

Stress Timing, Trauma Exposure, and Family Resilience Differentially Affect Internalizing and Externalizing Symptoms at 3, 5, and 7 years of Age

Corresponding Author: Dr Viviane Valdes

Version 0:

Decision Letter:

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Dear Dr Valdes,

Thank you for your patience during the peer-review process. Your manuscript titled "The Impact of Stressful Events, Traumatic Experiences, and Family Resilience on Psychopathology Symptoms in Children at 3, 5, and 7 Years of Age" 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 revised manuscript thoroughly acknowledges the limitations of the study design including the correlational nature of the study and the limitations on generalizability (i.e. the cohort characteristics) throughout the manuscript, including the abstract and ideally the title. Please provide descriptive, correlational, and psychometric statistics as requested by the reviewers. Please ensure you follow our statistical guidelines when reporting statistics (<https://www.nature.com/commpsychol/submit/submission-guidelines#statistical-guidelines>).

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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 would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

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:

Reviewer #1 stress, mental health, longitudinal designs

Reviewer #2 stress, mental health, longitudinal designs

Reviewer #3 stress, mental health, longitudinal designs

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

The manuscript under review presents results from a cohort of families recruited from a registry of local births and followed longitudinally from their child's infancy to 7 years of age. The authors analyze associations between parent-reported stressful life events and traumatic events and child internalizing and externalizing symptoms at ages 3, 5, and 7. They also consider the role of "family resilience" or "hardiness" assessed via a parent-report scale capturing the family's sense of internal strengths, efforts to experience new things, and locus of control. The authors conclude that greater exposure to stressful experiences was associated with greater psychopathology symptoms at age 3 and 5 years. They also report finding evidence for a variety of timing effects and "exposure/resilience specificity effects" – e.g., that that interpersonal (but not non-interpersonal) trauma was associated with greater internalizing symptoms, and that family locus of control was protective for externalizing symptoms at age 7 years but not the other two measures of family resilience.

I have mixed feelings about this paper. On the one hand, the longitudinal design and repeated, developmentally-appropriate assessments of stressful life events and mental health symptoms are both strengths. In addition, the questions tackled by the paper are important ones: identifying both sensitive periods and "specificity effects" could theoretically help interventionists fine-tune efforts targeting families experiencing high levels of stress to generate maximal benefits to children. On the other hand, the study design, measures, and methods are such that the authors' results could be consistent with several different interpretations, which makes it challenging to draw definitive conclusions regarding the research questions posed. The manuscript would also benefit from more precise and cautious language when describing the measures and findings, and a more expansive, thorough discussion of potential limitations and alternative explanations. Below, I have outlined specific issues and suggested edits to help improve the manuscript.

(1) The authors cite work appropriately acknowledging "resilience" as a multisystem phenomenon but capture it using only a single parent-report measure of family functioning. This is a significant limitation, and discussion regarding the multiple causal pathways that might lead to associations between this measure and other study constructs like child psychopathology is underdeveloped. For example, in labeling it a measure of "resilience" the assumption seems to be that families scoring high on this measure may be less impacted by stressors. But it is also possible that fewer stressors could lead directly to higher scores on the measure, or that child psychopathology could lead directly to lower scores. There is no mention (that I could find) of previous literature suggesting any of these possibilities is more or less likely than any of the others. This ambiguity makes it difficult to know how to best interpret their findings, particularly ones suggesting only certain forms of family "resilience" (i.e., "control") are associated with certain outcomes (i.e., externalizing symptoms).

(2) The authors note in the discussion that "the effect sizes for the association between stressful events and psychopathology symptoms were strongest in the prenatal period and steadily declined with age through 3 years." They interpret this as evidence supporting a potential "sensitive period" effect, after defining a sensitive period as a "period of greatest brain plasticity". While it is theoretically possible that the infant brain could be more plastic prenatally vs. postnatally, it is important to note that this association could also arise for reasons that have nothing to do with brain plasticity, including differences in the types or magnitude of stressors experienced prenatally vs. postnatally, variation in how parental stress is transferred to offspring (e.g., cortisol crossing the placenta to effect the child prenatally), or developments in child cognitive capacity and coping ability. It would help contextualize the authors' findings if they acknowledged these different possibilities and made it clearer that the current study has no effective way of distinguishing between them.

(3) The finding that stressful life events are associated internalizing symptoms at age 3 for females but not males (and that effects in males seem to, if anything, go in the opposite direction) is a curious one and deserves further exploration. Do the mean levels or variability in internalizing symptoms at this age differ significantly by sex? What about means/SDs for stressful life events? Can the authors tie this finding to any other previous publications or edit their discussion to speculate on a few possible explanations?

