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## Common Pitfalls in Evaluating Model Performance and Strategies for Avoidance

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<b>Corresponding Author:</b>	C. P. James Chen, Ph.D. Virginia Tech Blacksburg, Virginia UNITED STATES
<b>First Author:</b>	C. P. James Chen, Ph. D.
<b>Order of Authors:</b>	C. P. James Chen, Ph. D.  Robin R. White, Ph.D.
<b>Abstract:</b>	This study critically examines the methodologies and metrics used for evaluating prediction models in regression and classification tasks, making a case for the application of rigorous and standardized approaches in model performance assessment. Within the context of this work, we define modeling as a structured framework for hypothesis formulation and decision-making, which relies on the analysis and extrapolation of empirical data. The advancement of modeling is contingent on the accumulation of prior knowledge within the scientific community. The study conducted a series of simulations to delve into common pitfalls in cross-validation (CV), a technique crucial for characterizing expected model performance on "new" data. Issues such as using the same data for both training and assessment, excluding model selection from CV, and overlooking experimental block effects were explored through simulation examples. Moreover, the simulations in this study highlight that no single model performance metric suffices to represent model performance adequately and conservatively, emphasizing the need for understanding the underlying theory of each metric to avoid misleading conclusions. In conclusion, this simulation study aims to guide researchers in accurately and consistently reporting model performance, thereby supporting integrity and scientific rigor in prediction modeling research.
<b>Suggested Reviewers:</b>	Ines Adriaens ines.adriaens@kuleuven.be  Lucio Flávio Macedo Mota flaviommota.zoo@gmail.com  Päivi Mäntysaari paivi.mantysaari@luke.fi  Scott Denholm scott.denholm@sruc.ac.uk

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Drs. Yanbo Huang, Lie Tang, Lirong Xiang, and Jiajia Li,  
Special Issue Editors, Agricultural Cybernetics, Computers and Electronics in Agriculture

Dear Drs. Yanbo Huang, Lie Tang, Lirong Xiang, and Jiajia Li,

I am writing to express my gratitude for the opportunity to submit our manuscript for consideration in the Special Issue on Agricultural Cybernetics., in Computers and Electronics in Agriculture. I, an Assistant Professor of Animal Data Science, along with my colleague Dr. Robin White, have developed a manuscript titled "Common Pitfalls in Evaluating Model Performance and Strategies for Avoidance," which focuses on an overview of metric definitions and applications and introduces common pitfalls in the model evaluation process. This topic is critical for ensuring the reproducibility and comparability of machine learning studies regardless of the field. Given the journal's emphasis on data-driven agricultural practices, we believe our manuscript can offer valuable guidelines for both the journal readers and authors.

Thank you for considering our submission. We eagerly anticipate your feedback and hope for the opportunity to contribute to this publication.

Sincerely,



**Chun-Peng James Chen, Ph.D.**  
Assistant Professor of Animal Data Sciences  
Virginia Tech, Blacksburg, VA, USA

## Highlights

- Model evaluation reliability is determined by the choice of cross-validation strategy.
- Misuse of model selection can lead to over-optimistic performance estimates.
- Overlooking experimental block effects can lead to biased model performance estimates.
- Regression metrics can be misleading when the data structure is complex.
- Classification metrics can be misleading when the class distribution is imbalanced.

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# COMMON PITFALLS IN EVALUATING MODEL PERFORMANCE AND STRATEGIES FOR AVOIDANCE

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A PREPRINT

 **C. P. James Chen\***

School of Animal Sciences  
Virginia Tech  
Blacksburg, VA 24061  
niche@vt.edu

 **Robin R. White**

School of Animal Sciences  
Virginia Tech  
Blacksburg, VA 24061  
rrwhite@vt.edu

May 14, 2024

## ABSTRACT

This study critically examines the methodologies and metrics used for evaluating prediction models in regression and classification tasks, making a case for the application of rigorous and standardized approaches in model performance assessment. Within the context of this work, we define modeling as a structured framework for hypothesis formulation and decision-making, which relies on the analysis and extrapolation of empirical data. The advancement of modeling is contingent on the accumulation of prior knowledge within the scientific community. The study conducted a series of simulations to delve into common pitfalls in cross-validation (CV), a technique crucial for characterizing expected model performance on “new” data. Issues such as using the same data for both training and assessment, excluding model selection from CV, and overlooking experimental block effects were explored through simulation examples. Moreover, the simulations in this study highlight that no single model performance metric suffices to represent model performance adequately and conservatively, emphasizing the need for understanding the underlying theory of each metric to avoid misleading conclusions. In conclusion, this simulation study aims to guide researchers in accurately and consistently reporting model performance, thereby supporting integrity and scientific rigor in prediction modeling research.

**Keywords** Model Evaluation · Performance Metrics · Simulation Studies

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\*Corresponding author: James Chen <niche@vt.edu>

17 **1 Introduction**

18 **1.1 Modeling**

19 Modeling is an essential tool for hypothesis formulation and decision-making. It functions as a structured investigatory  
20 framework that allows researchers to explore system understanding through the summary and analysis of empirical data.  
21 Carefully constructed and evaluated models offer the potential to extend this understanding by enabling the extrapolation  
22 of results to novel trials and conditions. Although only one focus of the science of modeling, the predictive role is  
23 often explicitly or implicitly the ultimate goal of models derived within the precision agriculture context. Through  
24 this lens, modeling provides opportunity to standardize and formalize research advancement, through developing  
25 quantitative constructs that accumulate prior knowledge derived by the broader the scientific community. Evaluating  
26 model performance becomes particularly critical when considering this role within the knowledge generation enterprise,  
27 necessitating a rigorous and standardized approach that allows for both reproducibility and comparability. As more and  
28 more model-based exercises are developed using slightly different methods, or slightly different datasets, it becomes  
29 increasingly challenging to evaluate, characterize, compare, and balance information generated by the resulting modeling  
30 tools, particularly when results are conflicting. Specifically, reporting model performance through poorly-defined  
31 metrics or incomplete procedures can create opportunity for confusion, misinterpretation, and miscommunication, and  
32 can ultimately result in distrust in model-based tools and impede scientific progress.

33 Here, we review two types of challenges that can be encountered during the model evaluation process: challenges in  
34 data structure and challenges in evaluation approach. Data structure challenges include those inherent to the types  
35 of data used in a modeling exercise. For continuous data types, challenges include measurement variance, extreme  
36 observations, and underlying variation structures like blocks. For categorical data, challenges largely center around the  
37 balance or lack thereof between categories. Challenges in the evaluation approach are driven by decision-making about  
38 which data are used for model derivation and which are used for evaluation. We will review these challenges in this  
39 study.

40 **1.2 Model Evaluation**

41 Model evaluation in the context of predictive analytics seeks to explore how well a model can generalize to new  
42 prediction contexts not seen during model training. Although commonly referred to as "model validation" in the  
43 literature, this term implies a false degree of confidence given that the word "validation" means to prove something  
44 true. There is no single test, or recognized suite of tests, to prove a model valid. Instead, the term "evaluation," which  
45 involves assessing the value, nature, character, or quality of something, is more fitting. It is essential to evaluate model  
46 performance on unseen data to ensure the approach is applicable to new experiments. To this end, cross-validation (CV)  
47 is widely recognized as a standard method for model evaluation.

48 The most common CV method is K-fold CV, which partitions the dataset into K equally sized folds. In each iteration,  
49 one fold is reserved as the test set (i.e., new data, noted as  $\mathcal{D}_{\text{test}}$ ), while the remaining folds are used as the training set

50 (noted as  $\mathcal{D}_{\text{train}}$ ) to construct the model. Once the model is trained, it is evaluated on the  $\mathcal{D}_{\text{test}}$  to obtain an estimate of  
51 the model performance  $\hat{g}$ . The process will iterate K times until each fold has been used as the  $\mathcal{D}_{\text{test}}$  once. The average  
52 performance over all K folds is deemed as the expected generalization performance of the model  $\mathbb{E}[\hat{g}]$  on new data.

53 However, there is always an evaluation bias between the estimated performance  $\mathbb{E}[\hat{g}]$  and the true generalization  
54 performance  $G$ , which can only be approximated by evaluating the same model on an infinite number of unseen data.  
55 Depending on the performance metric used in evaluation, a positive evaluation bias ( $\mathbb{E}[\hat{g}] - G$ ) typically suggests that the  
56 model evaluation procedure concludes a pessimistic estimation of the model performance, since the true performance  
57 is expected to be lower than the estimated performance. Another aspect of model evaluation error is the variance of  
58 each estimated performance  $\hat{g}$  across the K folds. For example, there are five estimates in a 5-fold cross-validation.  
59 The variance among these five estimates is defined as the evaluation variance. A high evaluation variance suggests that  
60 the performance is sensitive to the choice of data folds, and a small size or an over-complex model can lead to a high  
61 evaluation variance.

62 There is a trade-off relationship between the evaluation bias and variance from a squared evaluation bias, the derivation  
63 of the relationship is shown in the Eq. 23 in the Appendix. When performing K-fold CV with a fixed sample size  
64 and model complexity, the choice of K is the pivotal element shaping the model evaluation. When the K is set to a  
65 larger value; each training set  $\mathcal{D}_{\text{train}}$  is larger in size, resulting in a model trained on a more representative subset of the  
66 population of interest, leading to lower bias. However, a large K comes with a trade-off: the corresponding test subset  
67  $\mathcal{D}_{\text{test}}$  is compressed in size, making the tested model more sensitive to the specific data points, and thus inflating the  
68 validation variance. Conversely, a smaller K, along with a minor training set  $\mathcal{D}_{\text{train}}$ , reduces their representativeness and  
69 increases bias. Nevertheless, a larger size of the test set  $\mathcal{D}_{\text{test}}$  leads to more consistent estimations across the folds and,  
70 consequently, reduces the validation variance.

71 Leave-one-out cross-validation (LOOCV) is a variant of K-fold CV where K equals the sample size of the complete  
72 dataset  $\mathcal{D}$ . It provides an unbiased estimation of model performance because the training set  $\mathcal{D}_{\text{train}}$  closely resembles the  
73 unseen population of interest, given its size of  $N - 1$ , where  $N$  is the sample size. However, as the trade-off discussion  
74 suggested, this method can lead to high validation variance due to the model being evaluated on one sample at a time.  
75 The true unbiased nature of LOOCV is fully realized only when all K folds are utilized. Performing an incomplete  
76 LOOCV can introduce significant bias because of the inherent high validation variance, which often occurs when  
77 training each model iteration is prohibitively time-consuming or computationally demanding. In specific contexts, such  
78 as genomic prediction, strategies like the one described by Cheng et al. leverage the matrix inverse lemma, which  
79 allows for computational savings by avoiding the inversion of large matrices in each fold. This technique significantly  
80 reduces the dependency of computational resources on the sample size [1]. Van Dixhoorn et al. exemplify the use of  
81 LOOCV with a small dataset, aiming to predict cow resilience with limited data resources [2]. Nevertheless, for large  
82 datasets, LOOCV is generally not recommended due to computational inefficiency. Further details of bias-variance  
83 trade-off have been extensively explored in the statistical literature [3, 4].

**84 1.3 Model Selection**

85 Model selection becomes necessary when models are not entirely determined by the data alone. For example, in a  
86 regularized linear regression model such as a ridge regression [5] or the least absolute shrinkage and selection operator  
87 (LASSO) [6], it is essential to define a regularization parameter,  $\lambda$ , before fitting the model to the data. A larger  $\lambda$  value  
88 yields a more regularized model, which tends to reduce smaller coefficients to negligible values or zero. This approach  
89 helps in preventing overfitting noise in the training data. The definition of loss functions for the regularized models  
90 were described in 25 and 26 of the Appendix.

91 These pre-defined parameters, like  $\lambda$ , influence model fitting and remain constant during the training process. Such  
92 parameters are referred to as hyperparameters. Beyond regularized models, hyperparameters are crucial in other  
93 predictive models, enhancing flexibility and robustness. For example, in the Support Vector Regression (SVR) [7],  
94 the regressors X are projected onto a linear subspace to approximate the target variable Y. By choosing a suitable  
95 kernel function, which transforms the regressors into a non-linear space, as a hyperparameter, SVR can more effectively  
96 capture non-linear relationships, thus significantly improving model performance. Another hyperparameter example is  
97 the number of latent variables in the Partial Least Square (PLS) Regression [8], which condenses the original regressors  
98 into a more manageable set of latent variables, reducing multicollinearity issues. Fewer latent variables might lose  
99 significant information from the original regressors, while too many can lead to overfitting. Similarly, in Random Forest  
100 [9], hyperparameters such as tree depth and the number of trees dictate model complexity. The same applies to the  
101 number of hidden layers and the size of filters in convolutional neural networks [10]. All these examples highlight  
102 the fact that selecting the most suitable hyperparameters, which is known as hyperparameter tuning, is crucial for  
103 optimizing model performance. Feature selection is another crucial aspect of model selection. This process involves  
104 fitting the model to a selected subset of the original features, particularly essential in high-dimensional data scenarios  
105 where the number of features exceeds the number of observations, leading to poor model generalization. For instance,  
106 Ghaffari et al. sought to predict health traits in 38 multiparous Holstein cows using a metabolite profiling strategy. Out  
107 of 170 metabolites, only 12 were identified as effective discriminators between healthy and over-conditioned cows  
108 and were thus selected for the predictive model [11]. Therefore, optimizing feature subsets is a vital model selection  
109 strategy that significantly affects model performance. Including the model selection process within the cross-validation  
110 is essential to avoid common pitfalls. The risk of inflated model performance arises when model selection is guided  
111 by results on the test dataset. Even if the chosen model is subjected to k-fold cross-validation afterward, its selection  
112 bias toward the test set can lead to overestimating its efficacy. This issue has been highlighted in statistical literature  
113 [3]. A practical solution is to divide the dataset into training, validation, and test sets. The validation set is then used  
114 for model selection, ensuring the test set remains completely unused during the training phase, thereby providing a  
115 more accurate measure of model performance. For instance, the study by Rovere et al. exemplifies best practices in  
116 hyperparameter tuning and feature selection by employing an independent cross-validation step prior to assessing model  
117 performance. This approach enabled the precise selection of relevant spectral bands from the mid-infrared spectrum  
118 and the optimal number of latent dimensions in PLS with Bayesian regression for predicting the fatty acid profile in

119 milk [12]. Similarly, Becker et al. demonstrated a robust evaluation by using nested cross-validation loops; the inner  
120 loop conducted a grid search for the best hyperparameters in logistic regression, while the outer loop was designed to  
121 evaluate the performance of the resulting optimized model [13]. Both examples underscore the importance of separating  
122 model selection from performance evaluation to ensure the validity and reliability of the results.

123 **1.4 Cross Validation Design with Block Effects**

124 Blocking is an essential approach in experimental design to control for variations that can confound the variable of  
125 interest. For instance, Lahart et al. investigated the dry matter intake of grazing cows using mid-infrared (MIR)  
126 spectroscopy technology across multiple herds under varying experimental conditions [14]. Given the significant  
127 variation between herds, which may contribute to individual differences in both dry matter intake (i.e., response variable)  
128 and MIR spectra (i.e., independent variables), it is crucial to consider the herd as a blocking factor before evaluating the  
129 predictability of dry matter intake using MIR spectra. This consideration should also extend to model evaluation. In the  
130 cited study, variations in dry matter intake, the primary focus of the prediction model, were observed to exceed one  
131 standard deviation among some herds. In cross-validation, if samples from the same herd are assigned to different folds,  
132 with one fold used as the test set, the model is likely to achieve high accuracy. This accuracy may largely result from  
133 explaining the inter-herd variation rather than individual variations in dry matter intake, leading to an overestimation of  
134 model performance. To avoid this pitfall, block cross-validation, where each block (i.e., herd in this example) is used as  
135 a fold, is recommended for unbiased model evaluation. Literature reviews have indicated that block cross-validation  
136 effectively evaluates model performance on external or unseen datasets [15]. In the same study by Lahart et al., three  
137 cross-validation strategies were compared: random cross-validation (Random CV), which randomly assigns samples  
138 to folds; within-herd validation, training and testing the model within each herd; and across-herd validation (Block  
139 CV), where each herd is used as a fold and tested in turn. The results showed that performance estimates in block CV  
140 were noticeably lower than the other two strategies, supporting the hypothesis that ignoring block effects inflates model  
141 performance. Other studies considering block effects, including diet [16], herd [12], and farm location [17, 18], have  
142 shown similar results in cross-validation, demonstrating block CV's effectiveness in evaluating model performance on  
143 external datasets.

