Psychopharmacology*, 29(5), 378-385.

Kujawa, A., Proudfit, G. H., Kessel, E. M., Dyson, M., Olino, T., & Klein, D. N. (2015). Neural reactivity to monetary rewards and losses in childhood: Longitudinal and concurrent associations with observed and self-reported positive emotionality. *Biological psychology*, 104, 41-47.

Kujawa, A., Hajcak, G., & Klein, D. N. (2019). Reduced reward responsiveness moderates the effect of maternal depression on depressive symptoms in offspring: Evidence across levels of analysis. *Journal of Child Psychology and Psychiatry*, 60(1), 82-90.

The manuscript indicates that outliers were deleted. Given increasing recommendations against deletion of outliers and in favor of robust statistical approaches, additional justification for this decision is needed (e.g., Karch, 2023). Alternatively, the authors could demonstrate that results are robust to analytic approaches less sensitive to non-normality or extreme values.

RESPONSE: The reviewer is correct that we removed outliers in our original approach. This is in part due to the nature of EEG research, where extreme values may be due to technological failures and connection issues rather than actually reflecting the phenomenon of interest. To address the reviewer's point, we now present alternative models to each of our main models wherein outlier values were winsorized (i.e., taken in to + 3 standard deviations) in the supplement. The results are consistent across approaches.

Generalizability should be discussed, as the sample is predominantly White.

RESPONSE. Thank you for this important point. We have added generalizability limitations to the Discussion section on page 22:

"In addition, parenting approaches and behaviors are in large part culturally constructed (Harkness & Super, 2002). While the importance of some parenting dimensions, such as warmth (Bowlby, 1983; MacDonald, 1992) are observed across cultures from an evolutionary perspective, evidence suggests that family circumstances, cultural context, and parent past experiences influence parenting beliefs and behaviors (Bornstein, 2012; Bornstein & Lansford, 2014; Harkness & Super, 2002; National Research Council and Institute of Medicine, 2000). The current sample was predominately white and the degree to which the observed results generalize is unknown. Future work should directly test whether cultural context and family circumstances moderate associations between parenting dimensions and reward trajectory and how these associations may change over time."

Minor point:

A closing square bracket is missing on page 8, line 146.

RESPONSE: Thank you, the bracket has been added.

Reviewer 2:

In this manuscript, “Early parenting is associated with the development of neural reward responsiveness from childhood through late adolescence”, the authors probe the relationship between parental parenting behavior and stress exposures for the child between 0-3 years on reward responsivity as measured with ERP, at ages 9, 12, 15, and 17 years. This is a powerful study looking at developmental trajectories of reward, and the impact of both parental behaviors and exposure to stressors in early childhood. While prior studies have looked at acute effects of stress exposures on reward processing, and early parenting impact on early reward, this longitudinal study allows for a developmental profile to be generated. This is a novel study, and the findings are quite powerful, the statistics are thorough, but there are a few minor concerns that dampen the overall enthusiasm for this specific study.

RESPONSE: Thank you for your time and expertise in reviewing our manuscript, and for your kind words regarding the importance and novelty of this study as well as the powerful findings observed. We appreciate each of your comments and address each below.

Concerns:

1) The authors draw a lot of attention in the introduction to prior studies showing an impact of parental affect (both positive and negative) on reward responsivity. However, the scales the authors are using don't directly measure parental affect. If the authors point of view in the introduction is to draw attention to the impact a parent may have, whether parenting style or affect, then, a little more direct statement of that would be helpful for the readers. As it is currently written, it's clear that this is a new, and important finding. But, how it integrates with existing literature is less clear given this study hasn't measured affect directly.

RESPONSE: We have revised this section of the Introduction to more clearly specify what has been done in this area as well as what will be the focus of the present study. Specifically, on pages 4-6:

“Parenting is fundamental to early experiences (Humphreys et al., 2024; King et al., 2021), and parental relationships are key processes by which emotions are socialized (Breaux et al., 2022). Importantly, parent–child relationships are built on the summation of many day-to-day interactions (Humphreys et al., 2024). As such, interactions with parents, shaped by the parent’s parenting style, are important contextual features that may affect both baseline levels and trajectories of reward development. In addition to evidence that infants demonstrate increased reward learning with their parent versus a stranger (Tummeltshammer et al., 2019), preschoolers display more dispositional positive affect when parents report higher use of positive emotion socialization approaches in daily interactions (Hill, Kujawa, et al., 2023), and parental encouragement is associated with enhanced reward responses in real-time (Kawamoto & Hiraki, 2019). Further, low positive parenting in early childhood is prospectively associated with reduced reward responsiveness in later childhood (Kujawa, Proudfit, Laptook, et al., 2015) and parent responsiveness moderates the impact of familial risk for depression on decreased reward responses (Morgan et al., 2022). Prior studies suggest parenting, including parent physical presence, parenting style, as well as affect and behaviors are associated with reward responsiveness at single time points; however, examining the

association between parenting behaviors and the *developmental trajectories* of reward responsiveness is important for supporting long-term health and well-being.

Associations between parenting and reward processes are not limited to childhood. Positive parenting practices—that is, demonstration of warmth, respectful communication, and consistency in caregiving (Seay et al., 2014)—are also associated with higher self-esteem and lower depressive symptoms in adolescents (Milevsky et al., 2007). Further, parent–adolescent interactions are associated with adolescent reward responsiveness, such that increased maternal negative affect in these interactions was associated with decreased adolescent reward response to social acceptance (Tan et al., 2014). Notably, parent–adolescent conflict may shape associations between reward responsiveness and adolescent depression (Hill, Dickey, et al., 2023). Across child and adolescent samples, parent emotion socialization practices, parenting styles, and parent behaviors seem to play a key role in positive valence systems development (Breux et al., 2022; Brown et al., 2021; Brown & Fredrickson, 2021). Given the substantial evidence of concurrent associations between parenting styles and parent–child interactions and the significant influence early experiences have on developmental trajectories (Humphreys et al., 2015; King et al., 2021; Lyons-Ruth et al., 2017; Thompson, 2001), the evidence to date suggests early observed parenting behaviors in parent–child interactions may shape baseline and longitudinal trajectories of reward responsiveness.”