(4) The authors' decision to operationalize stressful/traumatic events exposure as a count of the number of different types of events children's parents experienced during the reporting period is not without precedent but requires justification. This is particularly true given their stated aim of exploring potential timing effects, given the strong possibility that any timing effects might reflect differences in the types of stressful/traumatic experiences occurring in each reporting period (rather than developmental changes in plasticity). There is also some evidence indicating children's age of peak "sensitivity" to different types of exposure may vary (e.g., Schaefer et al., 2022), which would argue against their "sum of endorsed events" approach.

Smaller comments:

(1) Ln. 128-129: Please explain what is meant by "may function on one spectrum"

(2) Ln. 226-134: Please clarify in the description of the "traumatic events" measure whether it relies on parent or child report

References:

Schaefer, J. D., Cheng, T. W., & Dunn, E. C. (2022). Sensitive periods in development and risk for psychiatric disorders and related endpoints: A systematic review of child maltreatment findings. *The Lancet Psychiatry*, 9(12), 978–991. [https://doi.org/10.1016/S2215-0366\(22\)00362-5](https://doi.org/10.1016/S2215-0366(22)00362-5)

Reviewer #2 (Remarks to the Author):

This longitudinal study examined reports of 456 parents taken at their child's infancy, and 2, 3, 5, and 7 years later. More reports of exposure to stressful experiences during the prenatal period through age 5 was linked to more internalizing and externalizing symptoms at age 5, but at age 3 only to externalizing symptoms and to internalizing symptoms for girls only. Exposure to interpersonal trauma was linked to more subsequent internalizing symptoms, and interpersonal and non-interpersonal traumatic experiences were linked to more externalizing symptoms. Family resilience features, such as more family commitment, embrace of challenges, and sense of control appeared to protect against internalizing symptoms; only sense of control appeared to protect against age 7 externalizing symptoms. Findings add to a large body of work including in older youth showing that stressful life events are linked to more subsequent psychopathology.

While the findings are not particularly novel, the work adds to a growing literature and points to possible protective factors that could be examined in prevention studies. The literature review is relatively comprehensive, stats are generally well executed, and the work is largely reproducible. A few suggestions may improve the reproducibility and communication of findings.

Abstract:

- Please add effect size indicators and p-thresholds for effects described.

Intro:

- Is there a DALY estimate that is newer than 2019?
- Is this statement in fact accurate, "most existing research has focused on middle childhood, adolescence, and adulthood, with limited longitudinal research examining outcomes in early childhood"?

Methods

- Participants: A bit more context about the source of participants would be helpful; for example, specify the general location of "local births" and the years when participants were recruited.
- Participants: please clarify how many participants had internalizing an externalizing data at all 3 time points?
- Measures: were the forms self-completed or completed via a research associate?
- Measures: please provide some psychometrics for the RLEQ and TESI scales used.

Results

- It is surprising that parent education and annual household income were not linked with outcomes. Was resilience examined in relation to parent education and annual household income as well?
- Given the large number of analyses run, a p-threshold for considering significance should be established.
- The resilience findings are interesting; p-values should be provided in this paragraph, as in the prior paragraphs.

Discussion

- The sample is quite white, educated, and high earning; good that this is cited as a limitation.
- The Discussion section is longer than needed.

Tables:

- Table 1: both maternal and paternal educational attainment are provided. What percent of children's parents were together

at infancy?

- Table 2: could be more informatively provided as a line graph over time.
- Tables 3 and 4 should state the name of the measure used for the DV.
- Tables 6 and 7 should include t and p values.
- The paper could really benefit from a figure (e.g., of Tables 3-7) to convey the findings.

Reviewer #3 (Remarks to the Author):

This is an interesting paper that capitalises on an existing longitudinal data-set to address questions related to the salience of temporal features of stress exposure in relation to young children's adjustment. Specifically, the authors examine whether the timing of stressors matters for adjustment problems (int and ext) at ages 3 and 5, consider whether the nature of the stressor (interpersonal or not) matters for adjustment at age 7 and assess potential protective effects of child characteristics and family support.

The background to these important questions is well presented and the statistical analyses appear rigorous and well-reported. Alongside sex differences, the findings offer some support for cumulative effects, sensitivity periods and proximity effects. The results also demonstrate both equifinality and multifinality, with some evidence for specificity of effects (e.g., only the control component of family resilience mattered for externalising problems).

