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Fairness-Aware Machine Learning for Predicting Diabetes Distress from Conscientiousness, Self-Efficacy, Medication Adherence, and Glycemic Control

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the participant-flow paragraph, the manuscript reports that "721 potentially eligible patients were approached," 699 consented, and 684 were included in the final analysis. Please provide a more transparent account of the exclusions. The exact numbers excluded because of unavailable HbA1c results, excessive questionnaire missingness, and ineligibility identified during verification should be reported separately. The authors should also compare included and excluded participants on any available demographic or clinical characteristics to assess potential selection bias. A participant-flow diagram would improve reporting, even though the main findings tables need not include demographic information. The manuscript should further clarify whether any participant withdrew after consent and whether repeated clinic attendees were screened to prevent duplicate enrollment.

The statement that the sample size "was considered sufficient for estimating predictive performance while also permitting subgroup-specific assessment" is insufficiently justified. Please add a formal sample-size rationale appropriate for prediction-

model development. The justification should consider the number of outcome events, the four primary predictors, the planned nonlinear algorithms, hyperparameter tuning, the 70:30 split, and subgroup fairness estimation. With 303 participants meeting the diabetes-distress criterion but only 91 positive cases in the test set, some subgroup-specific true-positive and false-positive estimates may be unstable. The authors should report the number of positive and negative cases within every sex, age, and residence subgroup in both the development and test sets and discuss whether these counts were adequate for reliable fairness auditing.

In the model-development paragraph, the authors state that penalized logistic regression, support vector machine, random forest, and gradient-boosted decision trees were evaluated. Please report the exact software environment, programming language, package names, package versions, algorithm implementations, hyperparameter search spaces, optimization method, number of tuning iterations, class weights, and stopping criteria. The manuscript currently provides only general descriptions of the tuned parameters. Reproducibility requires specification of whether gradient boosting was implemented through XGBoost, LightGBM, CatBoost, or another library, because these algorithms differ materially. The selected final hyperparameters for every model should be provided in supplementary material, together with the random seeds used during resampling and optimization.

Authors revised the manuscript and uploaded the document.

1.2. Reviewer 2

Reviewer:

In the eligibility paragraph, the authors state that participants were required to have “an available glycated hemoglobin measurement obtained within the preceding three months.” Please justify the three-month window and describe whether the timing between questionnaire completion and HbA1c assessment differed systematically among participants. HbA1c reflects glycemic exposure over approximately two to three months, but a value obtained nearly three months before distress assessment may not represent the same temporal state as a value obtained on the same day. The authors should report the median and range of the interval between HbA1c testing and questionnaire administration and conduct a sensitivity analysis restricted to participants whose HbA1c was measured within a narrower period, such as 30 days.

The paragraph defining the outcome states that “a mean score of 2.0 or higher indicated clinically meaningful diabetes distress.” Please provide a stronger justification for applying this cutoff in the Kenyan population. The manuscript should indicate whether the Diabetes Distress Scale has been psychometrically validated in Kenya or in culturally comparable East African populations. If the threshold was adopted from studies conducted elsewhere, the authors should acknowledge that the clinical meaning of the cutoff may vary across languages, healthcare systems, and cultural patterns of emotional expression. Sensitivity analyses using alternative thresholds or an ordinal classification of low, moderate, and high distress should be presented to demonstrate that model conclusions are not dependent on a single dichotomization decision.

The Data Collection Tools section states that Kiswahili versions were produced through forward translation, reconciliation, back-translation, and pilot testing with 30 adults. This is a useful procedure, but the psychometric evaluation remains incomplete. Please report whether the pilot participants represented the same educational, urban–rural, and clinical characteristics as the main sample. The authors should provide evidence of content validity, comprehensibility, and cultural equivalence and should report whether any items required substantive cultural adaptation. In the main sample, internal consistency alone is not sufficient. Confirmatory factor analyses or at least examination of the expected factor structures should be performed for the Diabetes Distress Scale, conscientiousness measure, self-efficacy scale, and medication-adherence scale across English and Kiswahili administrations.

In the paragraph describing conscientiousness, the manuscript identifies the instrument as the conscientiousness domain of the Big Five Inventory–2 but does not report the number of items, item scoring range after averaging, or treatment of missing item responses. These details are required for reproducibility. The authors should also explain whether the full domain or a short form was administered. Since language and literacy may affect responses to negatively worded personality items, measurement invariance across English and Kiswahili versions should be examined. Measurement invariance across sex may

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also be relevant because sex was a protected attribute in the fairness analysis. Without demonstrating comparable measurement, observed algorithmic disparities could partly reflect differential item functioning rather than genuine differences in predictor-outcome relationships.

The manuscript states that the 12-item Adherence to Refills and Medications Scale was used and that "higher scores indicate greater difficulty adhering to the prescribed treatment regimen." Please clarify whether this scale was previously validated for adults with diabetes in Kenya and whether medication adherence referred to all diabetes medications or only glucose-lowering medication. Participants receiving oral medication, insulin, or combined therapy may face substantially different adherence demands. The authors should consider adjusting for treatment regimen or examining whether the association between adherence and distress differs by insulin use. In addition, self-reported adherence is vulnerable to social-desirability bias; the discussion should more clearly distinguish reported adherence difficulties from objectively verified medication-taking behavior.

In the Methods paragraph stating that sensitive attributes "were retained in a separate audit dataset and were not used as predictors in the primary models," the conceptual basis for excluding these variables requires further explanation. Fairness through unawareness does not necessarily prevent discriminatory predictions because other variables may act as proxies for protected characteristics. The authors should clarify whether age, sex, and residence were excluded only from the feature matrix or also from preprocessing and hyperparameter selection. They should additionally report whether the four primary predictors had different distributions across protected groups and whether these distributions contributed to observed disparities. A supplementary comparison between models with and without protected attributes could help demonstrate whether exclusion improved or worsened calibration and error-rate equity.

The Data Analysis section reports a "70% development set and an independent test set containing the remaining 30%." Please explain whether the split was stratified only by diabetes-distress status or jointly stratified by outcome and protected groups. Stratification solely by the outcome may produce imbalanced subgroup representation in the test set, particularly for intersections such as rural men with distress. The random seed used for the split should be reported, and the authors should assess the stability of model rankings across repeated train-test partitions. A single random split can produce optimistic or pessimistic estimates. Nested cross-validation or repeated external holdout evaluation would provide a more robust estimate of predictive performance and fairness variability.

The missing-data paragraph states that "missing continuous values were imputed using the median of the corresponding training fold," but the extent and pattern of missingness are not reported. Please present the percentage of missing data for each predictor, outcome component, demographic attribute, and clinical variable. The authors should assess whether missingness differed according to diabetes-distress status or protected-group membership. Median imputation is operationally simple but may attenuate associations and underestimate uncertainty. Multiple imputation or model-based imputation should be considered as a sensitivity analysis. The manuscript must also clarify how participants with incomplete questionnaire items below the 20% exclusion threshold received scale scores and whether scale-specific scoring rules were followed.

Authors revised the manuscript and uploaded the document.

2. Revised

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

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