144 **1.5 Model Performance Metrics**

145 Model performance metrics serve as quantitative indicators for evaluating model performance. They are critical for  
146 benchmarking various modeling approaches and for evaluating hypotheses underpinning these different approaches.  
147 Choosing appropriate metrics to support hypothesis testing is crucial, as in-ideal selection may lead to overly optimistic  
148 conclusions. Due to the different goals of regression and classification tasks, it is critical to ensure that these different  
149 model types are evaluated using different metrics. As such, metrics for regression and classification are discussed  
150 individually.

151 **1.5.1 Metrics in Regression Tasks**

Table 1: Summary of model performance metrics for regression tasks.

Metric	Type	Scale-invariant	Range
Root mean square error (RMSE)	Error-Based	No	$[0, \infty]$
Mean absolute error (MAE)	Error-Based	No	$[0, \infty]$
Root mean squared percentage error (RMSPE)	Error-Based	Yes	$[0, \infty]$
Root mean standard deviation ratio (RSR)	Error-Based	Yes	$[0, \infty]$
Pearson's correlation coefficient ( $r$ )	Linearity-Based	Yes	$[-1, 1]$
Coefficient of determination ( $R^2$ )	Linearity-Based	Yes	$[-\infty, 1]$
Lin's concordance correlation coefficient (CCC)	Linearity-Based	Yes	$[-1, 1]$

152 Regression models aim to predict continuous variables and are commonly employed in diverse applications, such as  
 153 estimating body condition scores [19, 20], body weight [21, 22], milk composition [12, 18, 23, 24], efficiency of feed  
 154 resource usage [16, 25, 26], and early-lactation behavior [2]. The metrics in regression tasks evaluate the agreement  
 155 between the predicted value  $\hat{y}$  and the true values  $y$ . The agreement can be generally quantified in two ways: error-based  
 156 metrics and linearity-based metrics. The metrics are summarized in Table 1.

157 Error-based metrics focus on the deviation of each pair of predicted and true values, while linearity-based metrics  
 158 consider overall linear relationships between the predictions and the truths. The root mean square error (RMSE) and the  
 159 mean absolute error (MAE) are two common error-based metrics:

$$\text{RMSE} = \sqrt{\frac{1}{n} \sum_{i=1}^n (y_i - \hat{y}_i)^2} \quad (1)$$

$$\text{MAE} = \frac{1}{n} \sum_{i=1}^n |y_i - \hat{y}_i| \quad (2)$$

160 where  $y_i$  and  $\hat{y}_i$  are the true and predicted values, respectively, and  $n$  is the sample size. Both metrics preserve the scale  
 161 of the original data, making them easy to interpret in real-world units. Additionally, compared to MAE, RMSE penalizes  
 162 large errors more due to the squared term, making it more sensitive to outliers. In the cow production, monitoring  
 163 animal body weight is a common practice to aid in the management of dairy cows. Studies by Song et al. and Xavier et  
 164 al. have utilized RMSE to assess the effectiveness of three-dimensional cameras in estimating dairy cow body weight,  
 165 yielding RMSE values of 41.2 kg and 12.1 kg, respectively [21, 22]. These figures provide a straightforward value for  
 166 farmers to gauge whether the prediction error is tolerable, considering their specific operational costs and management  
 167 thresholds. In essence, RMSE translates complex model accuracy into practical insights for productive agricultural  
 168 units. When evaluating the same model across different traits, which may have different scales, a common practice is to  
 169 express error metrics in a scale-free manner. This can be achieved by expressing RMSE as a percent of the deviation  
 170 from the observed value, such as root mean squared percentage error (RMSPE), or as a Root Mean Standard Deviation  
 171 Ratio (RSR) that normalizes the RMSE by the standard deviation of the observed values:

$$\text{RMSPE} = \sqrt{\frac{1}{n} \sum_{i=1}^n \left( \frac{y_i - \hat{y}_i}{y_i} \right)^2} \quad (3)$$

$$\text{RSR} = \frac{\text{RMSE}}{\sigma_y} \quad (4)$$

172 where  $\sigma_y$  is the standard deviation of the observed values. When expressed as a percent, RMSPE typically ranges from  
 173 0 and above, with values closer to 0 indicating perfect prediction. Much like expressing RMSE as a percent, RSR is  
 174 valuable to interpret RMSE in terms of the context of the variance in the observations. Values below 1 suggest that the  
 175 model yields predictions less variable than the standard deviation, while values above 1 suggest that the prediction is  
 176 imprecise.

177 On the other hand, Pearson's correlation coefficients ( $r$ ) and the coefficient of determination ( $R^2$ ) are two common  
 178 linearity-based metrics:

$$\begin{aligned} r &= \frac{\text{cov}(y, \hat{y})}{\sigma_y \sigma_{\hat{y}}} \\ &= \frac{\sum_{i=1}^n (y_i - \bar{y})(\hat{y}_i - \bar{\hat{y}})}{\sqrt{\sum_{i=1}^n (y_i - \bar{y})^2 \sum_{i=1}^n (\hat{y}_i - \bar{\hat{y}})^2}} \end{aligned} \quad (5)$$

$$\begin{aligned} R^2 &= 1 - \frac{SS_{\text{residual}}}{SS_{\text{total}}} \\ &= 1 - \frac{\sum_{i=1}^n (y_i - \hat{y}_i)^2}{\sum_{i=1}^n (y_i - \bar{y})^2} \end{aligned} \quad (6)$$

179 where  $SS_{\text{residual}}$  is the residual sum of squares and  $SS_{\text{total}}$  is the total sum of squares. Each  $y_i$  and  $\hat{y}_i$  are the  $i$ th elements  
 180 of the actual response vector  $y$  and the predicted response vector  $\hat{y}$ , respectively.  $\bar{y}$  and  $\bar{\hat{y}}$  are their respective means.  
 181 Both  $r^2$  and  $R^2$  are scale invariant, meaning their values are unaffected by the scale of the observed data because they  
 182 are normalized by the variation in the denominator.

183 The correlation coefficient  $r$  measures the strength of the linear relationship between two continuous variables,  $y$  and  $\hat{y}$ ,  
 184 and ranges from -1 to 1. A value of 0 indicates no prediction accuracy in the evaluated model. One special characteristic  
 185 of correlation  $r$  is that it is unaffected by the scale of the predictions or biases; it focuses on the relative changes  
 186 in the predicted values compared to the true values. Thus, even if the prediction biases are scaled up or down, the  
 187 correlation  $r$  between  $\hat{y}$  and  $y$  remains the same. This property is particularly useful when the focus is more on ranking  
 188 predictions rather than their absolute values. For example, this metric has been used to evaluate models that identify  
 189 high-performing production individuals, demonstrating the ability to predict nutrient digestibility in dairy cows [26] and  
 190 to select models based on their ability to rank traits such as feed intake and milk composition in dairy cows [27, 12].

191 The coefficient of determination  $R^2$  quantifies model performance from the proportion of variance in the dependent  
 192 variable that is predictable from the independent variables. It ranges from negative infinity to 1, where 1 indicates  
 193 that the model explains all the variance in the dependent variable, and 0 indicates that the model performs no better

than predicting all samples as the mean of the observed values.  $R^2$  is useful in comparing multiple regression models, as demonstrated in studies that regress body weight of dairy cows on a set of morphological traits [22], examine the relationship between milk spectral profiles and nitrogen utilization efficiency [16], and evaluate the predictive performance of milk fatty acid composition [23].

It worth noting that many literatures have misinterpreted the relationship between  $r$  and  $R^2$ . The coefficient of determination  $R^2$  is not always equivalent to the square of the correlation coefficient  $r^2$ . The equivalence only holds when the same dataset is used for both model fitting and evaluation in a least squares regression model. The model assumes a zero covariance between the fitted residual and the predicted values  $\hat{y}$ , and it also assumes that the residuals (i.e., prediction biases) are centered on zero. In practice when predictions are made on new data, those assumptions are often violated, leading to discrepancies between  $r^2$  and  $R^2$ . A details derivation of the equivalence is provided in Equation 27–28 in the Appendix.

In addition to  $r^2$  and  $R^2$ , another linearity-based metric is Lin's concordance correlation coefficient (CCC) [28]:

$$\begin{aligned} \text{CCC} &= \frac{2r\sigma_y\sigma_{\hat{y}}}{\sigma_y^2 + \sigma_{\hat{y}}^2 + (\bar{y} - \bar{\hat{y}})^2} \\ &= \frac{2\text{cov}(y, \hat{y})}{\sigma_y^2 + \sigma_{\hat{y}}^2 + (\bar{y} - \bar{\hat{y}})^2} \end{aligned} \quad (7)$$

where  $r$  is the Pearson correlation coefficient. The CCC is a comprehensive metric because it considers both the correlation and the scale bias between the predicted and true values. It fills the gap left by  $r^2$  where the scale bias is ignored. Geometrically, CCC measures how well the predicted values  $\hat{y}$  fall on the 45-degree line in a scatter plot of the predicted (x-axis) and true values (y-axis). It is advantageous over  $R^2$  because it consistently ranges from -1 to 1, making it easier to interpret and compare across different studies. The CCC is crucial when precise predictions are required for both the scale and the rank of the trait of interest, such as in studies predicting cotton crop yields based on soil and terrain profiles [29].

### 1.5.2 Metrics in Classification Tasks

Classification models aim to predict categorical outcomes such as 'healthy' or 'sick,' 'susceptible' or 'resistant,' and 'high yield' or 'low yield.' To evaluate classification performance, one must first establish a confidence threshold to dichotomize the prediction probabilities. For instance, if a prediction probability exceeds the threshold, the sample is predicted as a positive sample. It is worth mentioning that this threshold is adjustable to fine-tune model performance for particular uses. The discussed metrics in classification tasks are summarized in Table 2.

Table 2: Summary of model performance metrics for classification tasks.

Metric	Label-invariant	Threshold-independent
Accuracy	No	No
Precision	No	No
Recall	No	No
F1 score	No	No
Matthews correlation coefficient (MCC)	Yes	No
Area under the precision-recall curve (AUC-PR)	No	Yes
Area under the receiver operating characteristic curve (AUC-ROC)	Yes	Yes
Area under the MCC curve (AUC-MCC)	Yes	Yes

219 Accuracy is the most straightforward metric for evaluating classification models:

$$\begin{aligned} \text{Accuracy} &= \frac{\text{Total Correct Predictions}}{\text{Total Predictions}} \\ &= \frac{\text{TP} + \text{TN}}{\text{TP} + \text{TN} + \text{FP} + \text{FN}} \end{aligned} \quad (8)$$

220 where TP, TN, FP, and FN represent the number of true positives, true negatives, false positives, and false negatives,  
 221 respectively. It summarizes an overall model performance by calculating the proportion of correctly classified samples  
 222 among all samples. Nonetheless, accuracy can be misleading when the classes are imbalanced. For example, if a study  
 223 predicting the presence of a specific event, of which the prevalence was only 10%. In this case, a model that predicts all  
 224 samples as negative would achieve an accuracy of 90%, which is misleadingly high. To address this issue, precision and  
 225 recall are introduced:

$$\begin{aligned} \text{Precision} &= \frac{\text{TP}}{\text{Total Predicted Positives}} \\ &= \frac{\text{TP}}{\text{TP} + \text{FP}} \end{aligned} \quad (9)$$

$$\begin{aligned} \text{Recall} &= \frac{\text{TP}}{\text{Total Actual Positives}} \\ &= \frac{\text{TP}}{\text{TP} + \text{FN}} \end{aligned} \quad (10)$$

$$\text{F1} = 2 \times \frac{\text{Precision} \times \text{Recall}}{\text{Precision} + \text{Recall}} \quad (11)$$

226 Precision and recall refine the assessment of a classification model by offering insights that accuracy alone may  
 227 overlook. Precision calculates the fraction of true positives among all positive predictions, essentially measuring  
 228 the trustworthiness of positive predictions made by the model (Eq. 9). High precision is crucial in scenarios where  
 229 false positives incur significant costs, and false negatives are more tolerable. For instance, in contexts where clinical

treatments and culling are expensive, such as detecting bovine tuberculosis [30] or mastitis [31] using non-invasive methods, a high-precision model is crucial to minimize unnecessary costs and interventions from false positives. On the other hand, recall, also known as sensitivity, quantifies the ratio of true positives to all actual positives, assessing the model's ability to identify positive cases (Eq. 10). High recall is essential where missing a positive case has serious consequences, or where false positives are easily rectifiable. For instance, detecting lameness or abnormal gait is crucial, as these can indicate underlying pathologies [32] and impact welfare-related transport decisions [33]. An automated detection system [32, 34, 35] with high recall can mitigate economic losses by flagging at-risk cows. The benefit here lies in the feasibility of re-examining false positives, thus preventing more severe outcomes from undetected cases. Lastly, the F1 score, which is the harmonic mean of precision and recall, provides a balanced measure of model performance (Eq. 11). It is usually used as an overall performance metric when precision and recall are equally important.

However, it is worth emphasizing that precision and recall focus predominantly on positive samples. Inappropriately assigning a predominant background event as the positive class can lead to skewed interpretations. Hence, the Receiver Operating Characteristic (ROC) curve provides an another crucial tool for assessing a model's performance in a label-agnostic manner, meaning it is not biased by the class distribution as precision and recall are. An ROC curve plots one minus specificity against sensitivity. The equations for specificity and sensitivity are as follows:

$$\begin{aligned} \text{Specificity} &= \frac{\text{TN}}{\text{Total Actual Negatives}} \\ &= \frac{\text{TN}}{\text{FP} + \text{TN}} \end{aligned} \tag{12}$$

$$\begin{aligned} \text{Sensitivity} &= \text{Recall} \\ &= \frac{\text{TP}}{\text{Total Actual Positives}} \\ &= \frac{\text{TP}}{\text{TP} + \text{FN}} \end{aligned} \tag{13}$$

A model's effectiveness, as depicted on the ROC curve, is gauged by how closely a point on the curve approaches the top-left corner. A steep ascent from the left side of the curve signifies the model's ability to correctly identify most true positives while incurring a low rate of false positives. A random guess, with a 50% chance of correct prediction, corresponds to a diagonal line on the ROC curve. In dairy science, the ROC curve has been extensively utilized, for example, in predicting mastitis from milk composition [36] and diagnosing pregnancy using spectroscopy technology [37]. In this hypothetical example, the ROC curve also demonstrates robustness and label-invariance with a consistent AUC of 0.875, regardless of whether the original or inverted labels are used.

253 In addition to the metrics, the Matthews Correlation Coefficient (MCC) provides a symmetrical measure of the quality  
 254 of binary classifications. The MCC considers both positive and negative samples in the dataset, providing a balanced  
 255 measure of a model's performance [38]. It is defined as:

$$\text{MCC} = \frac{\text{TP} \times \text{TN} - \text{FP} \times \text{FN}}{\sqrt{(\text{TP} + \text{FP})(\text{TP} + \text{FN})(\text{TN} + \text{FP})(\text{TN} + \text{FN})}} \quad (14)$$

256 The equation 14 symmetrically incorporates all four components of TP, TN, FP, and FN). This symmetry makes MCC  
 257 invariant to class distribution changes. The coefficient ranges from -1 to 1, where 1 indicates perfect classification,  
 258 0 indicates no better performance than random guessing, and -1 signifies total disagreement between prediction and  
 259 observation. In a case study that used feeding and daily activity behaviors to diagnose Bovine Respiratory Disease  
 260 in dairy calves, MCC proved particularly insightful [39]. The models in this study exhibited strong performance on  
 261 negative samples (i.e., healthy calves), which were more prevalent, resulting in high specificity. However, sensitivity  
 262 was relatively low at 0.54. In this context, MCC, with a value of 0.36, provided a more nuanced and representative  
 263 measure of model performance, especially given the skew towards negative samples.