These are interesting findings, and the study has several strengths (multiple time-points, consideration of protective factors as well as fine-grained analyses of the temporal salience of risk factors). However there are two key limitations, only one of which is directly addressed by the authors. As they note, recruitment for the original study (of emotion processing) included multiple exclusion criteria. As a result, the sample consists of predominantly affluent, well-educated parents with neurotypical children. The analyses are not pre-registered, and anyone setting out to examine how stress matters for adjustment problems would prioritise families facing multiple stressors. As such, there are serious questions to be asked regarding the generalisability of the study findings. Accentuating this point, the study relies on information from mothers for all measures, and this single informant design may well inflate associations. The authors do note that it would be valuable to include children's perspectives, but this was clearly not feasible given the children's young age. However, the conclusions would have much more strength if the adjustment problems had been rated by nursery staff or teachers (or even fathers).

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Version 1:

Decision Letter:

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Dear Dr Valdes,

Your manuscript titled "Stress Timing, Trauma Exposure, and Family Resilience Differentially Affect Internalizing and Externalizing Symptoms at 3, 5, and 7 years of Age: Findings from a Prospective Cohort Study" 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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Best regards,

Jennifer Bellingtier

Jennifer Bellingtier, PhD
Senior Editor
Communications Psychology

REVIEWERS' EXPERTISE:

Reviewer #1 stress, mental health, longitudinal designs

Reviewer #2 stress, mental health, longitudinal designs

Reviewer #3 stress, mental health, longitudinal designs

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I thank the authors for their careful consideration of my earlier comments and thoughtful responses. I think the manuscript is much improved. However, I still stand by my original recommendation that it is inappropriate to conclude aspects of family resilience "were protective" for internalizing/externalizing symptoms given the lack of appropriate temporal ordering needed to support such a claim. Similarly, I recommend that the authors expand their limitations section to explicitly mention that associations between the manuscript's putative measure of family "resilience" and psychopathology could arise through multiple processes, of which "better family adaptation buffering against the effects of stressors" is only one. This is, in my view, even more important than reminding readers that resilience is best assessed as a multisystem phenomenon because it directly impacts how this study's results might be interpreted by readers.

Reviewer #2 (Remarks to the Author):

This revised manuscript was highly responsive to reviewer comments. However, the limitations of the paper in measurement, sample, and robustness of findings stand. Nonetheless, the revised manuscript is improved in clarity and adds to a growing literature of potential protective factors that could be examined in prevention studies.

Reviewer #3 (Remarks to the Author):

I think the authors are to be commended for their serious approach to making the requested revisions. As such, I'm happy to approve for publication.

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Reviewer 1

(1) The authors cite work appropriately acknowledging “resilience” as a multisystem phenomenon but capture it using only a single parent-report measure of family functioning. This is a significant limitation, and discussion regarding the multiple causal pathways that might lead to associations between this measure and other study constructs like child psychopathology is underdeveloped. For example, in labeling it a measure of “resilience” the assumption seems to be that families scoring high on this measure may be less impacted by stressors. But it is also possible that fewer stressors could lead directly to higher scores on the measure, or that child psychopathology could lead directly to lower scores. There is no mention (that I could find) of previous literature suggesting any of these possibilities is more or less likely than any of the others. This ambiguity makes it difficult to know how to best interpret their findings, particularly ones suggesting only certain forms of family “resilience” (i.e., “control”) are associated with certain outcomes (i.e., externalizing symptoms).

We appreciate this point and the need to consider other forms of resilience beyond family functioning, especially as children grow older and increasingly function outside of the family environment. We have provided more discussion about other resilience constructs, according to the multisystems model, that may help to contextualize the current findings, limitations, and directions for future research. Below we have included a new paragraph added to the Discussion to capture some of the feedback provided. In the limitations section, we also highlight the need for continued research that includes multiple constructs (e.g., stress/adversity/trauma, different domains of family resilience) and that is longitudinal in nature to more thoroughly address the questions raised. In the current analyses, the final model included on resilience is cross-sectional, although it adjusts for the effects of trauma from birth to 7 years, and more work is needed to understand how stress, trauma, resilience, and psychopathology interact at given time points and with temporal feedback loops to produce risk. Both issues are now raised in the limitations section.

“Importantly, resilience factors are likely functioning across multiple systems. Additional research is needed to understand how each factor may interact to influence psychopathology outcomes. For instance, some prior research in adolescents suggests that individual resilience characteristics differentially provide protection for internalizing versus externalizing symptoms.