2) The authors have utilized statistical models to probe the role of either positive parenting or negative parenting on reward responsivity. However, the authors haven’t justified why they didn’t include both the positive and negative parenting scores for each parent in the statistical models. It’s possible that the behaviors that comprise the negative parenting construct are important parental behaviors in specific situations. It may be possible that different combinations of negative and positive parenting across different tasks produce different reward responsivity. Similarly, a more comprehensive picture may include stress, and positive parenting, and negative parenting into the same statistical model. Ultimately, it is not understood what the justification is for running 3 different models, if they’re potentially connected, and all impacting the child.

RESPONSE: The reviewer’s point is well-taken and is echoed by another reviewer’s comment as well. To address the reviewer’s point, we added an additional supplemental analysis in which positive parenting, negative parenting, and early stress exposure were used as simultaneous predictors in the same model. In this model, no single predictor demonstrated statistically significant main effects or interactions with time. This may be due, in part, to the large bivariate association between positive and negative parenting behaviors in this sample (i.e., $r = -.61$, $p < .001$). See supplement for details. With these results in mind, we have been careful to clarify our approach and the implications for the combined model. As the reviewer suggests, it is likely that overall parenting quality may be what is related to reward response trajectories over time. In addition to the analysis in the supplement, we have added further discussion in the manuscript. On pages 18-19 we state:

“This study builds on previous evidence that early environmental factors relate to reward responsiveness at single points in time by demonstrating that early environmental factors also relate to trajectories across time. Previous work emphasizes the importance of parenting in early development broadly (Humphreys et al., 2024; King et al., 2021) and in positive valence system development specifically (Tummeltshammer et al., 2019),

and has consistently demonstrated associations between positive parenting and reward responsiveness in childhood (Kawamoto & Hiraki, 2019; Kujawa, Proudfit, Laptook, et al., 2015) and adolescence (Clark et al., 2024; Tan et al., 2014). Further, previous studies have demonstrated associations between both positive and negative parenting behaviors and childhood depressive symptoms (Dallaire et al., 2006) and the importance of positive parenting in association with adolescent and young adult mental health (Chen et al., 2019; Smokowski et al., 2015; Tabak & Zawadzka, 2017). Here, both positive parenting and negative parenting behaviors, when examined separately, were prospectively associated with reward response trajectories. Sensitivity analyses, in which positive and negative parenting behaviors were tested in the same model, did not demonstrate unique prospective associations. This is at odds with previous evidence suggesting positive and negative parenting are independent spectra (Dallaire et al., 2006). This may be due to the measurement approach used in the present study wherein parenting behaviors were coded from the same teaching tasks. The bivariate correlation between positive and negative parenting behaviors suggests that while they are not fully overlapping, they may be considered reflective of a broader parenting quality dimension rather than fully independent processes.”

3) This is an incredibly time intensive longitudinal study that was quite successful. There is a gap in time for data though between 3 and 9 years of age, even though the data was collected when kids were 6yrs old. What was/is the justification for not collecting ERP data on those kids at 6 years old, especially since the authors provide references stating that reward response is evident from ages 4/5 through adulthood?

RESPONSE: Thank you for your acknowledgement of the time intensive nature of this study as well as your thoughtful comment regarding assessments at each wave of this longitudinal investigation. Given the length of this study—participants were first enrolled 22 years ago in 2004—the evidence that the RewP could be observed in children as young as 5 years of age was not yet available. The studies investigating the RewP in early childhood were published over a decade later in 2014 and 2016 (e.g., Belden et al., 2016; Lukie et al., 2014). Importantly, while there is some evidence that the RewP may be observed at these early ages, we still do not have evidence regarding the reliability of the RewP at this age. Both Belden (2016) and Lukie (2014) used alternative scoring approaches for the RewP and research evidencing strong internal consistency of the RewP has been conducted only in late childhood, in a sample of children aged 8 – 13 years (Bress et al., 2015).

Methodology questions:

1) Were only moms answers used? Uncertain whether both moms and dads were asked to complete forms, or if it was just moms who completed surveys and performed parenting task

RESPONSE: Thank you for the opportunity to clarify. At the initial assessment, 3-year-old children and at least one biological parent were eligible to participate. While either mothers or fathers were invited to participate, the participating parents overwhelmingly identified as mothers; 23 children completed the behavioral tasks with their father, in contrast to 458 children who completed the behavioral tasks with their mother. For this reason, the manuscript focuses specifically on maternal parenting behaviors and data were excluded if fathers completed the task. The reviewer’s point is well received and we have now clarified this information in the Method on page 10:

“Further, because parents participating in the Teaching Tasks overwhelmingly identified as mothers (>95%), the present analyses included only data obtained from mothers.”

In addition, we have noted the exclusive focus on maternal observed behaviors as a limitation in the Discussion section on pages 21-22:

“Lastly, with regard to the Teaching Tasks, the present study included parenting behaviors exclusively of mothers. While either biological parent was invited to participate in the study, parents who participated in the Teaching Tasks were overwhelmingly mothers (>95%) and more work, especially studies focused on recruitment and retention of father participants, is needed to examine the generalizability of the associations observed between mothers and their children in the present study.”