## 264 1.6 Study Objectives

265 This simulation study aims to highlight how biased or over-optimistic estimations of model performance usually come  
 266 from inappropriately conducting CV, a technique crucial for characterizing expected model performance on “new”  
 267 data. We demonstrate how common pitfalls, including using the exact data for both training and model assessment,  
 268 excluding the model selection process from CV, and neglecting experimental block effects, contribute to challenges  
 269 in model evaluation. Further, we scrutinize common metrics used in evaluating prediction models, including those  
 270 used for regression and classification tasks. Because no single metric provides a comprehensive perspective of model  
 271 performance, we seek, through this work, to highlight the importance of understanding the underlying theory of each  
 272 metric to avoid misleading conclusions.

273 There are five simulation studies being conducted to address these challenges. The first simulation study will focus  
 274 on the bias-variance trade-off in CV, demonstrating how the choice of K in K-fold CV affects the evaluation bias and  
 275 variance. The second simulation study will investigate the impact of mistakenly using the same data for model selection  
 276 and evaluation, highlighting the inflated model performance. The third simulation study will explore the effect of  
 277 excluding block effects in CV, demonstrating how ignoring block effects can lead to over-optimistic model performance.  
 278 The fourth simulation study will present four hypothetical predictions made in the same regression tasks, leading to  
 279 different interpretations with different metrics. The fifth simulation study will demonstrate the impact of imbalanced  
 280 data on classification model evaluation, showing how the choice of metrics can lead to misleading conclusions. Overall,  
 281 this series of simulation studies aims to guide researchers in accurately and consistently reporting model performance,  
 282 thereby supporting integrity and scientific rigor in prediction modeling research.

283 **2 Materials and Methods**

284 **2.1 Study 1: Evaluation bias and variance of cross-validation**

285 This study investigated the interplay between sample size and various performance estimators and their collective  
 286 impact on bias and variance during model evaluation. It is hypothesized that increasing the sample size will reduce  
 287 both bias and variance. Additionally, it is expected that the validation variance will increase with the number of folds  
 288 in the CV, while simultaneously reducing bias. Since K-fold CV employs a fraction (i.e.,  $K - 1$  folds) of the data  
 289 for training, it may provide a pessimistic estimate of model performance. Hence, this study designed to assess the  
 290 underestimation from each performance estimators, including K-fold CV with K set to 2, 5, and 10, as well as LOOCV  
 291 where K equals the sample size N, and the "In-Sample" evaluation, which assesses model performance on the same  
 292 dataset used for training, potentially leading to an overly optimistic bias. To gauge model performance, three metrics  
 293 are employed: RMSE (Eq. 1), r (Eq. 5), and  $R^2$  (Eq. 6). The validation model is a multivariate linear regression with  
 294 ten input features and one output target, all drawn from a standard normal distribution  $\mathcal{N}(0, 1)$ , implying no expected  
 295 linear relationship between inputs and the target, with an expected correlation r of zero. The sample sizes N are varied  
 296 among 50, 100, and 500 to explore the dynamics between sample size and performance estimators. Each configuration  
 297 is repeated across 1000 iterations to assess the distribution of bias and variance.

298 For each iteration, the dataset  $\mathcal{D} = (X, Y)$  was sampled as per the simulation's premise. In the case of K-fold CV, the  
 299 dataset  $\mathcal{D}$  was partitioned into K folds in which each fold is  $\mathcal{D}_k = (X_k, Y_k)$ . For the "In-Sample" approach, partitioning  
 300 does not occur. The linear model f is trained on the training set  $\mathcal{D}_{-k}$  (denoted as  $f_{\mathcal{D}_{-k}}$ ) to estimate regression coefficients  
 301  $\beta$ , which then predicts the target variable  $\hat{Y}_k$  from the test set  $\mathcal{D}_k$ . The procedure of K-fold CV can be expressed as:

$$\begin{aligned} \text{Training: } & Y_k = f_{\mathcal{D}_{-k}}(X_k) + \epsilon \\ & = X_k\beta + \epsilon \\ \text{Testing: } & \hat{Y}_k = f_{\mathcal{D}_{-k}}(X_k) \\ & = X_k\beta \quad k = 1, 2, \dots, K \end{aligned} \tag{15}$$

302 For the "In-Sample" performance estimator, predictions were made without splitting, as:

$$\begin{aligned} \text{Training: } & Y = f_{\mathcal{D}}(X) \\ & = X\beta + \epsilon \\ \text{Testing: } & \hat{Y} = f_{\mathcal{D}}(X) \\ & = X\beta \end{aligned} \tag{16}$$

303 Where:

- 304 •  $X$  denotes the input regressors sampled from a standard normal distribution  $\mathcal{N}(0, 1)$  with dimensions  $N \times 10$ .
- 305 •  $Y$  denotes the target variable sampled from a standard normal distribution  $\mathcal{N}(0, 1)$  with dimensions  $N \times 1$ .
- 306 •  $X_{-k}$  and  $Y_{-k}$  are the input regressors and target variable in the training set  $\mathcal{D}_{-k}$ .
- 307 •  $X_k$  denotes the input regressors in the test set  $\mathcal{D}_k$ .
- 308 •  $\hat{Y}_k$  denotes the predicted target variable in the test set  $\mathcal{D}_k$ .
- 309 •  $\beta$  denotes the estimated regression coefficient with dimensions  $10 \times 1$ .
- 310 •  $\epsilon$  denotes the error term assumed to be normally distributed.

311 Estimated performance  $\mathbb{E}[\hat{g}(f_{\mathcal{D}})]$  was derived by averaging the performance metrics across all K folds as per Eq. 20.  
 312 The bias and variance of the evaluation were calculated using Eqs. 21 and 22, respectively. To approximate true  
 313 model performance  $G(f_{\mathcal{D}})$ , a hundred unseen datasets  $\mathcal{D}^*$  were generated identically to  $\mathcal{D}$ , and the performance  $G(f_{\mathcal{D}})$   
 314 was estimated by averaging the performance metrics across all  $\mathcal{D}^*$ . The detailed steps to compute evaluation bias and  
 315 variance are provided in the supplementary materials.

## 316 2.2 Study 2: Model Selection in Cross-Validation

317 The objective of this simulation study is to examine the effect of improper model selection implementation on validation  
 318 bias. The focus will be on the model selection procedures of feature selection and hyperparameter tuning. The study  
 319 hypothesizes that utilizing the test set inappropriately during any model selection stage will lead to a significant  
 320 overestimation of model performance. This study simulated a regression task using an SVR model, which utilized  
 321 various kernel functions to project a subset of features, X, to predict a target variable, Y. Both X and Y are drawn from  
 322 a normal distribution  $\mathcal{N}(0, 1)$  to establish a baseline null correlation (performance r=0) for assessing validation bias.  
 323 This study set the sample size and number of features at 100 and 1000, respectively. Feature selection is executed by  
 324 choosing the top 50 features that correlate most strongly with Y. For hyperparameter tuning, four kernel functions were  
 325 evaluated: linear, polynomial, radial basis function, and sigmoid.

326 This study introduces notations FS for feature selection and HT for hyperparameter tuning, assigning a binary indicator  
 327 (0 or 1) to denote incorrect (0) or correct (1) implementation of model selection. This yields four possible combinations  
 328 of model selection strategies: “FS=0; HT=0”, “FS=0; HT=1”, “FS=1; HT=0”, “FS=1; HT=1” (Figure 1). When  
 329 FS=0, feature selection precedes cross-validation splitting. If FS=1, feature selection occurs within each fold of the  
 330 training set during cross-validation. With hyperparameter tuning, a correct implementation (HT=1) involves splitting  
 331 the dataset into training (64%), validation (16%), and test (20%) sets. The model is trained and tuned using the training  
 332 and validation sets, respectively, while the test set is reserved for a single evaluation of model performance. Conversely,  
 333 with HT=0, only training (80%) and test (20%) sets are used, risking validation bias as the test set informs both  
 334 training and performance reporting. A 5-fold cross-validation approach was deployed for all strategies. Validation

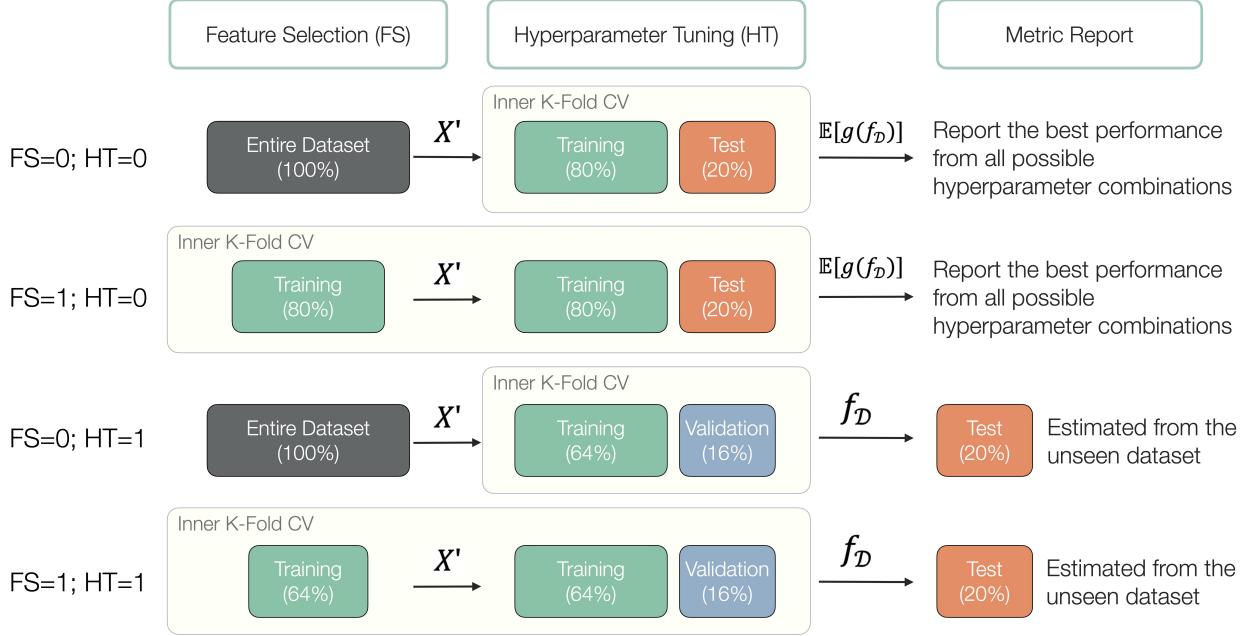


Figure 1: Workflow diagram illustrating four cross-validation strategies of feature selection (FS) and hyperparameter tuning (HT), where 0 denotes incorrect implementation and 1 indicates correct practice.  $X'$  is the selected feature subset,  $\mathbb{E}[\hat{g}(f_D)]$  is the expected generalization performance,  $f_D$  is the model trained on the training set without being revealed to the test set.

335 bias is measured as the discrepancy between the model selection-influenced performance estimate and the expected  
 336 generalization performance ( $r=0$ ), using the Pearson correlation coefficient between predicted and observed values.  
 337 Over 1000 sampling iterations, the study assesses the distribution of validation bias. A t-test will determine whether the  
 338 validation bias significantly deviates from zero.

### 339 2.3 Study 3: Block Effects in Cross-Validation

340 The objective of the study is to demonstrate how a Random CV, which randomly assigns the samples to folds without  
 341 considering the block effects, could overestimate the model performance. This study also conducts a block CV, where  
 342 each block is used as a fold in the cross-validation, as the benchmark. The hypothesis is that the model performance  
 343 estimated by Random CV is significantly higher than the estimation by block CV. This study simulated a regression  
 344 task with 100 instances across ten features, denoted as  $X$ , and one single response variable,  $Y$ . Both  $X$  and  $Y$  are  
 345 derived from a standard normal distribution. To introduce a block factor, the study groups every 20 observations into a  
 346 block, with each block incrementally increasing by  $b$  units from zero, where  $b$  was simulated from 0.5 to 3.0 with an  
 347 increment of 0.5. Within these ten features, one is substituted as the block level, represented by an integer from 0 to 4,  
 348 augmented with random noise drawn from a standard normal distribution. This setup aims to simulate a scenario where  
 349 the predictors primarily capture block variation, given the null expectation in predictability when using ten random  
 350 variables  $X$  to forecast another random variable  $Y$ . The study investigates two model evaluation strategies: Block CV  
 351 and Random CV, both utilizing a 5-fold cross-validation method. In block CV, each block serves as a separate fold,

352 while in Random CV, samples are randomly allocated to each fold (Figure 2). The predictive model is linear regression,  
 353 and the performance is evaluated using Pearson's correlation coefficient. This simulation runs for 1000 iterations, with  
 354 X and Y being resampled in each cycle. A one-tailed t-test assesses if the mean estimated performance significantly  
 355 exceeds zero. Additionally, an Analysis of Variance (ANOVA) table is calculated when b is 0.5 to ascertain if the  
 356 simulated block variation notably exceeds the assumed individual variation, representing the primary interest.

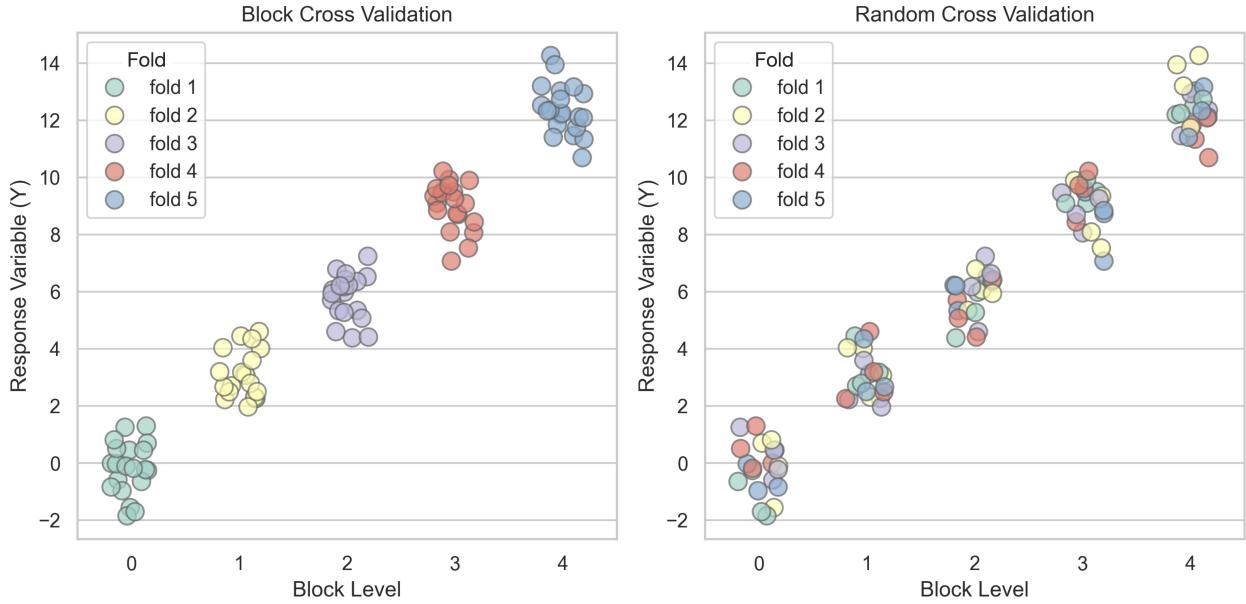


Figure 2: Illustration of fold assignment in block cross validation (left) and random cross validation (right). Folds are color-coded, and the block effect is set to 3 in this example.