Specifically, low levels of negative cognitions (i.e., a lower tendency towards worry, ruminative thinking, or pessimism) were shared protective factors for all psychopathology in the context of sexual trauma, whereas high levels of social skills and confidence were uniquely protective for internalizing symptoms, and high levels of connectedness and empathy/tolerance were protective for externalizing symptoms (Pérez-González et al., 2017). Additional research is needed at younger ages and with more types of traumatic experiences captured to determine how different forms of resilience may interact to differentially confer protection.”

(2) The authors note in the discussion that “the effect sizes for the association between stressful events and psychopathology symptoms were strongest in the prenatal period and steadily declined with age through 3 years.” They interpret this as evidence supporting a potential “sensitive period” effect, after defining a sensitive period as a “period of greatest brain plasticity”. While it is theoretically possible that the infant brain could be more plastic prenatally vs. postnatally, it is important to note that this association could also arise for reasons that have nothing to do with brain plasticity, including differences in the types or magnitude of stressors experienced prenatally vs. postnatally, variation in how parental stress is transferred to offspring (e.g., cortisol crossing the placenta to effect the child prenatally), or developments in child cognitive capacity and coping ability. It would help contextualize the authors’ findings if they acknowledged these different possibilities and made it clearer that the current study has no effective way of distinguishing between them.

We agree completely that sensitive periods of exposure are not only limited to early childhood and neurodevelopmental processes. Sentences referencing sensitive periods have been updated throughout the manuscript to more accurately reflect all the processes that may be impacted by stress exposure during sensitive windows, including expanding our discussion to the prenatal period. An example is provided below from the Introduction.

We considered other reasons presented by the reviewer that might explain the timing effects such as differences in the magnitude of stress exposure or variability by developmental periods. We did not find evidence that time periods with greater effect sizes (e.g., the prenatal period) had a greater severity of stress exposure or more variability in stress exposure in the current sample. Stressors tended to increase with age, with the lowest levels during the prenatal period ($M=1.02$, $SD=1.31$) and highest levels by 2-3 years ($M=2.98$, $SD=2.27$). Stress exposures then declined slightly again from 3-4 ($M=1.48$, $SD=1.72$) and 4-5 years of age ($M=1.80$, $SD=1.91$). If the quantity or variability of stress exposures affected the observed effect sizes, we would expect the opposite pattern of results. That is, higher means and variability during the prenatal period and recent exposures when we observe the strongest effect sizes between stress and psychopathology symptoms.

“Another model, the “sensitive period model,” posits that the timing of exposure is crucial, with stress exposures during windows of vulnerability like the prenatal period and early childhood having the greatest impact on neurodevelopmental, endocrine, immune, and metabolic systems (i.e., biological embedding) (Bailey Jr et al., 2001; Berens et al., 2017; Dunn et al., 2013; Dunn et al., 2018; Knudsen, 2004).”

(3) The finding that stressful life events are associated internalizing symptoms at age 3 for females but not males (and that effects in males seem to, if anything, go in the opposite direction) is a curious one and deserves further exploration. Do the mean levels or variability in internalizing symptoms at this age differ significantly by sex? What about means/SDs for stressful life events? Can the authors tie this finding to any other previous publications or edit their discussion to speculate on a few possible explanations?

Levels of internalizing symptoms did differ by sex at 3 and 5 years of age but not at 7 years, which is why interaction effects with sex were tested in longitudinal models at those earlier time points. We did not test whether levels of stressful events differed by sex in our original submission, but we have tested that now and there were no significant differences by sex at any time point. We have also included further interpretation of the observed sex differences in the revised manuscript. Relevant sections are provided below:

“Bivariate analyses of the main study variables with all sociodemographic variables (child sex, child race, child ethnicity, parent education, annual household income) were conducted to identify potential covariates. Only one of the sociodemographic variables tested, child sex, was associated with psychopathology symptoms, specifically greater internalizing symptoms at 3 years ($r=.152$, $p=.001$) and at 5 years ($r=.118$, $p=.014$) among females than males.”

“Results showed a significant sex effect when examining internalizing symptoms at age 3 years in relation to stress exposures. Stressful events were associated with more internalizing symptoms in females but not in males, suggesting that the impact of stressful events on internalizing symptoms may be different for males and females in early childhood. By age 5 years, stress exposures from the prenatal period through age 5 years was associated with greater internalizing symptoms across sexes. Some systematic review work has found evidence for sex-specific programming of the hypothalamic-pituitary-adrenal (HPA) axis (Carpenter et al., 2017). Specifically, the placenta in pregnancies of female fetuses may have increased permeability to maternal glucocorticoids following stress exposure due to differences in enzymatic activity, with evidence for continued increased reactivity and sensitivity in female offspring (Carpenter et al., 2017). As such, the amount and chronicity of accumulated stress exposure needed to produce risk for internalizing symptoms may be greater in males than in females.”