2) The authors provide different sample sizes that were used for statistical analyses throughout the paper, but it is difficult to follow where the drop outs occurred, and reasons for significantly different n's across tasks. N= 465 with data on stress and EEG at each time point, n= 458 who completed dyadic parenting tasks, 13 removed for ERP data- resulting in n=383, 319, and 284 at different time points. Where are the other several dozens (difference to n=458?). Perhaps a figure documenting sample size distribution across tasks would help here.

RESPONSE: Thank you for this suggestion. We have now created Table 2 to specify where observations were lost across study tasks and waves of assessment. To clarify, preliminary analyses included 465 participants with at least one EEG observation across the study waves, and the other smaller sample sizes (e.g., $n = 383$) reflect the number of participants who completed EEG at that specific wave (e.g., wave 3). Because our analytic approach is robust to missingness, we were able to accommodate missing data points across waves as long as participants completed at least one EEG assessment with usable data to measure the RewP. See Table 2.

Recommendations:

1) While the demographic information is provided in the text, it would be better suited as a Table.

RESPONSE: The demographic information is now available in Table 1.

2) The information in the footnote on the bottom of page 14 really belongs in the main body of the text.

RESPONSE: The footnote has been moved to the main text.

Reviewer 3:

This study addresses an important developmental question regarding how early caregiving environments may shape the development of neural reward systems from childhood to adolescence. The work has several very notable strengths, including a longitudinal dataset and design, multimethod assessment of early environmental factors, and a large sample. The main findings provide suggestive and interesting evidence that early caregiving experiences may be associated with individual differences in trajectories of reward sensitivity from childhood to adolescence. However, several conceptual and methodological issues currently weaken the introduction and discussion of the main

findings. Addressing the concerns outlined above, particularly those related to the interpretation of environmental effects, the measurement of parenting, and conceptual clarity, would substantially improve the manuscript. I hereby provide a detailed report of all my recommendations and concerns below. I therefore recommend overall major revision before the manuscript can be considered for publication.

RESPONSE: Thank you for your time and expertise in reviewing our manuscript. We appreciate your comments on the notable strengths of the manuscript, including the study design, statistical approach, and interesting findings. We also appreciate your points regarding important conceptual considerations and interpretations and clarification of our approach. We address each comment below.

Introduction:

- Page 5 (Lines 92-94): Authors wrote “Importantly, parent–child relationships are built on the summation of many day-to-day interactions (Humphreys et al., 2024)”, emphasizing that parent–child relationships emerge from the accumulation of day-to-day interactions. However, parenting behavior in the present study was assessed at a single time point (age 3) during structured laboratory tasks. While observational assessments are valuable, they may capture only a limited snapshot of caregiving behavior. The authors may wish to clarify this distinction and acknowledge that the measure used here may not fully reflect the cumulative nature of everyday parent–child interactions described in the theoretical framework. In fact, while early parenting is certainly important for children and youth’s development, parenting is dynamic and often changes, especially during a transition as crucial as adolescence (authors could benefit from reading the works by Laurence Steinberg, Jennifer Lansford, Marc Bornstein, etc.). Because parenting was not measured longitudinally, it is difficult to determine whether the observed associations reflect early parenting per se or stable family interaction patterns across development. The authors should discuss this limitation more explicitly and clarify that the observed parenting measure likely represents only a partial snapshot of the child’s caregiving environment.

RESPONSE: We thank the reviewer for this important point, recommended readings, and the opportunity to strengthen interpretation and limitations of the present results. We have revised this section on pages 19-20:

“Importantly, parent–child relationships are built on the summation of many day-to-day interactions (Humphreys et al., 2024). The parenting behaviors we observed at age 3 are only a glimpse into the child’s environmental context for the development of positive affect, reward learning, and reward trajectories across childhood and adolescence. That is, the one-time lab-based observation of parenting does not fully reflect the cumulative nature of everyday parent–child interactions, especially those carried out in naturalistic contexts. Moreover, the importance of daily interactions between parents and children has been observed from infancy to young adulthood (Achterhof et al., 2022; Humphreys & Garon-Bissonnette, 2025; Lippold et al., 2016), with changing needs across developmental transitions (Steinberg & Silk, 2002). In addition, it is likely many additional factors also contribute to the child’s early environmental context, from the macro to micro layers of context (Tudge & Rosa, 2020; Witherspoon et al., 2023). These contextual factors, including parenting health, cultural norms, and resource availability to name a few, may impact the child through parent cognitions and emotions related to the child, parenting beliefs and behaviors, physical proximity to the child (Bornstein & Lansford, 2014; Evans & Kim, 2013; Humphreys et al., 2024; Humphreys & Zeanah, 2015; King et

al., 2021) and through complex interactions amongst child, family, and community levels wherein the child always plays an active role in their development (Humphreys & Garon-Bissonnette, 2025; Jack, 2000). It will be important to investigate how single observations of parenting at age 3 years, as in the present study, in contrast to more ecologically valid observations—for example, home-based daily assessments at multiple spurts of time—relate to reward response trajectories similarly or differently.”

We have also revised the Limitations of the present study and conclude the section in reflecting on these themes. Specifically, we state on page 22:

“Future work should directly test whether cultural context and family circumstances moderate associations between parenting dimensions and reward trajectory and how these associations may change over time.”

- Page 5-6: The authors conceptualized reward, stressing mainly its neurobiological characteristics as RewP component. Although this view is widely documented, readers outside this field of study could benefit from a deeper explanation of how parenting (a classic family construct) can also influence temperamental and biologically based constructs, such as reward sensitivity. Throughout the manuscript, I found that the conceptual bridge between these neural processes and parenting behaviors remained underdeveloped in terms of reasoning. I thereby suggest emphasizing the link between the neurobiological system and the family interaction processes by expanding the biological embedding theory and the social reward learning approach. In other words, the manuscript would benefit from clearer justification of the theoretical framework linking early parenting to reward system development. While the discussion references positive valence systems and developmental models of adolescence, the mechanisms linking early caregiving to later neural reward sensitivity could be elaborated further.