#### 357 2.4 Study 4: Performance Metrics in Regression Tasks

358 This study explores two error-based metrics, Root Mean Squared Error (RMSE) and Root Mean Squared Percentage  
 359 Error (RMSPE), and three linearity-based metrics, Pearson Correlation Coefficient ( $r$ ), the Coefficient of Determination  
 360 ( $R^2$ ), and the Concordance Correlation Coefficient ( $CCC$ ), in a variety of commonly-encountered data challenges.  
 361 These data challenges are depicted through 4 scenarios, representing data commonly encountered in predictive  
 362 applications varying in scope (scenarios "Baseline" and "Scaled"), data with outliers disrupting the scale of prediction  
 363 (scenario "Outlier Focused"), and data with an underlying grouping structure (scenario "Clustered"). The statistical  
 364 description of the approach to generating each of these scenarios is included below. Practical examples of real-world  
 365 instances of these types of data challenges are also described.

366 In the hypothetical example depicted in Figure 7, 100 observations were generated from two separate normal  
 367 distributions. The first 50 observations were drawn from a normal distribution with a mean of -3 and a standard  
 368 deviation of 1, denoted as  $\mathcal{N}(-3, 1)$ . The remaining 50 observations were generated from another normal distribution,  
 369  $\mathcal{N}(3, 1)$ . Utilizing two distinct distributions served to simulate experimental block effects, preset at a magnitude of 6

370 units for this experiment. Based on the simulated observations, four scenarios of predictions were derived according to  
371 the setting below:

- 372     • Scenario "Baseline": To establish a correlation relationship, the observations were added another random  
373       variable sampled from  $\mathcal{N}(0, 1)$  to introduce prediction errors. This scenario represents a “best case” for  
374       developing predictive analytics, and could be exemplary of scenarios like predicting a scaled performance  
375       response (i.e., milk yield, average daily gain) from measurable input variables like dry matter intake, sensor  
376       system data, or past performance data.
- 377     • Scenario "Scaled": The prediction outcome from Scenario "Baseline" was multiplied by 5, simulating  
378       predictions with a larger variance while maintaining the same relative order as the original predictions. There  
379       are some responses that have naturally greater proportional variation compared with others. For example,  
380       an animal’s body core temperature is unlikely to vary by more than 5%; however, daily variation around  
381       measurements like feed intake can range upwards of 30 to 40%. Comparison of scenarios "Baseline" and  
382       "Scaled" explore how this natural variation should be included in interpreting predictive analytics.
- 383     • Scenario "Outlier Focused": only the top 10% of predictions that deviate the most from zero in Scenario  
384       "Baseline" were raised to the power of 3. The rest of the predictions were set to zero. This scenario simulates  
385       a prediction that focuses solely on the extreme samples. In disciplines like nutritional exploration, the  
386       emphasis of predictive analytics typically focuses on understanding the mean animal or the mean response  
387       of an individual animal; however, in predictive analytics focused on health or genetic merit, the emphasis of  
388       prediction is often on the extreme observations. Analytics to understand the extreme observations is always  
389       complicated by the question of whether extremes are due to true outliers or some sort of measurement error.  
390       As precision livestock farming advances, the opportunities for measurement error due to erroneous sensor  
391       measurements increases.
- 392     • Scenario "Clustered": Values sampled from two normal distributions,  $\mathcal{N}(-3, 2)$  and  $\mathcal{N}(3, 2)$ , were added  
393       respectively to the predictions made in Scenario "Baseline" of Block A (cross markers in Figure 7) and  
394       Block B (circle markers in Figure 7). In the animal sciences we often rely on blocks as an experimental  
395       tool to support analytics given challenging experimental design or constrained animal units. Many times,  
396       the difference between blocks dwarfs the differences observed within a block, resulting in a masking of true  
397       effects due to the block influence. This scenario amplified the original block effects, simulating a model that  
398       effectively distinguished between different blocks (e.g., herd or breed) but was less capable of predicting  
399       individual variations within each block. An example of this scenario might be simulating milk production  
400       or body weight across species – the magnitude of the difference between sheep and cattle (for example) far  
401       outweighs the magnitude of the difference of sheep or cattle over time.

402 This quartet of predictions serves to simulate potential challenges and complexities encountered in real-world modeling  
403 scenarios, thereby providing a foundation for evaluating different performance metrics used in regression problems.

**404 2.5 Study 5: Performance Metrics in Classification Tasks**

405 This study presents a hypothetical example to highlight how the choice of different performance metrics can lead to  
406 different interpretations of a model's effectiveness. The example focuses on binary classification, where the outcome is  
407 either positive ( $Y=1$ ) or negative ( $Y=0$ ). Suppose a binary classification model always outputs a probability between 0  
408 and 1, indicating the likelihood that a sample belongs to the positive class. This example assumes that the model has  
409 high confidence in correctly predicting 1 out of 4 positive and 5 out of 6 negative samples. This example intends to  
410 illustrate a scenario where the positive outcome is rare, such as predicting the onset of a calving event in dairy cows  
411 [40, 41]. The example data is shown in Figure 8. In addition to the original labels, this example also examines a  
412 scenario with inverted labels (Figure 8. Upper). Since most classification metrics prioritize positive samples, it is  
413 generally advisable to designate the event of interest as the positive class in binary classification problems. Inverting  
414 the labels illustrates the potential overestimation of model performance when the more common, but less significant,  
415 background event is mistakenly marked as the positive class. It is important to note that inverting the labels in this  
416 example only affects the interpretation of model performance, not the model configuration or parameters. To evaluate  
417 classification performance, one must first establish a confidence threshold to dichotomize the prediction probabilities.  
418 For instance, if a prediction probability exceeds the threshold, the sample is labeled positive. By default, the threshold  
419 is set at 0.5 for its simplicity. For example, in the third data row of the example data: With a prediction probability  
420 of 0.38 that falls below the threshold, the sample is deemed negative, resulting in a false negative classification since  
421 the ground truth is positive. It is worth mentioning that this threshold is adjustable to fine-tune model performance for  
422 particular uses. A confusion matrix (Figure 8. Lower), effectively encapsulates prediction outcomes. The rows in this  
423 2x2 matrix correspond to ground truth, while its columns reflect predictions. Correct predictions populate the diagonal  
424 cells, and errors fill the off-diagonal ones. This matrix serves as the foundation for computing various metrics to assess  
425 model performance, which will be explored in the result sections.

### 426 3 Results and Discussion

#### 427 3.1 Study 1: The Impact of Estimator Choice and Sample Size on Model Evaluation Reliability

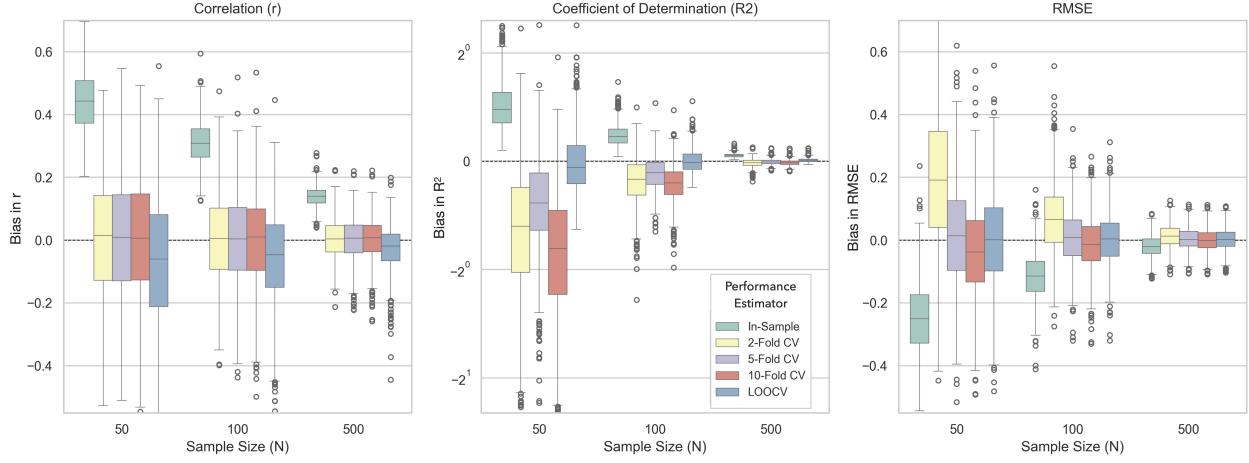


Figure 3: Simulation results of evaluation bias from 1000 sampling iterations. Multiple performance estimators across different sample sizes were color-coded. Three metrics:  $r$ ,  $R^2$ , and RMSE, were displayed in the column facets.

428 The simulation results, depicted in box plots (Figure 3 and 4), explored the evaluation bias and variance distribution.  
 429 Figure 3 examines the bias alterations across various estimators and sample sizes. Independent of the estimator and  
 430 metric, the bias diminishes with increasing sample sizes. The in-sample estimator consistently overestimates across all  
 431 metrics and sample sizes, underscoring the necessity of CV for unbiased performance evaluation. In CV estimators,  
 432 although LOOCV is traditionally viewed as unbiased, it shows underestimation in model performance, especially when  
 433 the metric is correlation coefficient ( $r$ ). Comparatively, 2-, 5-, and 10-fold CV provide a more unbiased estimation  
 434 than LOOCV for all sample sizes. However, for metrics like  $R^2$  or RMSE, LOOCV emerges as the least biased  
 435 estimator. While K-fold CV exhibits higher bias than LOOCV, this difference dwindles when the sample size exceeds  
 436 500. Notably, 10-fold CV, contrary to expectations, demonstrates higher bias than 5-fold CV for small sample sizes (50  
 437 and 100) in the  $R^2$  metric, though this disparity also becomes insignificant at larger sample sizes.  
 438 Considering LOOCV's singular data point testing, its evaluation variance is pertinent only for RMSE, which permits  
 439 single data point evaluations. Figure 4 illustrates the bias and variance in RMSE across different performance estimators  
 440 as a function of sample size N. Both bias and variance in RMSE decrease as sample size increases, aligning with the  
 441 hypothesis. LOOCV provides the least biased estimation, while 2-fold CV exhibits the highest bias without significant  
 442 reduction at larger sample sizes. However, biases across all estimators converge at a sample size of 500. In terms of  
 443 evaluation variance, LOOCV consistently shows higher values than other estimators for all sample sizes. Additionally,  
 444 a lower number of folds K correlates with reduced variance, which is also in line with the hypothesized trend.

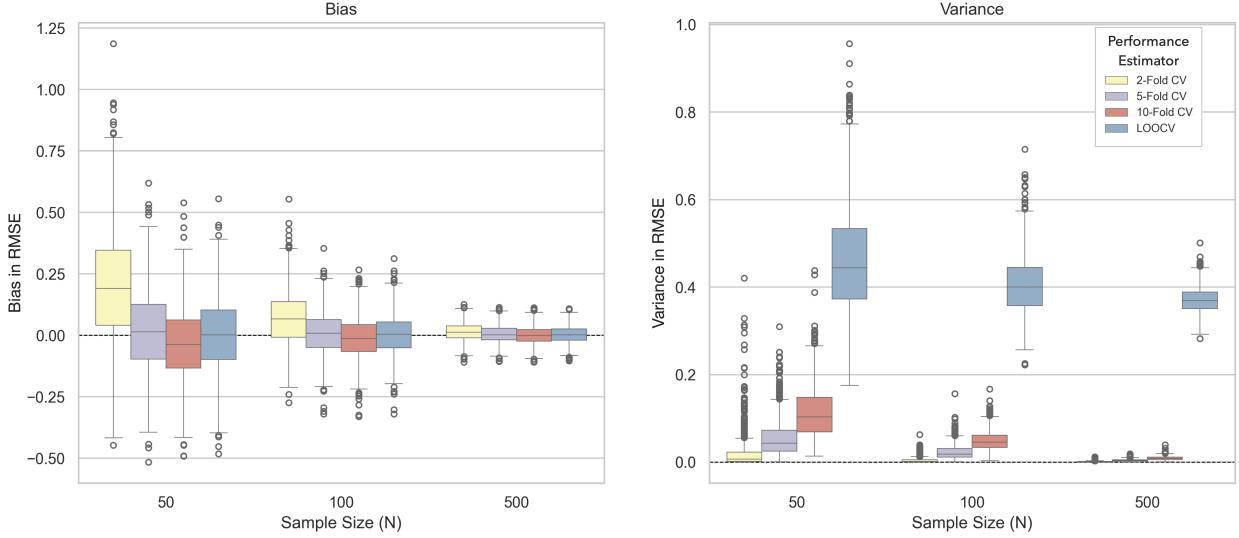


Figure 4: Simulation results of evaluation bias and variance from 1000 sampling iterations. Multiple performance estimators across different sample sizes were color-coded. Only RMSE was displayed. Bias and variance were listed in the left and right facets, respectively.

445 In conclusion, when conducting model evaluation, it is crucial to consider the estimator and sample size, as they  
 446 significantly influence evaluation reliability which can be decomposed into bias and variance. Larger sample sizes  
 447 generally lead to reduced bias and variance, enhancing the reliability of the evaluation process. For unbiased performance  
 448 estimation, CV methods, such as K-fold CV and LOOCV, are preferable to in-sample estimation. LOOCV often  
 449 provides less biased estimations for certain metrics but can exhibit higher variance. It is also noteworthy that the number  
 450 of folds in K-fold CV can affect bias and variance; thus, experimenting with different numbers of folds, especially  
 451 in smaller sample sizes, can be beneficial. Ultimately, the selection of appropriate evaluation techniques should be  
 452 tailored to the specific context of the dataset and the objectives of the modeling exercise, ensuring a robust and reliable  
 453 assessment of model performance.

### 454 3.2 Study 2: Misuse of Model Selection Can Lead to Over-Optimistic Performance Estimates

455 The evaluation bias was visualized using box plots (Figure 5), with the feature selection factor (FS) on the x-axis and  
 456 hyperparameter tuning (HT) distinguished by color — green for incorrect and yellow for correct implementation. The y-  
 457 axis represents the evaluation bias as measured by the correlation coefficient. The results indicate a clear overestimation  
 458 of model performance when feature selection is applied to the entire dataset, regardless of hyperparameter tuning. The  
 459 median biases were 0.797 for “FS=0; HT=0” and 0.761 for “FS=0; HT=1”. Moreover, inappropriate evaluation in  
 460 hyperparameter tuning resulted in a significant bias ( $p$ -value  $< 0.001$ ) with a median of 0.113 for “FS=1; HT=0”. The  
 461 only scenario without bias significantly occurred when both feature selection and hyperparameter tuning were correctly  
 462 incorporated within the cross-validation process “FS=1; HT=1”, yielding a median bias of -0.008. These findings align

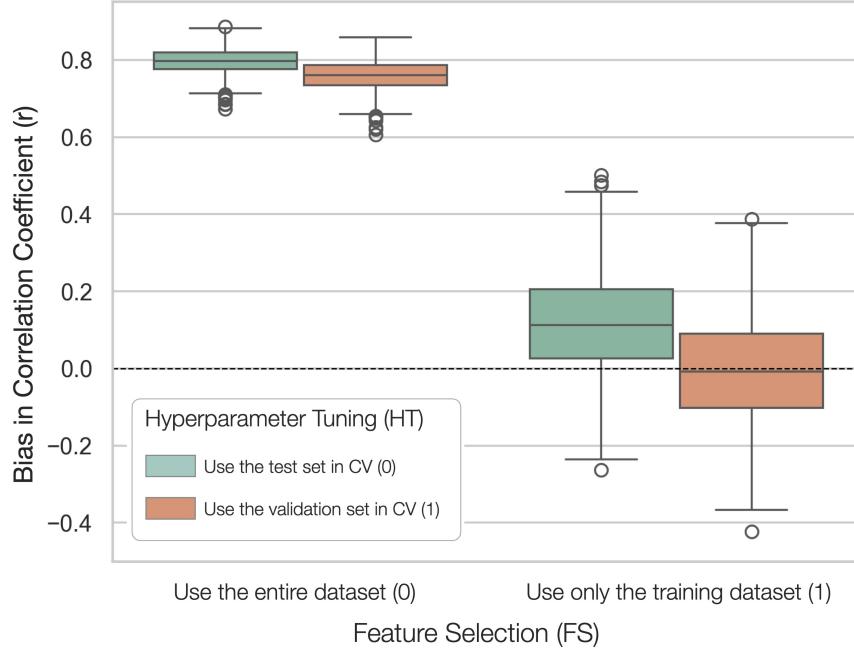


Figure 5: The evaluation bias of the four model selection strategies.