(4) The authors’ decision to operationalize stressful/traumatic events exposure as a count of the number of different types of events children’s parents experienced during the reporting period is not without precedent but requires justification. This is particularly true given their stated aim of exploring potential timing effects, given the strong possibility that any timing effects might reflect differences in the types of stressful/traumatic experiences occurring in each reporting period (rather than developmental changes in plasticity). There is also some evidence indicating children’s age of peak “sensitivity” to different types of exposure may vary (e.g., Schaefer et al., 2022), which would argue against their “sum of endorsed events” approach.

We have provided further justification for the decision to focus on total stressful event exposure and two dimensions of trauma exposure (interpersonal versus non-interpersonal). We have referenced Schaefer and colleagues’ (2022) systematic review as evidence for the need to further

disentangle potential timing effects by type of exposure in future work. Revised text in the Introduction is provided below:

“For the purposes of the current study, stress and trauma exposures were conceptualized as total counts and along two dimensions (interpersonal vs. non-interpersonal trauma) (Evans et al., 2013), given that past work has found inconsistent findings across types of maltreatment (e.g., physical versus sexual abuse) or other dimensions (i.e., threat versus deprivation) (Schaefer et al., 2022). Although the precise sensitive periods identified in the current literature for type of stress exposure vary, sensitive period effects nonetheless are observed and may need to be disentangled in future work (Schaefer et al., 2022).”

Smaller comments:

(1) Ln. 128-129: Please explain what is meant by “may function on one spectrum”

We have provided further clarification for this sentence and include the updated paragraph from the Introduction (#5) below:

“An additional limitation of the existing literature is the conflation of stress, adversity, and trauma, as it has been difficult to establish clear conceptual boundaries between the three (Krupnik, 2019). Existing conceptualizations suggest that normative stress and adversity may function on one dimensional continuum, whereas trauma may function categorically (Krupnik, 2019). That is, stress may range from mild (e.g., relocating residences by choice) to more severe experiences (e.g., experiencing homelessness) (Hosseini-Kamkar et al., 2021). Trauma is defined more narrowly as events that would meet diagnostic criteria in the Diagnostic and Statistical Manual of Mental Disorders (DSM), such as: “death, threatened death, actual or threatened serious injury, or actual or threatened sexual violence” (APA, 2000). However, others suggest that they function on the same dimensional continuum, and that trauma should be conceptualized by the subjective experience of an event, rather than a more narrowly defined set of experiences (Hosseini-Kamkar et al., 2021). Newer models advocate for hybrid conceptualizations, whereby experiences of stress are functioning on a continuum of severity, and in combination with one’s self-regulatory capacity, an individual can experience normative stress, pathogenic stress, or traumatic stress (Krupnik, 2019).”

(2) Ln. 226-234: Please clarify in the description of the “traumatic events” measure whether it relies on parent or child report

The traumatic events measure used for the current study retrospectively assesses traumatic events from birth to the time of the assessment (7 years) and was completed via parent report to ensure that early life experiences could be remembered and reported. This information has been included in the Methods section and is also copied below:

“Potentially traumatic events experienced by the child were measured via the Traumatic Events Screening Inventory (TESI) (Strand et al., 2005), a retrospective structured clinical interview completed by the child’s parent at the 7-year visit.”

Reviewer 2

Abstract:

- Please add effect size indicators and p-thresholds for effects described.

Effect sizes and p-values have been added to the Abstract:

“At 3 years, greater stressful events during the prenatal period, 1-2 years, and 2-3 years ($B=.833-.369$, $p=.028-.046$) predicted internalizing symptoms for females only. For externalizing symptoms at 3 years, every time point assessed was significantly associated with more symptoms across both sexes ($B=1.071-.414$, $p<.001$). At 5 years, both internalizing and externalizing symptoms were associated with a greater number of stressful events at every time point and across sexes ($B=1.372-.465$, $p<.001-.002$). There was evidence for timing effects, including cumulative effects, sensitive periods, and recency effects. Exposure to interpersonal trauma associated with greater internalizing symptoms ($B=2.120$, $p=.002$), whereas both interpersonal ($B=1.879$, $p=.005$) and non-interpersonal ($B=1.223$, $p=.032$) traumatic experiences were associated with greater externalizing symptoms. Aspects of family resilience including higher levels of family commitment, ability to face challenges, and sense of control were protective for internalizing symptoms ($B=-.496$, $p=.004$) while only greater sense of control ($B=-.838$, $p=.040$) was protective for externalizing symptoms at age 7 years, including in the context of trauma.”