RESPONSE: We appreciate these important theoretical points that strengthen the discussion of the present study. The introduction now discusses how experiences could, in theory, impact neurobiology while also maintaining discussion of the current observational study. For example, on page 4 we state:

“Understanding variation in this typical developmental trajectory is essential to identifying potential pathways to increased vulnerability to psychopathology and other adverse outcomes. Contributors to reduced reward responsiveness likely span genetics, temperament, and early experiences (for a review, see Kujawa et al., 2020). Importantly, these contributors interact (Calkins et al., 2013) such that early experiences can be biologically embedded and impact subsequent functioning and outcomes (Gonzalez et al., 2018; Rutter, 2016). Parenting styles and stress exposure are types of early experiences that may be the most associated with individual differences in developmental trajectories of reward responsiveness. Parenting is fundamental to early experiences (Humphreys et al., 2024; King et al., 2021), and parental relationships are key processes by which emotions are socialized (Breux et al., 2022).”

Method:

- Page 9-10: The sample is overwhelmingly White and geographically restricted, which limits generalizability. This limitation should be acknowledged more explicitly. In addition, further demographic context in the participant section would strengthen the manuscript. Considering their variables, parenting practices, emotional socialization, and family interaction patterns are known to vary across *cultural* and socioeconomic

contexts. The authors may wish to more explicitly acknowledge this limitation and discuss the extent to which the findings may generalize beyond relatively homogeneous White U.S. community samples.

RESPONSE: Thank you for these important points. We have updated the Method and Discussion sections:

Method, on page 8:

“Participants were from the Stony Brook Temperament Study, an ongoing prospective community cohort study focused on childhood temperament and risk for internalizing disorders in which parents and children completed assessments approximately every 3 years starting at child age 3 years through 18 years (Klein & Finsaas, 2017). Initial eligibility criteria for study participation included the child being 3 years of age, without a diagnosed developmental disability or serious medical condition, with at least one English-speaking biological parent, and living within twenty miles of Stony Brook, New York. The Stony Brook University Institutional Review Board approved this study.

At the first study wave, initiated in 2004, 559 families with a 3-year-old child were recruited. Most families were white, middle class, with approximately half of parents having obtained a four-year college degree or higher (see Olino et al., 2010 for more details). Parents gave written informed consent at the first wave and each subsequent wave of data collection until the child was 18, at which time the child then gave written informed consent. In addition, children provided assent at the age 12 and 15 waves. Of the families recruited to participate in the study, 541 families completed parental structured clinical interviews to assess child stress exposure since birth, and 525 families completed dyadic behavioral tasks coded to assess parenting behaviors. Excluding participants with missing or unusable EEG data at every wave led to a final sample of 465 children with data on stress exposure since birth and 458 who completed dyadic parenting tasks with their mothers.”

Discussion, on page 22:

“In addition, parenting approaches and behaviors are in large part culturally constructed (Harkness & Super, 2002). While the importance of some parenting dimensions, such as warmth (Bowlby, 1983; MacDonald, 1992) are observed across cultures from an evolutionary perspective, evidence suggests that family circumstances, cultural context, and parent past experiences influence parenting beliefs and behaviors (Bornstein, 2012; Bornstein & Lansford, 2014; Harkness & Super, 2002; National Research Council and Institute of Medicine, 2000). The current sample was predominately white and the degree to which the observed results generalize is unknown. Future work should directly test whether cultural context and family circumstances moderate associations between parenting dimensions and reward trajectory and how these associations may change over time.”

- Page 10 – the parenting assessment: Parenting behaviors were assessed through brief laboratory-based observational coding of parent–child interaction tasks. Although observational approaches have important strengths in reducing shared method variance, such assessments capture only a limited segment of interaction within a structured laboratory context, and parenting practices typically unfold across a wide range of everyday situations in the home environment. Thus, it may be helpful to clarify more deeply the rationale of this choice, and to discuss the extent to which laboratory observations can be considered representative of naturalistic parenting behaviors, and

whether complementary perspectives (e.g., using the multi-informant measures of parenting provided by the other parent), might provide additional insight into their results. I believe that this aspect represents one of the main limitations of this study, so the authors should clarify in the limitations section that future works integrating neural measures with more naturalistic assessments of parenting (e.g., home observations, ecological momentary assessments, or multi-informant reports) could help clarify whether the associations observed here generalize to everyday caregiving environments.

RESPONSE: The reviewer's point is well-taken. In addition to the revisions made to related comments in other parts of the manuscript, we have directly spoken to this point in the Limitations section. On page 21 we state:

"Future work would benefit from testing alternative approaches to measuring parenting constructs and stress exposure. A strength of the present study was that parenting was measured via direct observation of parent-child interactions rather than parent reports; however, this measure is nonetheless limited to the task and context at assessment. Emerging evidence suggests that directing parents to teach their young children increases parent intrusiveness (part of the negative parenting composite in the present study; King et al., 2023). This may have impacted observed parenting behaviors in the present study. In addition, alternative approaches to measuring parenting behaviors—notably ecological momentary assessments, home visits, multi-informant reports collected across development—may better capture naturalistic parenting behaviors and how they unfold across development."

- Page 13 – Line 289: On this line, the authors specified for the first time that in their study, they considered maternal parenting practices, rather than parenting practices in general. Throughout the introduction, the authors refer broadly to "parenting" as the environmental construct of interest. However, the observational measure used in the study appears to capture maternal parenting behaviors specifically, as interactions were coded within mother-child dyads. It may therefore be helpful for the authors to clarify this distinction earlier in the manuscript and to discuss whether the findings should be interpreted specifically in terms of maternal caregiving rather than parenting more generally.