463 with the initial hypothesis and the prevailing literature, reinforcing that model selection must be integrated into the  
 464 cross-validation workflow to prevent an overestimation of model performance.

465 The simulation results robustly confirm the hypothesis that improper implementation of model selection inflates  
 466 performance estimates. Specifically, the evaluation bias is markedly high when feature selection precedes data splitting,  
 467 with or without correct hyperparameter tuning. Although integrating feature selection within cross-validation folds  
 468 mitigates this bias, incorrect hyperparameter tuning still significantly skews performance metrics. Notably, this  
 469 overestimation from the hyperparameter tuning is even more pronounced in complex models, such as neural network  
 470 architectures that often entail over a million parameters. These findings underscore the necessity of meticulous cross-  
 471 validation practices, particularly for feature selection and hyperparameter tuning, to ensure accurate performance  
 472 estimations and generalizability in predictive modeling.

### 473 3.3 Study 3: Overlooking Experimental Block Effects Can Lead to Biased Model Performance Estimates

474 In this simulation, an ANOVA table (Table 3), calculated from a single iteration for illustrative purposes, demonstrates  
 475 that the simulated data exhibits block variation significantly greater than the residual variance. The result (Figure 6)  
 476 shows that regardless of the amplitude of block effects in this simulation study, the Block CV strategy consistently yields  
 477 a mean performance estimate close to zero, while the Random CV strategy consistently and significantly overestimates  
 478 the model performance ( $p\text{-value} < 0.001$ ). This finding supports the hypothesis that Random CV tends to overestimate  
 479 model performance when block variation predominates over residual variation.

Table 3: ANOVA results for a single iteration of the simulated data with  $b = 0.5$ . SS: sum of squares; DF: degree of freedom; MS: mean square; F: F-statistic

Source	SS	DF	MS	F	p-value
Between	60.971	4	15.243	20.580	<0.001
Within	70.363	95	0.741		
Total	131.35	99			

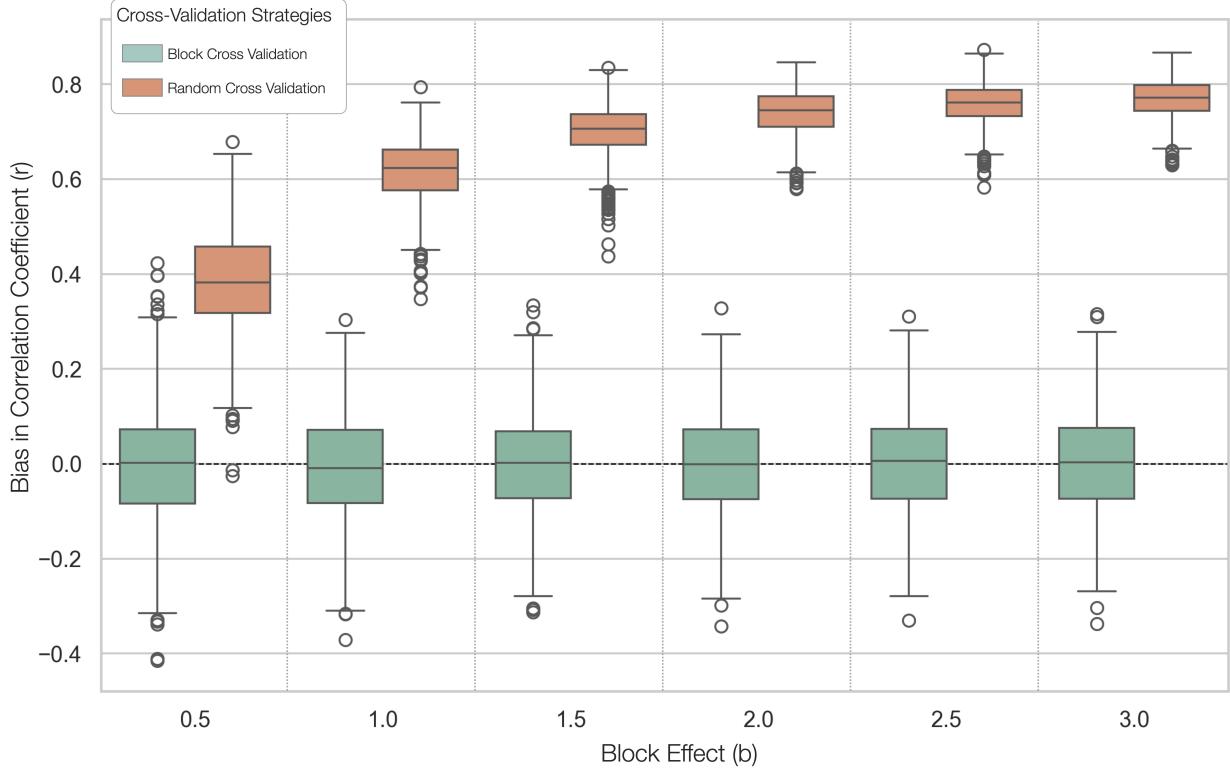


Figure 6: Bias in model performance estimation by Block CV and Random CV across 1000 iterations. The dashed line represents the null hypothesis that the mean performance estimate is zero.

480 In conclusion, block CV proves to be a vital tool in assessing the generalizability and accuracy of a predictive model,  
 481 especially in contexts where block effects, such as herd variations, play a significant role in both the predicting features  
 482 and response variable. The random CV strategy, which randomly assigns samples to folds without considering block  
 483 effects, tends to overestimate model performance. This study recommends that block CV be used as a benchmark in  
 484 model evaluation, especially when block effects are present

#### 485 3.4 Study 4: Different Regression Metrics Illustrate Different Aspects of Model Performance

486 The simulated hypothetical example in Figure 7 illustrates the performance of four different prediction scenarios. The  
 487 error-based metrics, RMSE and RMSPE, are sensitive to the magnitude of the error. In Scenario "Scaled", where the  
 488 errors are five times larger but remain the same in rank order compared to Scenario "Baseline", the RMSE inflates from

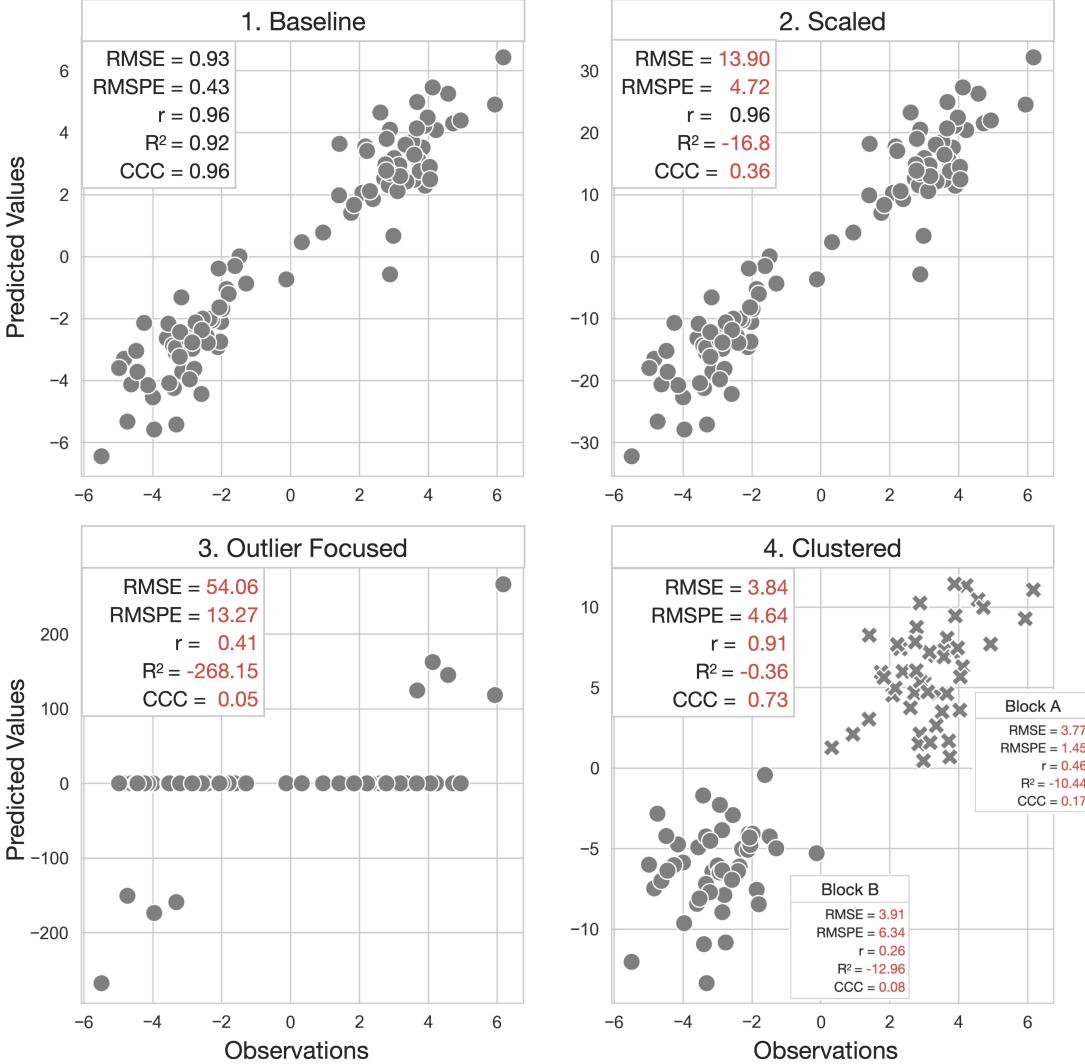


Figure 7: Scatter plots display the same observations against four different prediction scenarios in the given hypothetical example. Scenario "Baseline" serves as a baseline for the metrics, with any metric better than the baseline highlighted in bold and underscored, and any worse metric colored in red.

489 0.93 to 13.90, and RMSPE also increases from 0.43 to 4.72. Another notable characteristic of RMSE and RMSPE is  
 490 that they weigh more on large errors, which is essential when making a large error is costly and should be prioritized  
 491 for avoidance. In Scenario "Outliers", where certain predictions deviate substantially from the majority, the squaring  
 492 operation in Equation 1 accentuates these outliers, culminating in an RMSE of 54.06 and RMSPE of 13.27. However,  
 493 when investigating into the intra-block performance in the scenario "Clustered", the RMSE failed to detect the inflated  
 494 performance due to the strong block effects. It resulted in a similar RMSE of 3.84 from the entire prediction set and  
 495 3.77 and 3.91 within each block. This phenomenon emphasizes again that RMSE is affected solely by the magnitude  
 496 of the error, which neglects the ability of the model to capture relative trends in intra-block or inter-block predictions.  
 497 On the other hand, when the goal is to rank observations of interest rather than predict the absolute magnitude of the  
 498 error, linearity-based metrics can provide more insights. The correlation  $r$  is an example showing its consistency across

499 the Scenario "Baseline" and "Scaled", despite the latter having five times larger errors. This metric is particularly  
500 useful when the relative order of predictions is more important than the absolute error magnitude. However, it is  
501 worth noting that the correlation  $r$  can be misleading in certain scenarios, such as the Scenario "Outlier Focused",  
502 where 90% of the predictions are zero. In this case, the correlation  $r$  show a moderate performance of 0.41, which is  
503 mainly contributed by the 10% of the outlier predictions that are "ranked" correctly but with a large error magnitude.  
504 This example highlights the importance of visually inspecting the regression results through scatter plots to avoid  
505 misleading conclusions. Moreover, one common pitfall of the correlation  $r$  is that block effects can influence it, leading  
506 to an inflated performance estimate if individual variation is of greater interest than inter-block variation. This was  
507 demonstrated in Scenario "Clustered", where the overall coefficient  $r$  was 0.91, but the metric within each block was  
508 only 0.46 and 0.26, respectively. Therefore, it is essential to examine regression results within identifiable blocks.  
509 Besides the correlation  $r$ ,  $R^2$  provides a more comprehensive insight, as it focuses both the linear trend from the variance  
510 composition and the error magnitude from the residual sum of squares. From the "Scaled" scenario,  $R^2$  successfully  
511 detected the inflated error magnitude, resulting in a negative value of -16.8. It also captured the outlier-induced variance  
512 in the "Outliers" scenario, with a negative value of -268.15. Lastly, in Scenario "Clustered", the value of  $R^2$  indicated a  
513 weak performance by the model with a score of -0.36. This score is statistically reasonable, as the predictions have  
514 larger variance than the observations. The metric also successfully detected the model failure in capturing intra-block  
515 variation, as the  $R^2$  values within each block were -10.44 and -12.96, respectively. However, an obvious limitation  
516 of  $R^2$  is that it has no standard scale. Considering this, a more nuanced evaluation metric,  $CCC$ , showcases a more  
517 balanced performance evaluation. It always range from -1 to 1, and it successfully captures all the characteristics of the  
518 four scenarios. In Scenario "Scaled", the  $CCC$  value dropped from 0.96 to 0.36. In Scenario "Outliers", the  $CCC$   
519 value plummeted to 0.05, showcasing the model's failure to "align" the predictions with the observations with 90% of  
520 the predictions being zero. In the Scenario "Clustered", although the  $CCC$  value was 0.73, the metric also showed  
521 the model's weakness in each block, with the  $CCC$  values of 0.17 and 0.08, respectively. This study demonstrates  
522 that  $CCC$  is a more balanced metric that considers both the linear trend and the error magnitude, making it a more  
523 comprehensive evaluation metric for regression models.