Intro:

- Is there a DALY estimate that is newer than 2019?

DALY counts by disorder and region are provided by the World Health Organization through 2021. However, to our knowledge, no peer-reviewed research has analyzed these data and provided global estimates across all mental health disorders to include the 2019-2021 data points.

- Is this statement in fact accurate, “most existing research has focused on middle childhood, adolescence, and adulthood, with limited longitudinal research examining outcomes in early childhood”?

More context has been provided for this statement in the Introduction:

“Most existing research has focused on middle childhood, adolescence, and adulthood, with relatively limited longitudinal research examining outcomes in early childhood. For instance, a recent meta-analysis ($N=62$) on the association between early life stress and depression in childhood and adolescence (<18 years) only included four longitudinal studies, six studies of children 7-10 years (of which only one was longitudinal), and no studies of children under 7 years (LeMoult et al., 2020).”

Methods:

- Participants: A bit more context about the source of participants would be helpful; for example, specify the general location of “local births” and the years when participants were recruited.

More information has been provided about the participants in the Methods section, including the general location of recruitment and the timeframe for study procedures. The two relevant sentences are included below:

“Participants were recruited from a registry of local births in the Greater Boston area comprising families who had indicated willingness to participate in developmental research.”

“The infancy visits started in April 2013 and ended in April 2017. Data collection for the 7-year visits continued through September 2023.”

• Participants: please clarify how many participants had internalizing and externalizing data at all 3 time points?

This information has been added to the Methods section:

“To be included in the current analyses, participants needed to have parent-reported child psychopathology data at one of the time points assessed: at 3 years ($n=456$ for internalizing scores; $n=441$ for externalizing scores), 5 years ($n=435$ for both internalizing and externalizing scores), or 7 years of age ($n=388$ for both internalizing and externalizing scores). A total of 246 participants had internalizing symptoms reported across all three time points, and 235 participants had externalizing symptoms reported across all three time points.”

• Measures: were the forms self-completed or completed via a research associate?

The measures used for the current analyses were self-completed by parents during the study visit. This information has been included in the “measures” section and is also below:

“Questionnaires, described below, were administered via REDCap, an online platform, to the child’s parent, primarily the child’s mother (97.8%). All measures were self-completed by parents.”

• Measures: please provide some psychometrics for the RLEQ and TESI scales used.

Reliability and validity information for both measures have been included in the Methods section and are also included below. Internal consistency estimates for the current sample are not calculated, as they are not appropriate given that participants are expected to have variability in number of experiences and consequently items are not expected to hang together across total scales or factors. Internal consistency estimates are not provided in the measures’ psychometric validation studies for this reason as well.

Recent Life Events Questionnaire:

“In validation studies, the RLEQ has shown high levels of concurrent validity ($SN=1.0$, $SP=.88$) in capturing stressful events in the past three months relative to more extensive interview data, and high inter-rater agreement across informants ($k=.88$) (Brugha & Cragg, 1990).”

Traumatic Events Screening Inventory:

“Validation studies with both parent and child report suggest that the TESI had mean inter-rater agreements of 97% ($k=.88-1$), and provide evidence of both convergent validity with other trauma measures and predictive validity for PTSD more broadly (Ford et al., 2022).”

Results

- **It is surprising that parent education and annual household income were not linked with outcomes. Was resilience examined in relation to parent education and annual household income as well?**

Resilience was not examined in relation to parent education and annual household incomes as a part of our original analytical plan. In response to this point, we have run bivariate analyses. These variables are not associated with any of the three resilience domains. We did not interpret or discuss these findings in the manuscript as they were not a part of the study’s aims. Further, we believe that these null findings could be due to range restrictions in the current sample and thus do not want to over-interpret the lack of associations.

- **Given the large number of analyses run, a p-threshold for considering significance should be established.**

All hypotheses were two-tailed, and significance was set at $p < 0.05$. This threshold is now included in the Methods section. Regarding the number of analyses, Aims 1 to 3 tested two hypotheses about internalizing and externalizing symptoms, respectively, across three time points (six total hypotheses). A single model was run to test each of these hypotheses for a total of six models. Aim 4 also included two hypotheses and was tested using the same two regression models as for Aim 3, adding new variables for resilience (hierarchically). Supplemental Ridge Regression models are included replicating some of the traditional regression models to reduce observed variance due to multicollinearity, but they do not include significance estimates by design and are not hypothesis-testing. We have moved Ridge Regression models to a supplement to avoid confusion for readers.