RESPONSE: Thank you for this important point. We have now specified that the present study focuses on maternal parenting behaviors earlier in the manuscript, have specified why this is the case for the current investigation, and note this as a limitation in the Discussion. Specifically, we state—

Introduction, page 7:

"Parenting behaviors were observed during mother-child interactions in the lab and stress exposure was measured using structured clinical interviews at the first study wave at child age 3 years. We focused on parenting behaviors observed at child age 3 as observational methods are considered gold standard (Zahidi et al., 2019) and conceptual frameworks propose that early experiences may shape trajectories over time (King et al., 2021; Opendak et al., 2025)."

Method: Parenting Behaviors, page 10:

"Further, because parents participating in the Teaching Tasks overwhelmingly identified as mothers (>95%), the present analyses included only data obtained from mothers."

Method: Data Analytic Plan and Preliminary Analyses, page 13:

“The predictors of interest were maternal parenting behaviors assessed at age 3 and cumulative stress exposure from birth to age 3.”

Discussion: Limitations, on pages 21-22:

“Lastly, with regard to the Teaching Tasks, the present study included parenting behaviors exclusively of mothers. While either biological parent was invited to participate in the study, parents who participated in the Teaching Tasks were overwhelmingly mothers (>95%) and more work, especially studies focused on recruitment and retention of father participants, is needed to examine the generalizability of the associations observed between mothers and their children in the present study.”

Results:

- The primary outcome variable was the RewP to win feedback rather than the traditional win–loss difference score. The authors justify this decision on reliability grounds, which is reasonable, but the choice also has interpretive implications because the RewP difference score is typically conceptualized as the canonical index of reward responsiveness. The manuscript would benefit from a clearer explanation of how the selected operationalization relates to standard approaches in the ERP literature and whether the findings replicate when alternative scoring approaches are used.

RESPONSE: The reviewer’s point is well-taken. The use of difference scores have been called into question in ERP and research more broadly given low reliability and the inability to distinguish which variable is driving results. As summarized by Clayson (2024), each ERP study’s operationalization of the ERP of interest make assumptions about the neural activity of interest and should impact the interpretations of findings. For example, previous work has demonstrated that the error-related negativity (ERN) is impacted by recording, analyzing, and scoring decisions (for a summary, see Clayson, 2024). This work has not yet been conducted with regard to the RewP; however, the present manuscript takes important steps forward in this regard. Here, our approach of using the RewP to win for our main analyses suggest specific interest in this process (e.g., reward responsiveness) rather than the isolated difference between win and loss feedback processing (e.g., win – loss scores). To complement this approach, and facilitate conversation within the context of literature to date, we offer a series of sensitivity analyses in the supplement. In the supplement, we report models (1) with the RewP to win covarying for the RewP to loss as a time-varying covariate, (2) with the RewP to win – RewP to loss subtraction-based difference scores, and (3) the RewP to loss, rather than the RewP to win, as the outcome.

The results of these analyses have important implications for the interpretation of our findings. First, the quadratic effect of time is robust across RewP scoring approaches when the focus is RewP to win (e.g., RewP to win scores, RewP to win covarying for RewP to loss, and RewP win – loss models) and when focusing on the RewP to loss. However, the interactions of interest (e.g., between time and parenting) only hold for the RewP to win and RewP to win covarying for RewP to loss approaches. The interactions are not statistically significant when using the subtraction-based difference score or when investigating the RewP to loss. Notably, as in our initial submission, only the interactions between negative parenting and time remained statistically significant when examining the RewP to win covarying for the RewP to loss as a time-varying covariate. Together, these results suggest that how the RewP is operationalized is important to the observed results. These scoring decisions should be presented transparently and the

implications of these scoring decisions should be discussed. We hope the present manuscript offers one pathway forward—to present the results of analyses according to the strongest psychometric properties as well as presenting sensitivity analyses that offer transparency and fuller discourse with the literature to date.

Clayson, P. E. (2024). The psychometric upgrade psychophysiology needs. *Psychophysiology*, 61(3), e14522.

- Positive and negative parenting measures were derived from overlapping observational ratings and appear to be strongly correlated. This raises conceptual and statistical concerns regarding the degree to which the two constructs capture distinct dimensions of parenting. The authors briefly note this issue but do not fully address its implications for interpreting the results. It would be helpful to clarify the correlation between these composites and discuss whether the findings might reflect a broader dimension of parenting quality rather than independent positive and negative processes.

RESPONSE: Thank you for this important point, which is echoed by another reviewer's comment as well. Positive parenting and negative parenting measures are indeed correlated in the present sample ($r = -.60$). This strong correlation suggests a high degree of overlap, with some degree of separate identity (e.g., 36% shared variance, leaving 64% unshared). As such, positive and negative parenting behaviors are not completely overlapping in the present study, and this is consistent with prior literature suggesting these are two separate dimensions such that parents could be high in both, low in both, or some combination of high and low in each. Nonetheless, the high degree of overlap precludes a statistical model with both included as predictors due to collinearity. To address the reviewer's point, we added an additional supplemental analysis in which positive parenting, negative parenting, and early stress exposure were used as simultaneous predictors in the same model. In this model, no single predictor demonstrated statistically significant main effects or interactions with time. See supplement for details. With these results in mind, we have been careful to clarify our approach and the implications for the combined model. As the reviewer suggests, it is likely that an overall parenting quality may be what is related to reward response trajectories over time. In addition to the analysis in the supplement, we have added further discussion in the manuscript. On pages 18-19 we state:

"This study builds on previous evidence that early environmental factors relate to reward responsiveness at single points in time by demonstrating that early environmental factors also relate to trajectories across time. Previous work emphasizes the importance of parenting in early development broadly (Humphreys et al., 2024; King et al., 2021) and in positive valence system development specifically (Tummeltshammer et al., 2019), and has consistently demonstrated associations between positive parenting and reward responsiveness in childhood (Kawamoto & Hiraki, 2019; Kujawa, Proudfit, Laptook, et al., 2015) and adolescence (Clark et al., 2024; Tan et al., 2014). Further, previous studies have demonstrated associations between both positive and negative parenting behaviors and childhood depressive symptoms (Dallaire et al., 2006) and the importance of positive parenting in association with adolescent and young adult mental health (Chen et al., 2019; Smokowski et al., 2015; Tabak & Zawadzka, 2017). Here, both positive parenting and negative parenting behaviors, when examined separately, were prospectively associated with reward response trajectories. Sensitivity analyses, in which positive and negative parenting behaviors were tested in the same model, did not demonstrate unique prospective associations. This is at odds with previous evidence

suggesting positive and negative parenting are independent spectra (Dallaire et al., 2006). This may be due to the measurement approach used in the present study wherein parenting behaviors were coded from the same teaching tasks. The bivariate correlation between positive and negative parenting behaviors suggests that while they are not fully overlapping, they may be considered reflective of a broader parenting quality dimension rather than fully independent processes.”

- In further analyses, parenting explains variation in slope through an interaction with time, as the authors seemed to affirm that the trajectory of RewP over time depends on parenting levels. However, parenting was assessed at a single early time point (age 3) and treated as a time-invariant predictor of reward responsiveness trajectories from ages 9 to 18. While this approach is common in longitudinal models examining early environmental influences and is statistically appropriate, this approach raised a conceptual concern for me. Parenting, as I said in a previous comment, is conceptually dynamic and may change substantially across development. Consequently, the current analyses capture associations with early caregiving behavior rather than parenting processes across childhood and adolescence. Clarifying this distinction and acknowledging the limitation would strengthen the interpretation of the findings.

RESPONSE: We agree with the reviewer that conceptually, parenting is a dynamic process that is influenced by many parent, child, and contextual factors over time. We appreciate that the reviewer noted that our statistical approach is appropriate, and addressed their concerns by expanding our discussion of the limitations and conceptual implications of observing parenting at one timepoint in the Discussion section. Specifically, on pages 19-20 we state:

“Importantly, parent–child relationships are built on the summation of many day-to-day interactions (Humphreys et al., 2024). The parenting behaviors we observed at age 3 are only a glimpse into the child’s environmental context for the development of positive affect, reward learning, and reward trajectories across childhood and adolescence. That is, the one-time lab-based observation of parenting does not fully reflect the cumulative nature of everyday parent–child interactions, especially those carried out in naturalistic contexts. Moreover, the importance of daily interactions between parents and children has been observed from infancy to young adulthood (Achterhof et al., 2022; Humphreys & Garon-Bissonnette, 2025; Lippold et al., 2016), with changing needs across developmental transitions (Steinberg & Silk, 2002). In addition, it is likely many additional factors also contribute to the child’s early environmental context, from the macro to micro layers of context (Tudge & Rosa, 2020; Witherspoon et al., 2023). These contextual factors, including parenting health, cultural norms, and resource availability to name a few, may impact the child through parent cognitions and emotions related to the child, parenting beliefs and behaviors, physical proximity to the child (Bornstein & Lansford, 2014; Evans & Kim, 2013; Humphreys et al., 2024; Humphreys & Zeanah, 2015; King et al., 2021) and through complex interactions amongst child, family, and community levels wherein the child always plays an active role in their development (Humphreys & Garon-Bissonnette, 2025; Jack, 2000). It will be important to investigate how single observations of parenting at age 3 years, as in the present study, in contrast to more ecologically valid observations—for example, home-based daily assessments at multiple spurts of time—relate to reward response trajectories similarly or differently.”

- Lastly, in the main text, the authors focused primarily on positive parenting, whereas in the supplementary material, we observe that this effect is not robust. I suggest including

a couple of sentences on this topic in the discussion section. The take-home message for me from these findings is that negative early parenting attenuated the development of reward responsiveness, not that positive parenting is associated with stronger reward sensitivity development.

RESPONSE: Given the additional analyses conducted in response to reviewer comments, we now have additional evidence as to how robust the observed associations between positive parenting and the RewP to win may be. First, in the primary models presented in text, positive parenting shared prospective associations with the RewP to win intercept as well as interactions with linear and quadratic trends of time. Each of associations held in sensitivity analyses that used winsorized rather than removed outliers. In addition, the statistically significant interactions between positive parenting and the linear and quadratic trends of time also held with and without the covariates of child sex and maternal depression diagnoses. These interactions were in the same pattern but were not statistically significant (see supplement for details). Conceptually, the reviewer's take-home message may be accurate, it is likely that overall parenting quality may be what is related to reward response trajectories over time. In addition to the analyses in the supplement, we have added further discussion in the manuscript. On pages 18-19 we state:

“This study builds on previous evidence that early environmental factors relate to reward responsiveness at single points in time by demonstrating that early environmental factors also relate to trajectories across time. Previous work emphasizes the importance of parenting in early development broadly (Humphreys et al., 2024; King et al., 2021) and in positive valence system development specifically (Tummeltshammer et al., 2019), and has consistently demonstrated associations between positive parenting and reward responsiveness in childhood (Kawamoto & Hiraki, 2019; Kujawa, Proudfit, Laptook, et al., 2015) and adolescence (Clark et al., 2024; Tan et al., 2014). Further, previous studies have demonstrated associations between both positive and negative parenting behaviors and childhood depressive symptoms (Dallaire et al., 2006) and the importance of positive parenting in association with adolescent and young adult mental health (Chen et al., 2019; Smokowski et al., 2015; Tabak & Zawadzka, 2017). Here, both positive parenting and negative parenting behaviors, when examined separately, were prospectively associated with reward response trajectories. Sensitivity analyses, in which positive and negative parenting behaviors were tested in the same model, did not demonstrate unique prospective associations. This is at odds with previous evidence suggesting positive and negative parenting are independent spectra (Dallaire et al., 2006). This may be due to the measurement approach used in the present study wherein parenting behaviors were coded from the same teaching tasks. The bivariate correlation between positive and negative parenting behaviors suggests that while they are not fully overlapping, they may be considered reflective of a broader parenting quality dimension rather than fully independent processes.”