### 524 3.5 Study 5: Label-Invariant Metrics Provide Balanced Assessment in Binary Classification

525 Different metrics in binary classification were evaluated in a simulated example (Figure 8). The original labels were  
526 inverted to examine the robustness of the metrics against label choices. The accuracy metric, with a 0.5 threshold in  
527 this example, stands at 0.60. This figure might suggest modest efficacy, marginally surpassing random chance, with an  
528 accuracy of 0.50. Nonetheless, the same accuracy level could be achieved by classifying every sample as negative in an  
529 imbalanced dataset where negatives are predominant. In contrast, precision and recall provide a more nuanced evaluation  
530 of model performance by separately assessing the correctness of positive predictions and the ability to detect actual  
531 positives. With a threshold of 0.5, the example dataset yields precision and recall values of 0.5 and 0.25, respectively.  
532 These metrics deliver more interpretable information that only half of the positive predictions are correct, and just a

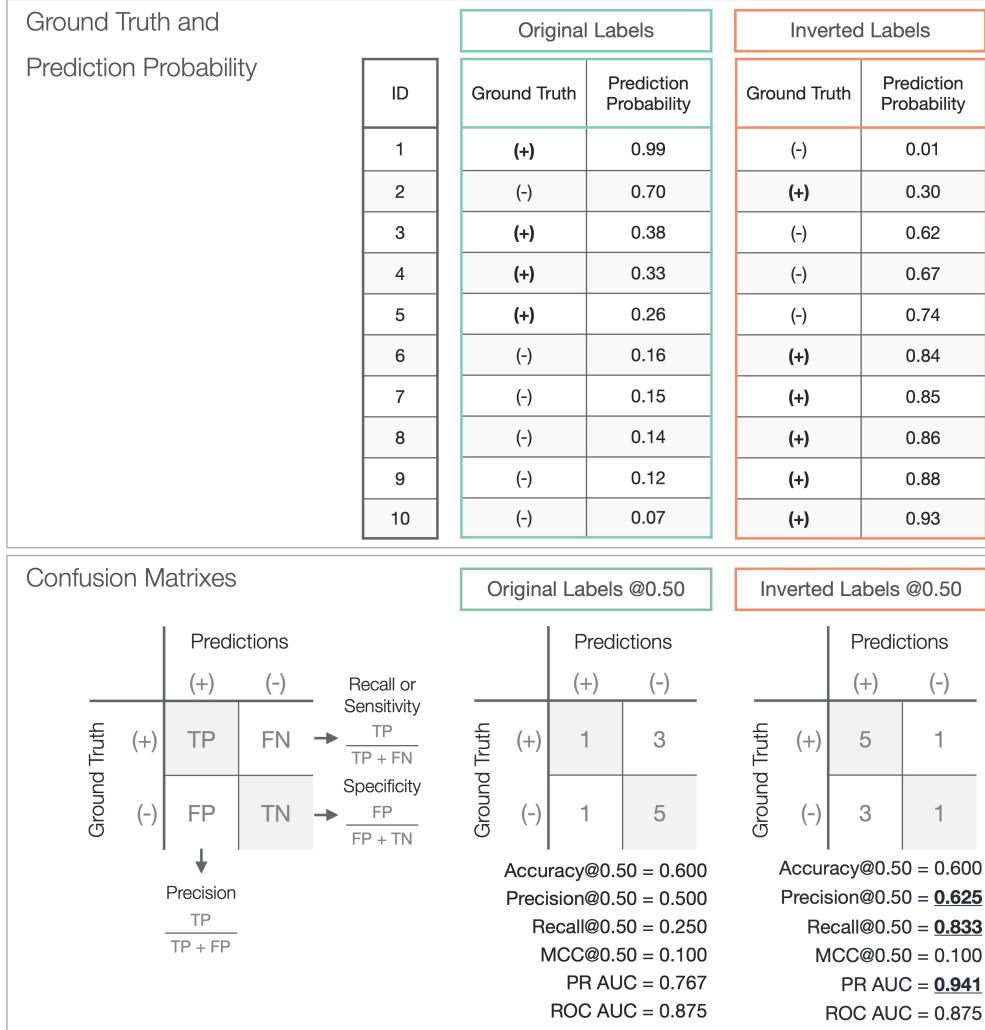


Figure 8: Simulated hypothetical example of binary classification. TP: true positive; FN: false negative; FP: false positive; TN: true negative; **Upper:** The ground truth and prediction probability. **Lower:** The confusion matrix of the prediction at a threshold of 0.5, followed by classification metrics of accuracy, precision, recall, MCC, PR curve AUC, and ROC curve AUC. The performance of the original labels serves as a baseline for comparison. Any better performance metrics from the inverted labels are highlighted in bold and underscored

533 quarter of the actual positives are detected. This contrasts with an accuracy of 0.6, which may appear misleadingly high  
 534 due to the abundance of negative samples. Additionally, it is noted that the chosen confidence threshold significantly  
 535 impacts precision and recall. While the trade-off between these two metrics is not always linear, it is generally observed  
 536 that a higher threshold increases precision but decreases recall, and vice versa. A high threshold indicates a conservative  
 537 approach in predicting positives, reducing false positives, and thus enhancing precision. However, this often leads to  
 538 missing actual positive cases, lowering recall. Hence, the Precision-Recall (PR) curve is an essential tool for evaluating  
 539 model performance across various thresholds. Plotted with recall on the x-axis and precision on the y-axis, this curve is  
 540 derived by computing these metrics at different thresholds (Figure 9, Left). The Area Under the Curve (AUC) provides  
 541 a summary measure of the PR curve's overall performance. A model's effectiveness is generally indicated by how close

542 a point on the PR curve is to the top-right corner. For example, at a threshold of 0.25, which is positioned near the  
 543 top-right of the PR curve, the model demonstrates impressive performance with an accuracy of 0.90, precision of 0.80,  
 544 and recall at 1.00.

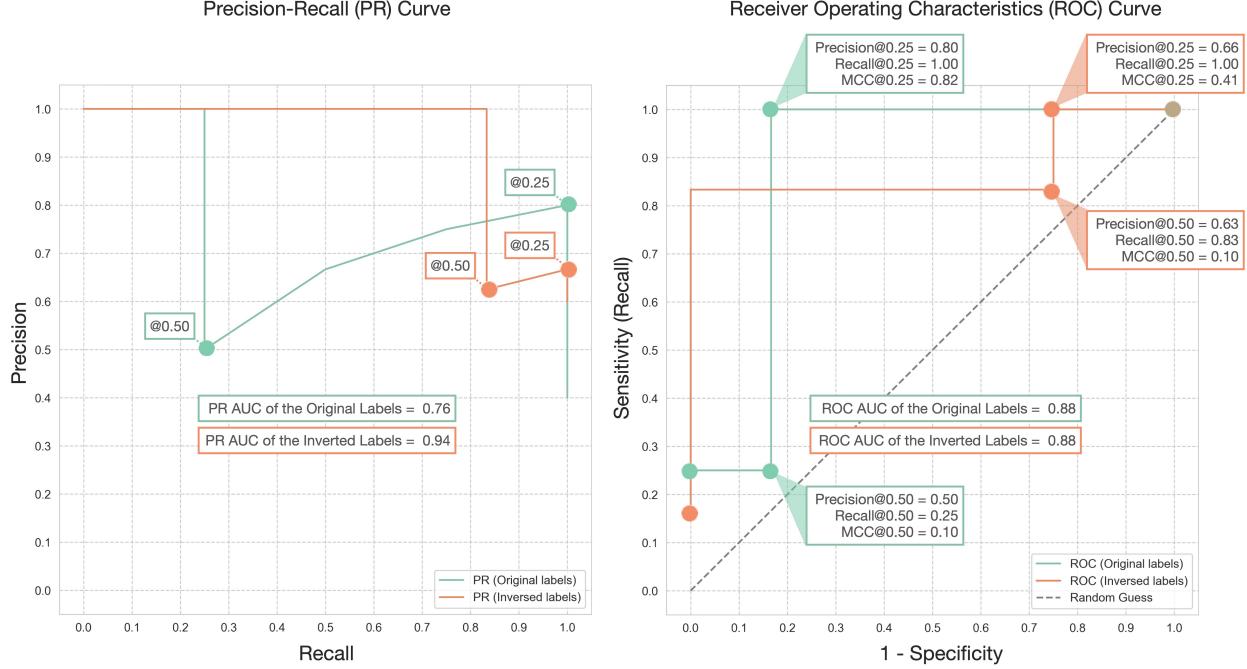


Figure 9: (**Left**) Precision-recall (PR) curve and (**Right**) Receiver operating characteristic (ROC) curve for the hypothetical example are displayed. The performance at confidence thresholds of 0.25 and 0.50 is highlighted. Original labels are marked in green, while inverted labels appear in orange. The Area Under the Curve (AUC) is depicted at the center of each curve.

545 However, it is worth re-emphasizing that precision and recall focus predominantly on positive samples. Inappropriately  
 546 assigning a predominant background event as the positive class can lead to skewed interpretations. This pitfall is  
 547 demonstrated in this example by inverting the labels. At a threshold of 0.50, precision increases from 0.50 to 0.63, and  
 548 recall jumps from 0.25 to 0.83. With the threshold set at 0.25, precision drops to 0.66 from 0.80, while recall remains  
 549 unchanged. The PR AUC also rises from 0.76 to 0.94. Such shifts in metrics, driven merely by label rearrangement  
 550 unrelated to the data or model characteristics, underscore the importance of label-invariant metrics that remain unaffected  
 551 by label assignments. Unlike metrics focusing solely on positive samples, the ROC curve accounts for both positive  
 552 and negative samples, making it a label-invariant metric. Specificity is plotted on the x-axis and sensitivity on the  
 553 y-axis, calculated at different thresholds (Figure 9, Right). In this hypothetical example, the ROC curve demonstrates  
 554 robustness and label-invariance with a consistent AUC of 0.875, regardless of whether the original or inverted labels are  
 555 used. Lastly, another label-invariant metric is MCC which provides a balanced assessment of both positive and negative  
 556 samples. Considering MCC's balanced approach to evaluating model performance, this study introduces the concept  
 557 of an MCC curve. This curve, which plots the MCC value against various threshold levels (Figure 10), serves as a  
 558 powerful tool for identifying the optimal confidence thresholds for model predictions. By examining this curve, one

559 can determine the specific threshold at which the MCC value peaks, thereby optimizing the model's performance. For  
 560 example, when applied to the hypothetical example, the optimum MCC value of 0.82 was attained at a threshold of 0.25.  
 561 This particular threshold corresponded to accuracy, precision, and recall values of 0.90, 0.75, and 1.00, respectively.  
 562 Notably, the MCC curve retains its symmetry even when labels are reversed, affirming its status as a label-invariant  
 563 measure. In scenarios with inverted labels, the maximum MCC value observed was 0.83, achieved at a threshold of  
 564 0.75, leading to accuracy, precision, and recall values of 0.90, 1.00, and 0.83, respectively. Such findings underscore the  
 565 MCC's ability to provide a balanced and comprehensive assessment of both positive and negative samples, thereby  
 566 reinforcing its utility as a versatile and effective metric for thorough model evaluation.

567 In conclusion, binary classification models are often evaluated using metrics focusing on positive samples, such as  
 568 precision and recall. It is generally advisable to designate the event of interest as the positive class. Otherwise, these  
 569 metrics can be misleading when the more common but less significant background event is mistakenly marked as the  
 570 positive class. To circumvent this potential bias, adopting label-invariant metrics is recommended. These metrics offer  
 571 a more balanced and reliable assessment of model performance. Notable examples of such metrics include the ROC  
 572 curve and the proposed MCC curve by this review, both of which are unaffected by the choice of positive and negative  
 class labels and are thus robust for a thorough model evaluation.

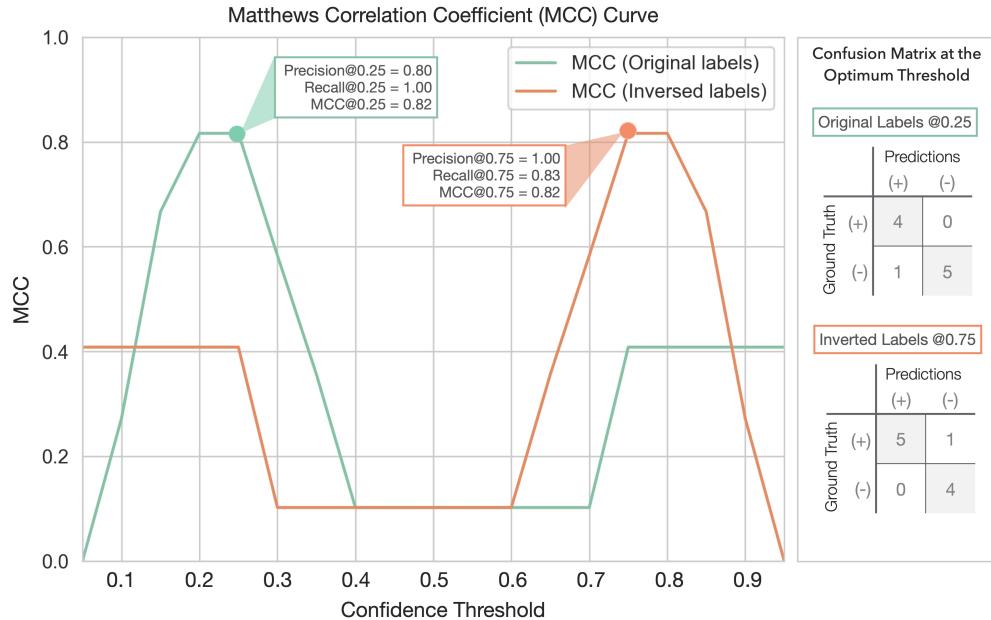


Figure 10: Matthews Correlation Coefficient (MCC) curve. A line chart plotting MCC at different thresholds for the hypothetical example. The optimal threshold is highlighted by the dot marks in green and orange for the original and inverted labels, respectively. The confusion matrix at the optimal threshold is displayed in the right panel.

**574 4 Conclusion**

575 In summary, the review highlights several key considerations for performance assessment in predictive modeling.  
576 When evaluating regression models, the choice of metrics like Correlation Coefficient  $r$ , RMSE, and  $R^2$  depends on  
577 the specific goals of the model. A comprehensive evaluation should include multiple metrics to understand different  
578 aspects of model performance. In binary classification models, precision and recall are crucial, but it is essential to  
579 correctly designate the positive class to avoid bias. Label-invariant metrics, such as the ROC curve and the proposed  
580 MCC curve, provide a balanced assessment, unaffected by class label choices. Additionally, the reliability of model  
581 evaluation is significantly influenced by estimator choice and sample size. Larger sample sizes tend to reduce bias and  
582 variance, increasing evaluation reliability. Cross-validation methods, such as K-fold CV and LOOCV, are preferable  
583 for unbiased performance estimation, with the number of folds in K-fold CV being particularly influential in smaller  
584 datasets. Moreover, the review underscores the importance of correct implementation in model selection processes,  
585 as improper techniques can inflate performance estimates. This is especially true in complex models where feature  
586 selection and hyperparameter tuning need meticulous cross-validation to avoid overestimation of performance. Finally,  
587 the utility of Block CV is emphasized in contexts where block effects are significant. It provides a more realistic  
588 assessment of model generalizability and accuracy compared to a Random CV, which tends to overestimate performance  
589 in such scenarios. Overall, the review recommends a thoughtful selection of metrics and evaluation techniques, tailored  
590 to the specific dataset and modeling objectives, to ensure accurate and reliable performance assessments in predictive  
591 modeling.

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705 **Appendix**

706 **Cross Validation**

707 Model cross validation aims to evaluate how well a given model generalizes to an independent dataset that it has not  
 708 seen during the training process. The most common method is K-fold cross-validation (**K-fold CV**). To implement the  
 709 K-fold CV, the available dataset, denoted as  $\mathcal{D}$ , is partitioned into K equally sized folds. We can express the dataset as  
 710 below:

$$\begin{aligned}\mathcal{D} &= \{(X, Y)\} \\ &= \{(X_1, Y_1), (X_2, Y_2), \dots, (X_K, Y_K)\}\end{aligned}\tag{17}$$

711 where  $X \in \mathbb{R}^{n \times p}$  represents the input features, and  $Y \in \mathbb{R}^{n \times 1}$  symbolizes the ground truth labels for a single target  
 712 variable. The value of n corresponds to the total number of samples, while p represents the number of features. In  
 713 each iteration of the K-fold CV, a single fold is reserved as the test set,  $\mathcal{D}_{\text{test}}$  (or  $\mathcal{D}_k$ ), to act as unseen data, while the  
 714 remaining folds make up the training set  $\mathcal{D}_{\text{train}}$  (or  $\mathcal{D}_{-k}$ ):

$$\begin{aligned}\mathcal{D}_{\text{train}} &= \mathcal{D}_{-k} \\ &= \{(X_1, Y_1), (X_2, Y_2), \dots, (X_{k-1}, Y_{k-1}), (X_{k+1}, Y_{k+1}), \dots, (X_K, Y_K)\} \\ \mathcal{D}_{\text{test}} &= \mathcal{D}_k \\ &= \{(X_k, Y_k)\}\end{aligned}\tag{18}$$

715 After splitting the dataset into  $\mathcal{D}_{-k}$  and  $\mathcal{D}_k$ , the examined model  $f$  is trained on the training set  $\mathcal{D}_{-k}$  and denoted as  $f_{\mathcal{D}_{-k}}$ .  
 716 The hold-out test set  $\mathcal{D}_k$  is then used to evaluate the model performance  $\hat{g}(f_{\mathcal{D}_{-k}})$ , which is defined by comparing the  
 717 predicted labels  $\hat{Y}_k = f_{\mathcal{D}_{-k}}(X_k)$  with the true labels  $Y_k$  using a performance metric  $\mathcal{L}$  (e.g., RMSE or  $R^2$ ):

$$\begin{aligned}\hat{g}(f_{\mathcal{D}_{-k}}) &= \mathcal{L}(Y_k, \hat{Y}_k) \\ &= \mathcal{L}(Y_k, f_{\mathcal{D}_{-k}}(X_k))\end{aligned}\tag{19}$$

718 To estimate the generalization performance of a model  $\mathbb{E}[\hat{g}(f_{\mathcal{D}})]$ , the K-fold CV procedure is repeated K times until  
 719 each fold has been used as the test set  $\mathcal{D}_k$  once. The entire dataset  $\mathcal{D}$  is leveraged to calculate the average prediction  
 720 performance over all K folds. The model's generalization performance can be expressed as:

$$\begin{aligned}\mathbb{E}[\hat{g}(f_{\mathcal{D}})] &= \mathbb{E}[\hat{g}(f_{\mathcal{D}_k})] \\ &= \frac{1}{K} \sum_{k=1}^K \hat{g}(f_{\mathcal{D}_k})\end{aligned}\tag{20}$$

721 It is noted that  $\mathbb{E}[\hat{g}(f_{\mathcal{D}})]$  is equivalent to  $\mathbb{E}[\hat{g}(f_{\mathcal{D}_k})]$  in K-fold CV. It is because the  $\mathbb{E}[\hat{g}(f_{\mathcal{D}})]$  is estimated by averaging  
 722 all  $\hat{g}(f_{\mathcal{D}_k})$  over K folds, which is also the definition of  $\mathbb{E}[\hat{g}(f_{\mathcal{D}_k})]$ .

