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Predicting Parenting Inconsistency Using Machine Learning: Executive Dysfunction, Stress Reactivity, and Role Overload

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the Introduction, the phrase “parenting inconsistency…has emerged as a critical construct” would benefit from stronger empirical grounding by including quantitative prevalence rates or effect sizes from prior meta-analyses to substantiate the claim of its criticality .

The paragraph beginning “Executive dysfunction has been identified as a central cognitive mechanism…” is theoretically rich but lacks a clear distinction between trait-level and state-level executive functioning; the authors should clarify whether BRIEF-A captures stable deficits or context-sensitive fluctuations .

In the Methods section, the sentence “internal consistency coefficients (Cronbach’s alpha)…exceeded 0.80” would benefit from reporting exact alpha values for each scale to enhance psychometric transparency and allow comparison with prior studies.

In the Discussion, the claim “provides strong empirical support for the conceptualization…” appears overstated given the cross-sectional design; the authors should temper causal language and emphasize predictive rather than explanatory inference.

Response: Revised and uploaded the new document.

1.2. Reviewer 2

Reviewer:

The description "outlier detection using Mahalanobis distance" requires specification of the threshold (e.g., χ^2 critical value) used to identify outliers, as well as how many cases were removed or retained, to ensure replicability of preprocessing steps .

In the Data Analysis section, the sentence "cross-validation procedures applied" is insufficiently detailed; the authors should specify the type (e.g., k-fold, leave-one-out), number of folds, and whether stratification was used to avoid overfitting .

The statement "Accuracy, mean squared error (MSE), coefficient of determination (R^2), and AUC-ROC" suggests a mix of regression and classification metrics; the authors must clarify whether parenting inconsistency was modeled as continuous, categorical, or both, as this affects interpretability of results .

In Table 1, the reported correlation "r = 0.56" between executive dysfunction and parenting inconsistency is described as "moderate to strong," but the authors should justify this classification using established effect size benchmarks (e.g., Cohen's criteria) .

The sentence "the sample…was composed of 61.17% mothers and 38.83% fathers" suggests a gender imbalance, and the authors should test for gender differences in key variables or include gender as a control/moderator in the predictive models .

In Table 2, the performance metric "Accuracy = 0.86" for Gradient Boosting is reported without a confidence interval or standard deviation across folds; reporting variability is essential to assess model stability .

The statement "Gradient Boosting achieved the highest performance" would benefit from statistical comparison (e.g., paired t-tests, DeLong test for AUC) to determine whether differences between models are statistically significant .

In the Feature Importance section, the authors state "executive dysfunction…most influential predictor," but they should clarify the method used (e.g., Gini importance, permutation importance, SHAP values), as different approaches yield different interpretations .

Response: Revised and uploaded the new document.

2. Revised

Editor's decision: Accepted.

Editor in Chief's decision: Accepted.

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