




Machine-Learning Prediction of Therapeutic Alliance Quality from Client Readiness to Change and Affective Dysregulation

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1. Round 1

1.1. Reviewer 1

Reviewer:

The methodological pipeline is generally strong—prospective design, validated measures, multiple imputation, cross-validation, and an independent test set—but several critical analytic details (sample size, exact train–test split, k in k -fold CV, and the precise hyperparameter ranges searched) need to be reported explicitly to allow full evaluation and reproducibility.

While the choice of URICA and DERS is theoretically coherent, the methods section should provide more psychometric information (e.g., internal consistencies in the present sample, language/translation version details, and any adaptations) to reassure readers about measurement validity in this particular context.

The treatment of limitations and generalizability needs to be expanded: the manuscript should discuss the implications of recruiting from Canadian urban/suburban outpatient settings, potential sampling biases (e.g., language, diagnosis mix, severity), the absence of therapist- or treatment-level predictors, and the restricted outcome window (alliance at session 4) for the broader applicability of the findings.

From a reporting and transparency standpoint, the paper would benefit from closer adherence to established guidelines for prediction studies (e.g., TRIPOD or analogous frameworks), including clearer descriptions of missing data patterns, any

feature-selection steps, handling of potential multicollinearity, and whether model performance was compared to simpler baselines (e.g., linear regression or naive mean-prediction models).

Authors revised and uploaded the document.

1.2. Reviewer 2

Reviewer:

The description of the machine-learning models remains somewhat high-level; for a methods-oriented contribution, the authors should specify the exact model configurations for the final SVR, Random Forest, and MLP (e.g., kernel type and C/epsilon for SVR, number of trees, max depth, and feature subsampling for RF, architecture and regularization settings for MLP) and justify these choices relative to the data size.

The results section repeatedly characterizes the Random Forest model as explaining a “substantial proportion of variance” with “lower prediction error,” but this language is difficult to interpret without exact R^2 and RMSE values (ideally with confidence intervals or bootstrapped uncertainty estimates); these quantitative results should be clearly presented in both the text and tables.

The paper appropriately incorporates permutation importance and SHAP for model interpretability, yet the exposition of these findings is too cursory: readers would greatly benefit from concrete rankings or visualizations (e.g., SHAP summary plots) showing which specific URICA stages and DERS subscales most strongly influenced predictions, and how (direction and nonlinearity).

The discussion does a good job linking the findings back to clinical concepts such as readiness to change and emotional dysregulation, but it somewhat overstates the immediate clinical applicability of the models; the authors should clearly distinguish between in-sample feasibility and the need for external validation, calibration, and prospective impact evaluation before using such models in real-world triage or decision-support.

Authors revised and uploaded the document.

2. Revised

Editor’s decision after revisions: Accepted.

Editor in Chief’s decision: Accepted.