Discussion:

- Page 18, Lines 402-404: authors stated that “Lower levels of positive parenting and higher levels of negative parenting, observed via parent–child interactions when children were 3 years old, were each associated with a weaker increase in the RewP over time.” In my opinion, considering that results for positive and negative parenting were significant only when such behaviors were considered in interaction with time, could be too strong. I suggest modifying this sentence by providing a more cautious interpretation of their findings. This suggestion could be applied to the entire discussion section.

RESPONSE: The Discussion, and this sentence specifically, has been revised to reflect the updated analyses as well as temper interpretation.

- Page 19: The absence of associations between early stress exposure and reward responsiveness is interpreted cautiously, but the limited range and severity of stressors in the sample may partly explain the null findings. The sample appears to be relatively low-risk, with modest levels of reported stress exposure. As a result, the analyses may have lacked sufficient variability to detect effects. This issue should be emphasized more clearly in the discussion.

RESPONSE: We have revised the Discussion to further discuss this important point. On pages 20-21 we state:

“We did not find statistically significant associations between early stress exposure (measured from birth to three years and from birth to nine years of age) and either the overall magnitude of reward responses or the trajectory of reward responsiveness across 9 to 18 years. These observations may be due to the observed associations with parenting, as previous work suggests that parenting buffers the effects of stress exposure (Brown et al., 2020). The findings may also be specific to the present sample, which reported a relatively limited range and severity of early stress exposure. As such, the relatively low-risk nature of the present sample could lead to reduced observed associations in contrast to samples selected for greater variability or known stress exposure.”

Minor Comments

- Table 1: It may be helpful to clarify the developmental timing of the predictors (e.g., positive and negative parenting assessed at age 3, and stress exposure measured from birth to age 3) to facilitate interpretation of the descriptive statistics, considering that the authors specified the age at which each variable was assessed for all other variables.

RESPONSE: Developmental timing has been added to the bivariate association table, now Table 3.

- Figures depicting developmental trajectories would benefit from clearer labeling and interpretation in the main text.

RESPONSE: We have updated these figures as part of the revisions and now more fully incorporate them in the main text.

Terminology use:

- The manuscript repeatedly refers to “mechanistic pathways” linking early parenting to reward responsiveness. However, the current design does not directly test mechanistic processes or mediating variables. The authors may wish to consider using more cautious terminology (e.g., developmental associations or potential pathways) unless specific mechanisms are empirically tested.

RESPONSE: Thank you, we appreciate this important point. We revised the abstract, introduction, and discussion to reflect the observational nature of this longitudinal study. Specifically, we state:

Abstract:

“Understanding associations between early experiences and variation in the typical developmental trajectory of reward responsiveness is essential to identifying potential pathways to health.”

Introduction on page 4:

“Understanding variation in this typical developmental trajectory is essential to identifying potential pathways to increased vulnerability to psychopathology and other adverse outcomes.”

Discussion on page 20:

“These results suggest there are prospective associations between early parenting observed in toddlerhood and reward response trajectories across critical periods of adolescent neurodevelopment.”

- The manuscript frequently frames the findings as evidence that early parenting shapes or influences the development of neural reward responsiveness. However, the observational design limits causal inference. Parenting behavior was assessed at a single early time point, and several potential confounding factors—such as parental psychopathology, genetic influences, child temperament, and broader socioeconomic context—could plausibly account for both parenting practices and reward system development. The discussion should therefore adopt more cautious language and explicitly acknowledge these alternative explanations.

RESPONSE: Thank you for these important points. We have revised the language used to reflect our observational design. We have also added text reflecting the likely additional contributing factors to parenting practices and reward system development. See exemplar revised text on page 20.

- On page 10, the authors named the section about the early stressors as “Stress Exposure”, but for improving the clarity of the paper, I would suggest modifying this title as follows: “Early stress exposure”.

RESPONSE: The section title has been changed.

Editor

Your manuscript titled "Early parenting is associated with the development of neural reward responsiveness from childhood through adolescence" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in *Communications Psychology*.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers and a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

RESPONSE: Thank you for your editorial review and the opportunity to publish our work in *Communications Psychology*, pending our revisions in response to the editorial requests and reviewer comments below. We are grateful for your and the reviewers' time and expertise and pleased that you saw the merit to this work. Please see specific responses to your and the reviewers' comments below.

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table". Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

RESPONSE: We have addressed each of the specific editorial comments and requests. Please see the attached "Editorial Requests Table" attached for our responses in the right-hand column.

Reviewer 1:

Thank you for the opportunity to review the revised manuscript. The authors have made considerable effort to address the comments raised in the previous round of review, and I am mostly satisfied with the revisions. However, the revision and responses still have one major issue:

The inclusion of additional sensitivity analyses is appreciated. However, the results seem to suggest that the interaction effects are considerably less robust after covarying for RewP to loss or using the difference score, and the pattern of results predicting RewP to loss, despite being nonsignificant, appear similar to the results predicting RewP to win. These follow-up analyses suggest that the interaction effects are less specific to RewP to win than stated in the manuscript. The authors should soften the language in the abstract and discussion to better reflect the results found across analyses.