Significance thresholds were not adjusted, as guidelines recommend that adjustments be made if an omnibus test cannot be conducted for each hypothesis. Each of the models run included omnibus tests of significance rather than post-hoc comparisons and often adjusted for existing effects of other variables (e.g., mixed models, hierarchical regression models). That is, variables within mixed and regression models are adjusting for the effects of other variables and are not multiple independent comparisons of significance run to test a single hypothesis. Consequently, thresholds were not decreased to balance the risk of type II errors due to over-correction.

García-Pérez, M. A. (2023). Use and misuse of corrections for multiple testing. *Methods in Psychology*, 8, 100120.

- **The resilience findings are interesting; p-values should be provided in this paragraph, as in the prior paragraphs.**

P-values are provided for all Linear Regression models for Aim 4 on resilience but are not generated by Ridge Regression (RR) models. RR models do not allow for variable selection and interpretation through significance and hypothesis testing but do account for potential collinearity, which is why they were included in current analyses. We have moved the RR model

tables to a supplement to avoid confusion for readers. More explanation is provided in the Methods section:

“A supplemental analysis for this aim was conducted using a Ridge Regression (RR) model to attain additional parameter estimates that better account for multicollinearity (Hoerl & Kennard, 1970). RR is a modified version of the least squares method that relies on shrinkage, also referred to as regularization, to bias coefficient estimates towards 0 and reduce the variance caused by multicollinearity (Ahmad et al., 2006). RR estimates have negligible increases in bias and are believed to better reflect true parameter values, improving model accuracy (Ahmad et al., 2006). However, a limitation of RR models is that, although they enhance the overall performance and reliability of linear models, they can limit the interpretability of individual coefficients. RR models do not allow for variable selection (e.g., through significance and hypothesis testing), but do allow for estimations of regression coefficients. Consequently, results from both OLS and RR models were run for this aim to enhance both the interpretability of findings and model accuracy by limiting the effects of any potential collinearity.”

Discussion

• The sample is quite white, educated, and high earning; good that this is cited as a limitation.

We agree that this is an important limitation that we tried to emphasize in the Discussion. Our research group is actively collecting longitudinal data to determine the generalizability of our findings in under-represented populations. The current findings do provide preliminary evidence for the deleterious effects of stress and trauma exposure during the first 7 years of life on psychopathology outcomes, even in the context of presumably lower levels of exposures to stress, suggesting that potential effects in higher stress contexts may be even more pronounced. However, more research is needed to determine whether the types of stressors, degree, timing, and chronicity of effects are generalizable. We are including the relevant section from the limitations section of the Discussion below:

“Study limitations should be considered when interpreting the study’s findings. The sample comprised predominantly middle to upper income, well-educated families living in an urban area of the United States. Existing meta-analytic work in the United States suggests that low family income, low Hollingshead index (education, occupation, sex, and marital status), low subjective socioeconomic status, low parental education, poverty status (being below the poverty line), and receipt of public assistance are all associated with higher levels of child psychopathology from 5 to 18 years of age (average age of 12) (Peverill et al., 2021). Consequently, future research is needed to test these models in more diverse samples and determine whether the findings are comparable in the context of higher stress and trauma exposure.”

• The Discussion section is longer than needed.

Given feedback from other reviewers and the editorial staff to further interpret study findings and emphasize limitations, the Discussion section’s length was retained. We believe that the length is commensurate with the recommended level of nuance necessary to accurately convey study findings. Nonetheless, we are open to further feedback to make this section more concise if it would benefit the quality of the manuscript after review of the revisions incorporated.

Tables:

- **Table 1: both maternal and paternal educational attainment are provided. What percent of children's parents were together at infancy?**

Marital status has been added to Table 1, including frequencies and percentages by category.

- **Table 2: could be more informatively provided as a line graph over time.**

A figure has been added to depict information provided in Table 2 (Figure 1).

- **Tables 3 and 4 should state the name of the measure used for the DV.**

The specific measures used (ITSEA for Table 3; CBCL 1^{1/2}-5 for Table 4) have been added to both tables.

- **Tables 6 and 7 should include t and p values.**

Tables 6 and 7 use Ridge Regression models, which do not produce t- and p-values. RR models do not allow for variable selection and interpretation through significance and hypothesis testing. RR models were used for the current analyses because, unlike traditional regression models, they do not assume independence, which allows for the inclusion of correlated residuals in models. We have moved these two tables to a supplement to avoid confusion for readers.

- **The paper could really benefit from a figure (e.g., of Tables 3-7) to convey the findings.**

Figure 2 has been added to depict findings from hierarchical regression models. Mixed models were not depicted in figures, as multiple dimensions are modeled (child sex, stressful events, time, random effects, psychopathology symptoms).