### 723 Cross Validation Bias and Variance

724 The true generalization performance of the model  $G(f_{\mathcal{D}})$  can only be approximated by averaging the performance  
 725 metrics over infinite unseen datasets. However, in practice, the dataset  $\mathcal{D}$  is finite and therefore, there is always a bias  
 726 when using a finite dataset to estimate  $G(f_{\mathcal{D}})$ . The bias is known as validation bias:

$$\text{Bias} = \mathbb{E}[\hat{g}(f_{\mathcal{D}})] - G(f_{\mathcal{D}})\tag{21}$$

727 For example, if RMSE is used as the performance metric, a positive validation bias suggests that the model validation  
 728 procedure concludes a pessimistic estimation of the model performance, since the true performance is expected to be  
 729 lower than the estimated performance. Another aspect of model validation is the variance of the estimated performance.  
 730 For example, in a 5-fold cross-validation, there are five estimates of the model performance. The variance among these  
 731 five estimates is known as validation variance. A high validation variance suggests that the performance is sensitive to  
 732 the choice of the test set  $\mathcal{D}_k$ , which may be caused by a small sample size or an over-complex model. The validation  
 733 variance can be defined as:

$$\begin{aligned}\text{Variance} &= \mathbb{E}[(\hat{g}(f_{\mathcal{D}_k}) - \mathbb{E}[\hat{g}(f_{\mathcal{D}})])^2] \\ &= \mathbb{E}[\hat{g}^2(f_{\mathcal{D}_k}) - 2\hat{g}(f_{\mathcal{D}_k})\mathbb{E}[\hat{g}(f_{\mathcal{D}})] + \mathbb{E}^2[\hat{g}(f_{\mathcal{D}})]] \\ &= \mathbb{E}[\hat{g}^2(f_{\mathcal{D}_k})] - 2\mathbb{E}[\hat{g}(f_{\mathcal{D}_k})]\mathbb{E}[\hat{g}(f_{\mathcal{D}})] + \mathbb{E}^2[\hat{g}(f_{\mathcal{D}})] \\ &= \mathbb{E}[\hat{g}^2(f_{\mathcal{D}_k})] - \mathbb{E}^2[\hat{g}(f_{\mathcal{D}})]\end{aligned}\tag{22}$$

734 Combining the Equations 21 and 22, the mean squared error (MSE) of the model validation can be decomposed as:

$$\begin{aligned}
\text{MSE} &= \mathbb{E}[(\hat{g}(f_{D_k}) - G(f_D))^2] \\
&= \mathbb{E}[\hat{g}^2(f_{D_k})] - 2\mathbb{E}[\hat{g}(f_{D_k})]G(f_D) + G^2(f_D) + \\
&\quad \mathbb{E}^2[\hat{g}(f_{D_k})] - \mathbb{E}^2[\hat{g}(f_{D_k})] \\
&= (\mathbb{E}^2[\hat{g}(f_{D_k})] - 2\mathbb{E}[\hat{g}(f_{D_k})]G(f_D) + G^2(f_D)) + \\
&\quad (\mathbb{E}[\hat{g}^2(f_{D_k})] - \mathbb{E}^2[\hat{g}(f_{D_k})]) \\
&= (\mathbb{E}[\hat{g}(f_{D_k})] - G(f_D))^2 + (\mathbb{E}[\hat{g}^2(f_{D_k})] - \mathbb{E}^2[\hat{g}(f_{D_k})]) \\
&= (\mathbb{E}[\hat{g}(f_D)] - G(f_D))^2 + (\mathbb{E}[\hat{g}^2(f_D)] - \mathbb{E}^2[\hat{g}(f_D)]) \\
&= \text{Bias}^2 + \text{Variance}
\end{aligned} \tag{23}$$

735 **Hyperparameter**

736 Here are the loss functions for ordinary least squares (OLS), ridge regression, and LASSO regression, respectively:

$$\mathcal{L}_{\text{OLS}}(\beta) = \sum_{i=1}^n (y_i - x_i \beta)^2 \tag{24}$$

$$\mathcal{L}_{\text{ridge}}(\beta) = \sum_{i=1}^n (y_i - x_i \beta)^2 + \lambda \sum_{j=1}^p \beta_j^2 \tag{25}$$

$$\mathcal{L}_{\text{LASSO}}(\beta) = \sum_{i=1}^n (y_i - x_i \beta)^2 + \lambda \sum_{j=1}^p |\beta_j| \tag{26}$$

737 Where  $x_i$  and  $y_i$  represent the  $i$ th row of the design matrix  $X$  and the response vector  $Y$ , respectively. The term  $n$   
 738 denotes the sample size, and  $\beta$  is the coefficient vector. All three models aim to find the optimal  $\beta$  that minimizes their  
 739 respective loss function,  $\mathcal{L}$ . In the regularized models (i.e., ridge and LASSO regression), the vector length of  $\beta$  is  
 740 penalized in the loss function.

741 **Squared Correlation Coefficient  $r^2$  and Determination Coefficient  $R^2$** 

742 The squared Pearson correlation coefficient,  $r^2$ , is not necessarily equivalent to the coefficient of determination,  $R^2$ .  
 743 This equivalence holds true specifically in the context of least squares regression when the same model and data are  
 744 used for both fitting and evaluation. However, this may not be the case when the model is assessed using new data.  
 745 To demonstrate the equivalence between  $r^2$  and  $R^2$  under these specific conditions, we begin by assuming that the  
 746 covariance between the predicted values  $\hat{Y}$  and the residuals  $\epsilon$  is zero:

$$\begin{aligned}
\text{cov}(Y, \hat{Y}) &= \text{cov}(\hat{Y} + \epsilon, \hat{Y}) \\
&= \text{cov}(\hat{Y}, \hat{Y}) + \text{cov}(\hat{Y}, \epsilon) \\
&= \text{var}(\hat{Y}) + \text{cov}(\hat{Y}, \epsilon) \\
&= \text{var}(\hat{Y})
\end{aligned} \tag{27}$$

747 With the assumption that  $\bar{\hat{Y}} = \bar{Y}$ , which typically holds when  $\epsilon \sim N(0, \sigma^2)$ , the squared correlation coefficient  $r^2$  is  
748 expressed as follows:

$$\begin{aligned}
r^2 &= \frac{\text{cov}^2(Y, \hat{Y})}{\text{var}(Y)\text{var}(\hat{Y})} \\
&= \frac{\text{var}(\hat{Y})^2}{\text{var}(Y)\text{var}(\hat{Y})} \\
&= \frac{\text{var}(\hat{Y})}{\text{var}(Y)} \\
&= \frac{\sum_{i=1}^n (\hat{Y}_i - \bar{\hat{Y}})^2}{\sum_{i=1}^n (Y_i - \bar{Y})^2} \\
&= \frac{\sum_{i=1}^n (\hat{Y}_i - \bar{\hat{Y}})^2}{\sum_{i=1}^n (Y_i - \bar{Y})^2} \\
&= \frac{SS_{\text{residual}}}{SS_{\text{total}}} \\
&= R^2
\end{aligned} \tag{28}$$

749 where  $SS_{\text{residual}}$  is the residual sum of squares and  $SS_{\text{total}}$  is the total sum of squares. Each  $Y_i$  and  $\hat{Y}_i$  are the  $i$ th elements  
750 of the actual response vector  $Y$  and the predicted response vector  $\hat{Y}$ , while  $\bar{Y}$  and  $\bar{\hat{Y}}$  are their respective means. This  
751 proof highlights that under certain assumptions,  $r^2$  and  $R^2$  can indeed be equivalent, but such conditions are specific  
752 to least squares regression where errors are normally distributed and predictions are unbiased estimates of the actual  
753 values.



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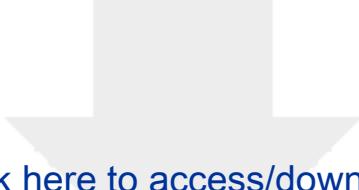
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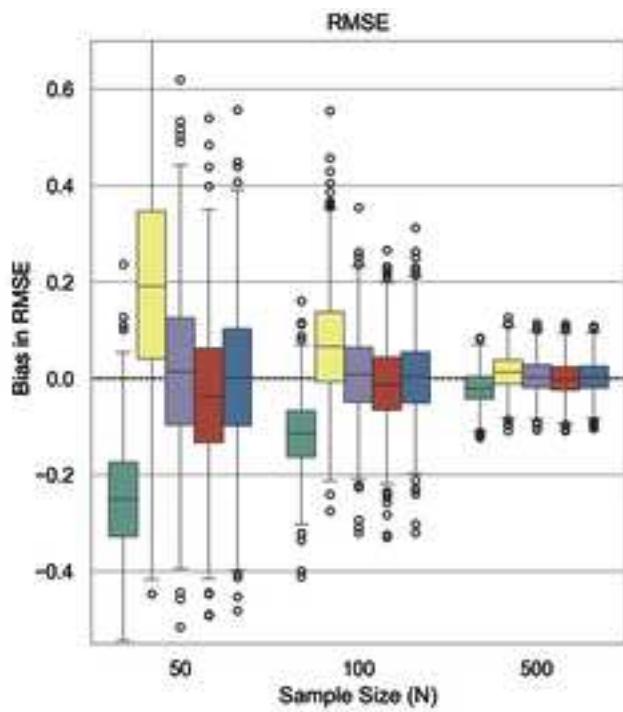
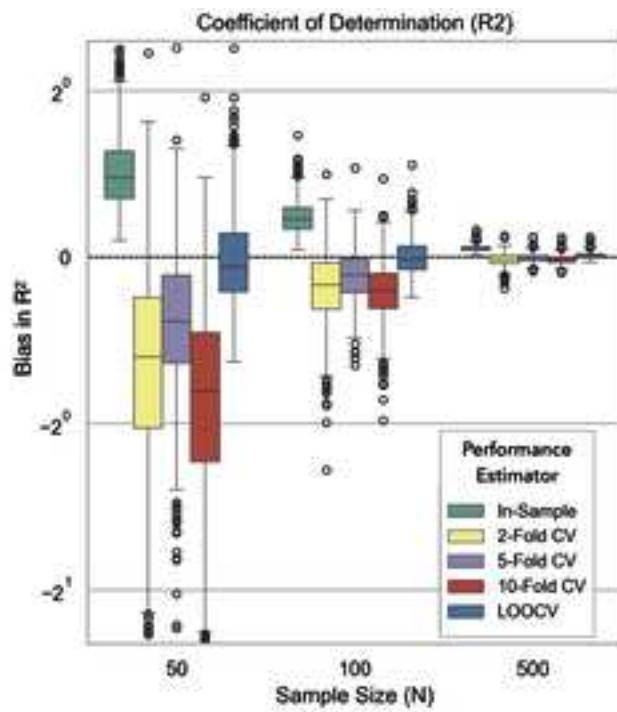
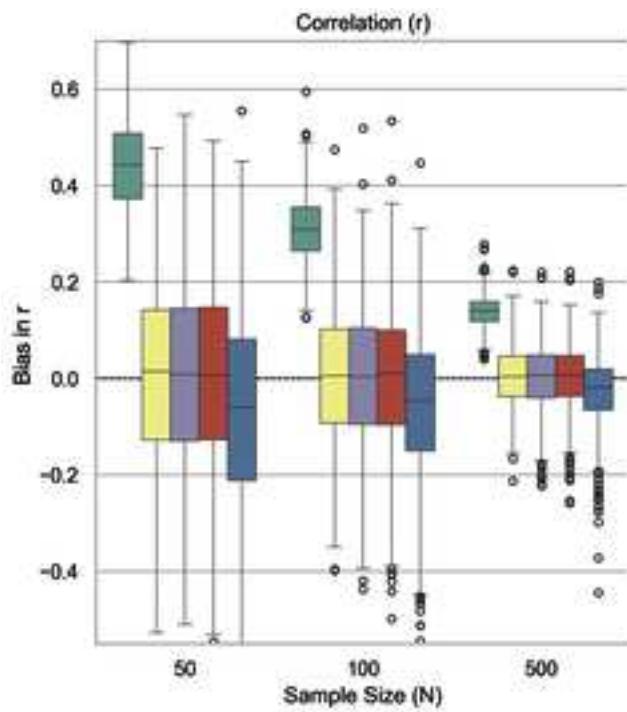
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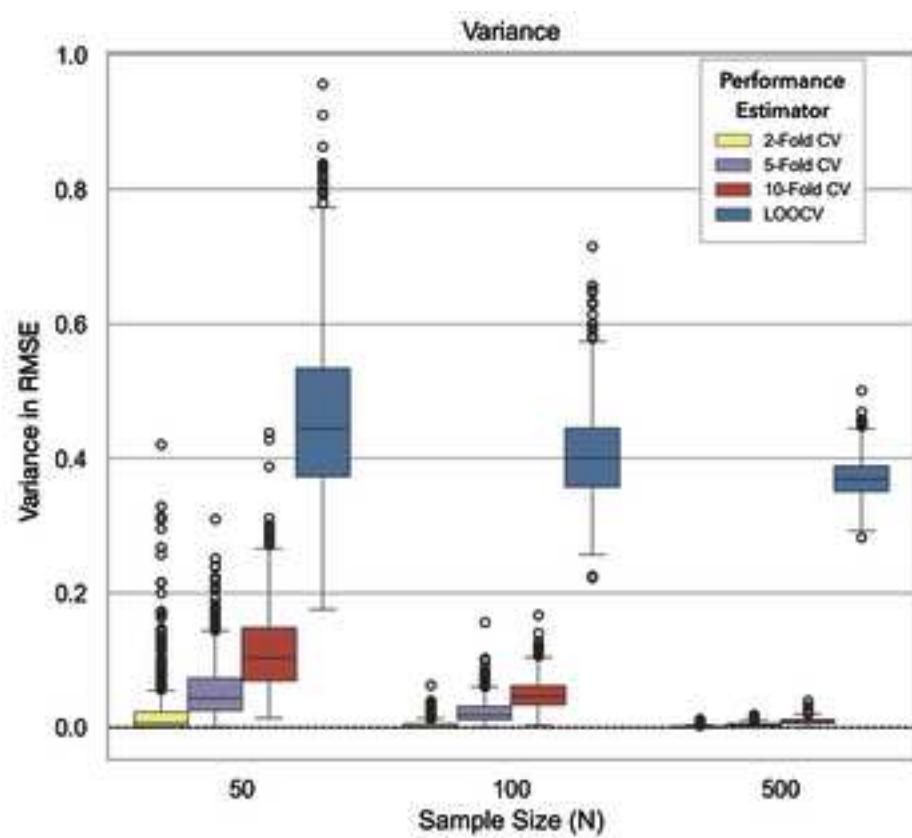
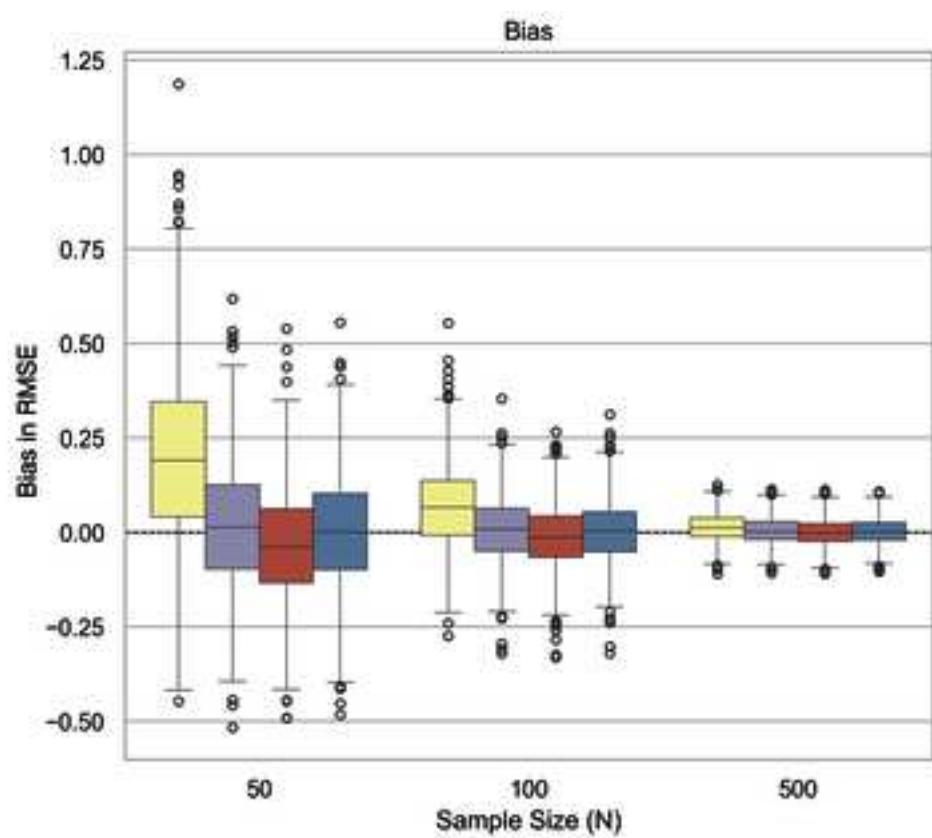