RESPONSE: Thank you for your time and expertise in reviewing our manuscript. To address your remaining comment, we have carefully reviewed the abstract and Discussion section. We made revisions to the Discussion section balancing both the reviewer concern and the editorial comment (see Editorial Request Table) which refers to R1's note and specifies to "continue to refrain from interpreting null results." On pages 20-21, we state:

"It is important to note that results varied depending on ERP scoring approach. Although it is common to use subtraction-based ERP difference scores to isolate the process of

interest across experimental conditions⁹⁴, these are prone to low reliability and may not be the most appropriate for examining change across time. Here, we used the RewP to win as the primary outcome changing across time, and we conducted a series of sensitivity analyses using other scoring approaches. The RewP to loss demonstrated a similar growth trajectory over time to the RewP to win, though this was not statistically significant when including the RewP to win in the model. When examining the RewP to win while covarying the RewP to loss as a time-varying covariate, we observed a similar growth trajectory over time as in the primary models and higher negative parenting scores predicted weaker linear and quadratic trends over time. However, positive parenting interactions with linear and quadratic time were no longer statistically significant. When we examined subtraction-based difference scores (e.g., RewP to win – RewP to loss) as the outcome, the RewP difference score demonstrated quadratic growth over time, but neither positive parenting nor negative parenting interacted with time effects in predicting the RewP difference. As such, the quadratic effect of time appeared robust across RewP scoring approaches (e.g., RewP to win scores, RewP to win covarying for RewP to loss, and RewP win – loss models). However, many of the interactions of interest only held when modeling the RewP to win or RewP to win covarying for RewP to loss. The reduced reliability of subtraction-based difference scores likely contributed to changes in results, and additional research is needed on best practices for longitudinal analyses of ERP components.”

Reviewer 2:

The authors provide an excellent response to reviewers, and made substantial edits to the manuscript addressing concerns that were provided. Unfortunately, neither of the 2 new tables (Tables 1 and 2) that were created are visible to me, and I had specifically requested those tables. Please make those visible within the review file.

RESPONSE: Thank you for your time and expertise in reviewing our manuscript, and for your positive feedback regarding the response to reviewers and revisions in the manuscript. We apologize that the 2 new tables were not initially available to you.

There are a few minor issues that should be addressed:

1. The authors state that participants received \$400 per wave, which seems an astronomical compensation. If correct, please explain why payments were so high-otherwise, please revise.

RESPONSE: We have revised this section to further justify the participant compensation rate. These procedures were approved by the IRB and were reflective of participants' time in the study. Specifically, compensation was provided at each assessment wave. Compensation amounts varied across waves and was prorated to reflect which assessments were completed depending on the number of assessments completed and time demands of each, with total amounts adjusted across time to account for inflation and promote retention. Across waves, the maximum total compensation amount was \$400 USD per wave including separate diagnostic and life stress assessments with the child and one or both parents; diagnostic interviews with each parent about the parents' history of psychopathology; extensive questionnaire batteries for both parents and the child; in the early waves separate laboratory temperament and parent-child interaction observational assessments; an EEG assessment of the child, and in one wave, a parent; home-based saliva collections for hormonal assays (on up to nine

occasions); and in several waves intensive 2-week (5x per day) ecological momentary assessments. Participants were generally reimbursed at the rate of \$25-30/hour. On pages 8-9, we state:

“Compensation was provided at each assessment wave. Compensation amounts varied across waves and was prorated to reflect which assessments were completed, with total amounts adjusted across time to account for inflation and promote retention. The maximum total compensation amount was \$400 USD per wave if all assessments were completed including separate diagnostic and life stress assessments with the child and one or both parents; diagnostic interviews with each parent about the parents' history of psychopathology; extensive questionnaire batteries for both parents and the child; in the early waves separate laboratory temperament and parent-child interaction observational assessments; an EEG assessment of the child, and in one wave, a parent; home-based saliva collections for hormonal assays (on up to nine occasions); and in several waves intensive 2-week (5x per day) ecological momentary assessments. Participants were generally reimbursed at the rate of \$25-30 USD/hour.”

2. In the new paragraph labeled "Maternal Lifetime Depression Diagnosis", the description of who administered the SCID should be similar in level of detail to the description for the PAPA. Currently, PAPA is succinct and is written, "trained interviewers", while the SCID says, master-level clinicians and upper-level doctoral students".

RESPONSE: We have made this revision.

3. Additionally, there is a sentence in that same paragraph that "Interrater reliability was excellent". It's unclear what was being coded and scored for interrater reliability. Additionally, if there are reliability scores, those should be provided.

RESPONSE: We have revised this section to state:

“Lifetime depression history in mothers was assessed using the Structured Clinical Interview for DSM-IV-Non-Patient Version (SCID⁹¹) at child age 3 and age 9 waves. The SCID was administered to mothers by trained interviewers. Scores at both waves were combined to reflect lifetime diagnoses. Interrater reliability, scored as Kappas for diagnoses of depressive disorders, was excellent (age 3 Kappa = .93, age 9 Kappa = .91⁵⁵).”

4. In the next to last paragraph of "EEG Data Collection and Processing", remove the sentence "See Table 2 for sample sizes across variables and waves", and insert (Table 2) at the end of the sentence that immediately precedes it, "after outlier removal.....variable and assessment waves (Table 2).

RESPONSE: We have made this revision.

5. The fifth paragraph of the discussion section, consider the following revision, "...for example, home based daily assessments at multiple timepoints relate to reward response trajectories."

RESPONSE: We have made this revision.