Reviewer 3

...However there are two key limitations, only one of which is directly addressed by the authors. As they note, recruitment for the original study (of emotion processing) included multiple exclusion criteria. As a result, the sample consists of predominantly affluent, well-educated parents with neurotypical children. The analyses are not pre-registered, and anyone setting out to examine how stress matters for adjustment problems would prioritise families facing multiple stressors. As such, there are serious questions to be asked regarding the generalisability of the study findings.

(Same Response to Reviewer 2) We agree that this is an important limitation that we tried to emphasize in our Discussion. Our research group is actively collecting longitudinal data to determine the generalizability of our findings in under-represented populations that are more likely to have higher levels of stress exposures. The current findings do provide preliminary evidence for the deleterious effects of stress and trauma exposure during the first 7 years of life on psychopathology outcomes, even in the context of presumably lower levels of exposures to stress, suggesting that potential effects in higher stress contexts may be even more pronounced. We are including the relevant section from the limitations section below:

“Study limitations should be considered when interpreting the study’s findings. The sample comprised predominantly middle to upper income, well-educated families living in an urban area of the United States. Existing meta-analytic work in the United States suggests that low family income, low Hollingshead index (education, occupation, sex, and marital status), low subjective socioeconomic status, low parental education, poverty status (being below the poverty line), and receipt of public assistance are all associated with higher levels of child psychopathology from 5-18 years of age (average age of 12) (Peverill et al., 2021). Consequently, future research is needed to test these models in more diverse samples and determine whether the findings are comparable in the context of higher stress and trauma exposure.”

Accentuating this point, the study relies on information from mothers for all measures, and this single informant design may well inflate associations. The authors do note that it would be valuable to include children's perspectives, but this was clearly not feasible given the children's young age. However, the conclusions would have much more strength if the adjustment problems had been rated by nursery staff or teachers (or even fathers).

A review of the existing literature and recommendations suggests that reports across informants (e.g., parents, teachers) tend to be reliable and consistent. When discrepancies are observed, they are typically the result of variations in behavior across settings rather than differences in informant perceptions. Given these findings, the current longitudinal study is implementing observations from other raters (e.g., child self-report) in upcoming longitudinal visits through later childhood and adolescence (11 years of age and onwards). It is expected that, at these older ages, children will have more language/metacognitive thinking to accurately describe experiences of psychopathology. Moreover, age 8 years is typically considered the youngest age at which children can be expected to reliably self-report psychopathology symptoms. As the Reviewer notes, this is important, as parents have limited ability to observe the child in non-home settings and are likely receiving less detailed reports of their child’s behavior from teachers relative to younger ages. More information has now been included in the limitations section:

“Additionally, given that this work was conducted in early childhood, parent-report measures were used, which appears to be consistent with other informant reports (e.g., teachers/childcare providers) at younger ages. Existing evidence suggests that, as children become more independent and parents receive less information about their child’s experiences at school (e.g., after elementary school), parent ratings can become discrepant with other informants (e.g., teachers, child self-report) if there are differences in behavior across settings (De Los Reyes, 2013). As such, future longitudinal work examining the current models in adolescence should include respondents who can capture behaviors across settings (e.g., teacher, parent, child).”

Reviewer #1

I thank the authors for their careful consideration of my earlier comments and thoughtful responses. I think the manuscript is much improved. However, I still stand by my original recommendation that it is inappropriate to conclude aspects of family resilience “were protective” for internalizing/externalizing symptoms given the lack of appropriate temporal ordering needed to support such a claim.

We appreciate the reviewer’s point of view and careful attention to language. It is our understanding that protective factors typically refer to attributes that are associated with positive adjustment or development, including reduced psychopathology symptoms. As with risk factors, protective factors refer to associations and do not imply causality or temporal ordering. Nonetheless, we have followed the reviewers feedback and updated language throughout the manuscript from “protective” to “reduced risk”. We are also open to amending language further if other terms may be more appropriate.

Similarly, I recommend that the authors expand their limitations section to explicitly mention that associations between the manuscript’s putative measure of family “resilience” and psychopathology could arise through multiple processes, of which “better family adaptation buffering against the effects of stressors” is only one. This is, in my view, even more important than reminding readers that resilience is best assessed as a multisystem phenomenon because it directly impacts how this study’s results might be interpreted by readers.

We have expanded the limitations section to address this point explicitly: “Finally, family resilience may only be one process, among many possible processes, through which the effects of stress on psychopathology may be buffered and further research on resilience is necessary.”