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Clustering Adolescent Emotion Regulation Profiles via Gaussian Mixture Models

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1. Round 1

1.1. Reviewer 1

Reviewer:

The literature review is impressively comprehensive, integrating psychopathology, digital behavior, family systems, and intervention findings; nonetheless, the density of citations and breadth of topics risk diluting the central conceptual thread, so I recommend pruning some tangential material (e.g., the more peripheral digital/addiction sub-sections) and more explicitly organizing the introduction around a smaller set of core theoretical propositions that directly motivate the latent profile analysis.

The methodological decision to use CERQ and ERQ subscales as indicators is conceptually sound and consistent with the goal of capturing both adaptive and maladaptive ER strategies, yet the paper should report more detailed psychometric information for this specific adolescent sample (e.g., internal consistency coefficients for each subscale, evidence of factor structure if available, and justification for using subscale scores rather than latent factors) to strengthen confidence in the validity of the indicators entering the GMM.

The description of data screening and handling of missing data is appropriate and up-to-date (e.g., FIML, examination of skewness/kurtosis), but key quantitative details are currently underreported; the authors should provide the exact percentage of missingness per variable (or overall range), the precise skewness and kurtosis values (or their ranges), and a short justification for treating the distributions as approximately normal, given that GMM solutions can be sensitive to non-normality and missing data patterns.

The Gaussian Mixture Modeling procedure is generally well framed, with appropriate use of criteria such as BIC, AIC, BLRT, and entropy; still, the model selection process would be more transparent if the authors presented a summary table of all candidate models (1–6 clusters) including fit indices, entropy, smallest class size, and BLRT p-values, and briefly discussed any trade-offs between statistical fit and substantive interpretability, especially if the chosen solution is not the one with the absolute lowest BIC.

The manuscript would greatly benefit from a clearer and more nuanced description of the identified emotion regulation profiles themselves: beyond labeling them (e.g., "adaptive-dominant," "mixed," "maladaptive-dominant"), the authors should present standardized profile means (e.g., z-scores) on all ER strategies in a figure or table, emphasize effect-size differences between profiles, and offer theoretically grounded interpretations of what each configuration implies for adolescents' regulatory functioning rather than relying on broad descriptive labels.

While the focus of the paper is methodologically on clustering ER strategies, the reader needs more information about how these profiles relate to external criteria (e.g., depressive symptoms, NSSI, suicidality, problem behaviors, or digital addiction indicators) to establish external validity; if such outcomes were collected, profile differences on these variables should be systematically examined (e.g., distal outcome analyses), and if not, this limitation should be made explicit and framed as a key direction for future research.

The sample description is currently incomplete for evaluating generalizability and potential biases: the authors should report detailed demographic characteristics (age range, mean age and SD, gender distribution, socioeconomic indicators, urban/rural composition, and recruitment procedures including school or region selection) and discuss how these characteristics, together with the cultural context (Greek adolescents), may limit or shape the interpretation and cross-cultural applicability of the identified ER profiles.

Authors uploaded the revised manuscript.

1.2. Reviewer 2

Reviewer:

The description of the LPA procedures would benefit from greater transparency: the authors should report the full model selection indices (AIC, BIC, adjusted BIC, entropy, Lo–Mendell–Rubin LRT, and BLRT if available) for each tested class solution (e.g., 1–5 classes), justify the choice of the three-class model not only statistically but also conceptually, and provide information on class sizes, posterior probabilities, and classification accuracy to support the stability and interpretability of the identified profiles.

While the actigraphy protocol (14 consecutive days with a sleep diary) is a notable strength, the manuscript should supply more technical details (device model, scoring algorithm, epoch length, thresholds for defining sleep and wake, handling of non-wear time, and criteria for valid nights) and clarify how many nights per participant were retained after quality control, as these factors can substantially influence estimates of sleep onset latency, WASO, and sleep efficiency.

The handling of potential confounders and comorbidities appears limited; beyond controlling for age, gender, and SES, the authors should consider assessing and statistically adjusting for internalizing and externalizing psychopathology (e.g., depression, anxiety, ADHD), medication use beyond explicitly excluded psychotropics, and chronic health conditions, since these variables may simultaneously influence both sleep characteristics and emotion dysregulation and thus complicate causal interpretation of the observed associations.

Authors uploaded the revised manuscript.

2. Revised

Editor's decision after revisions: Accepted.
Editor in Chief's decision: Accepted.

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