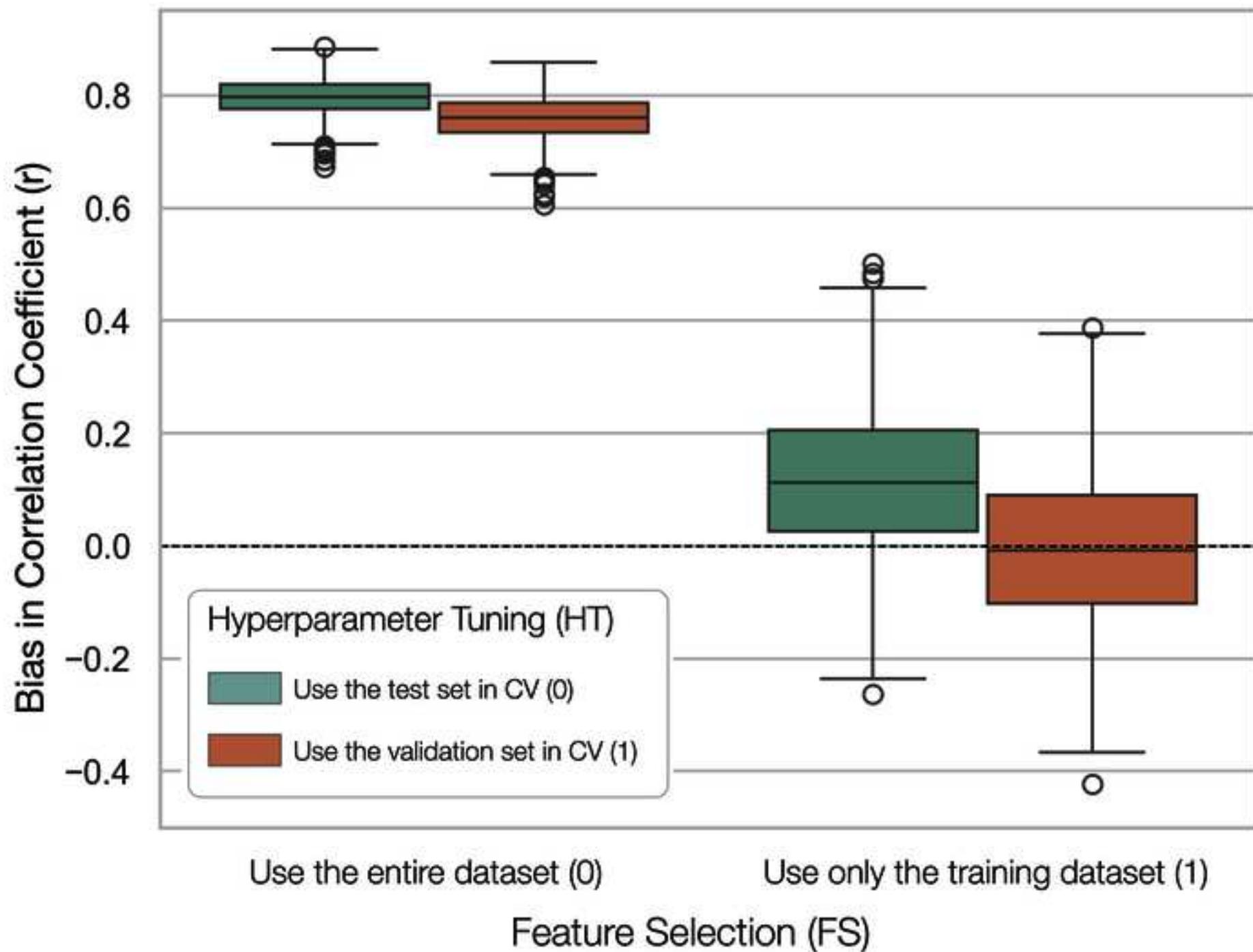
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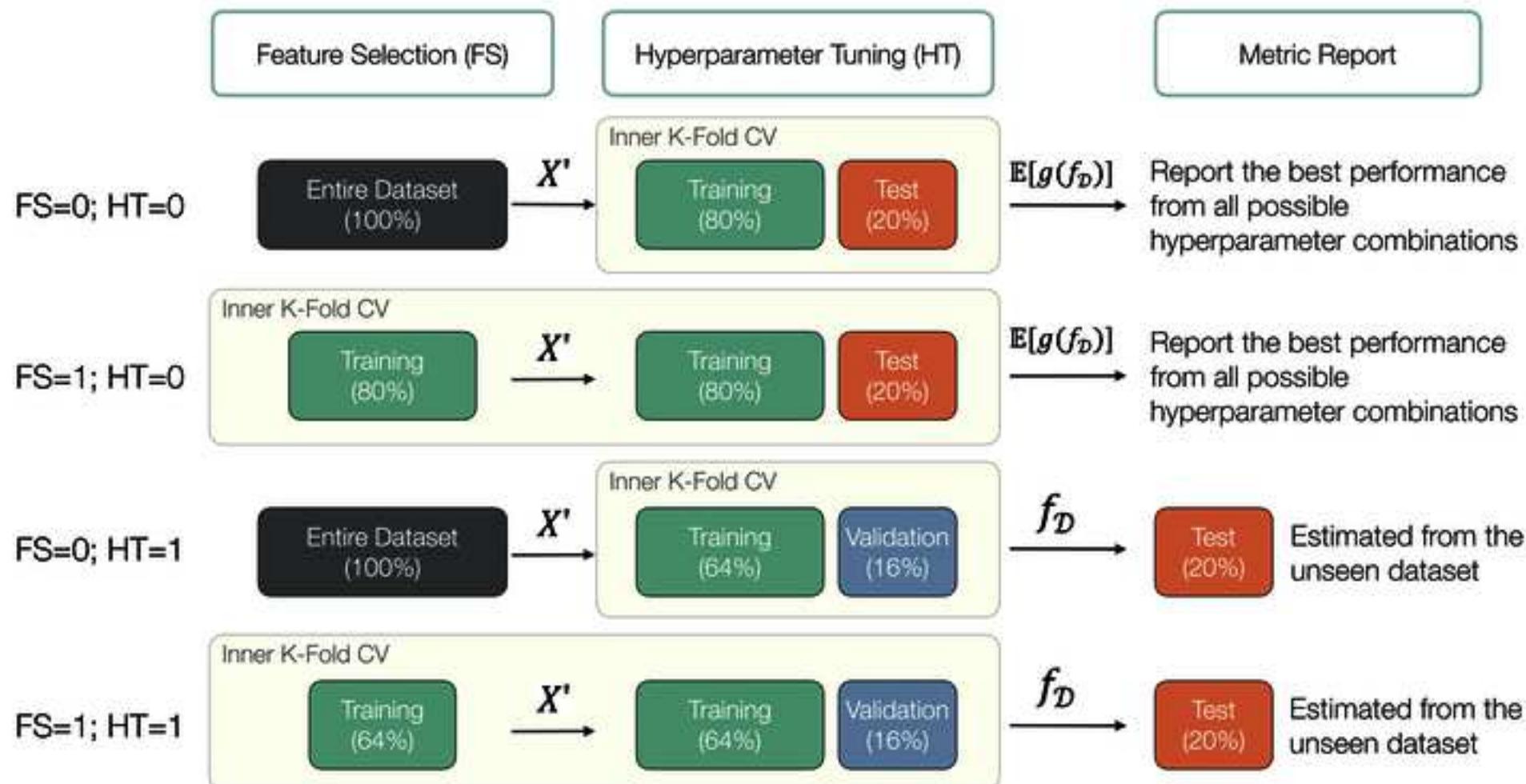


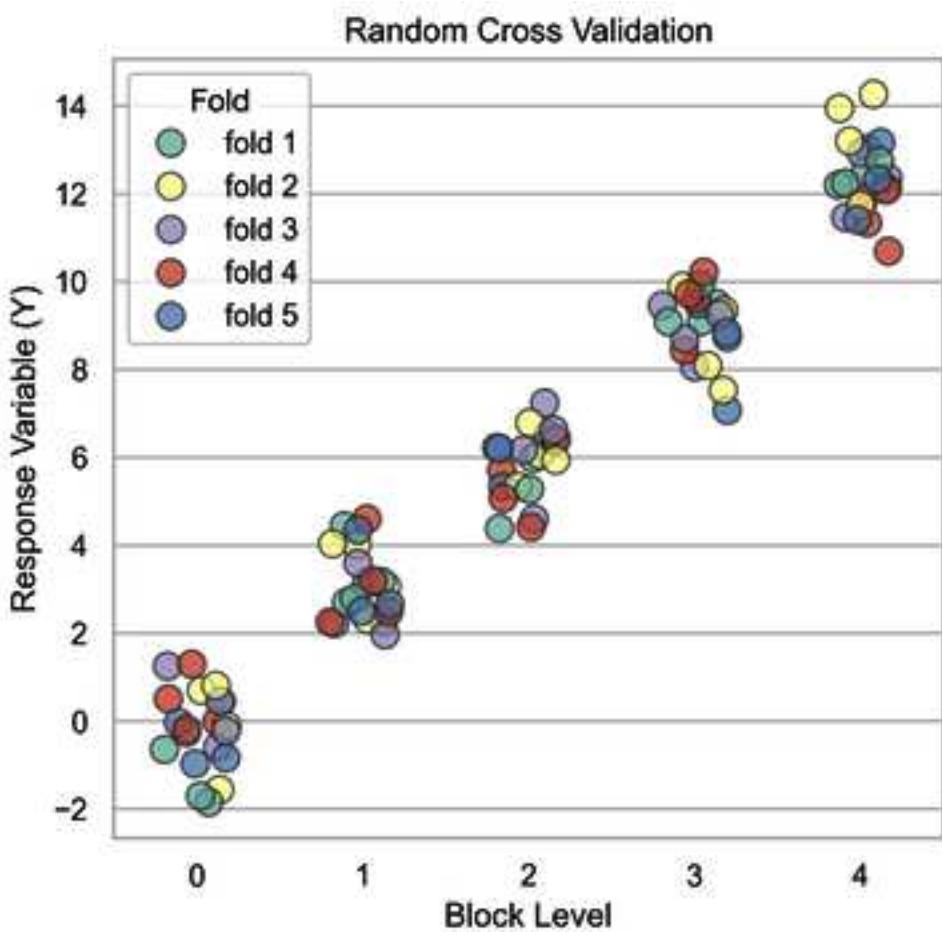
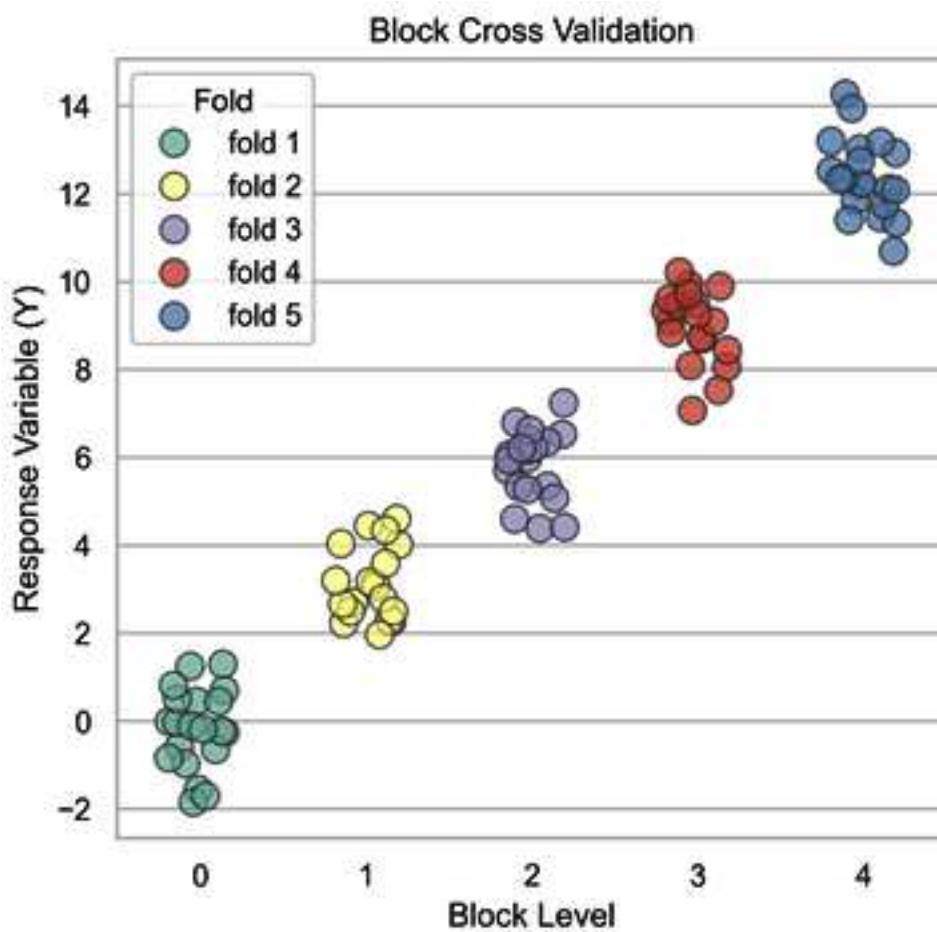
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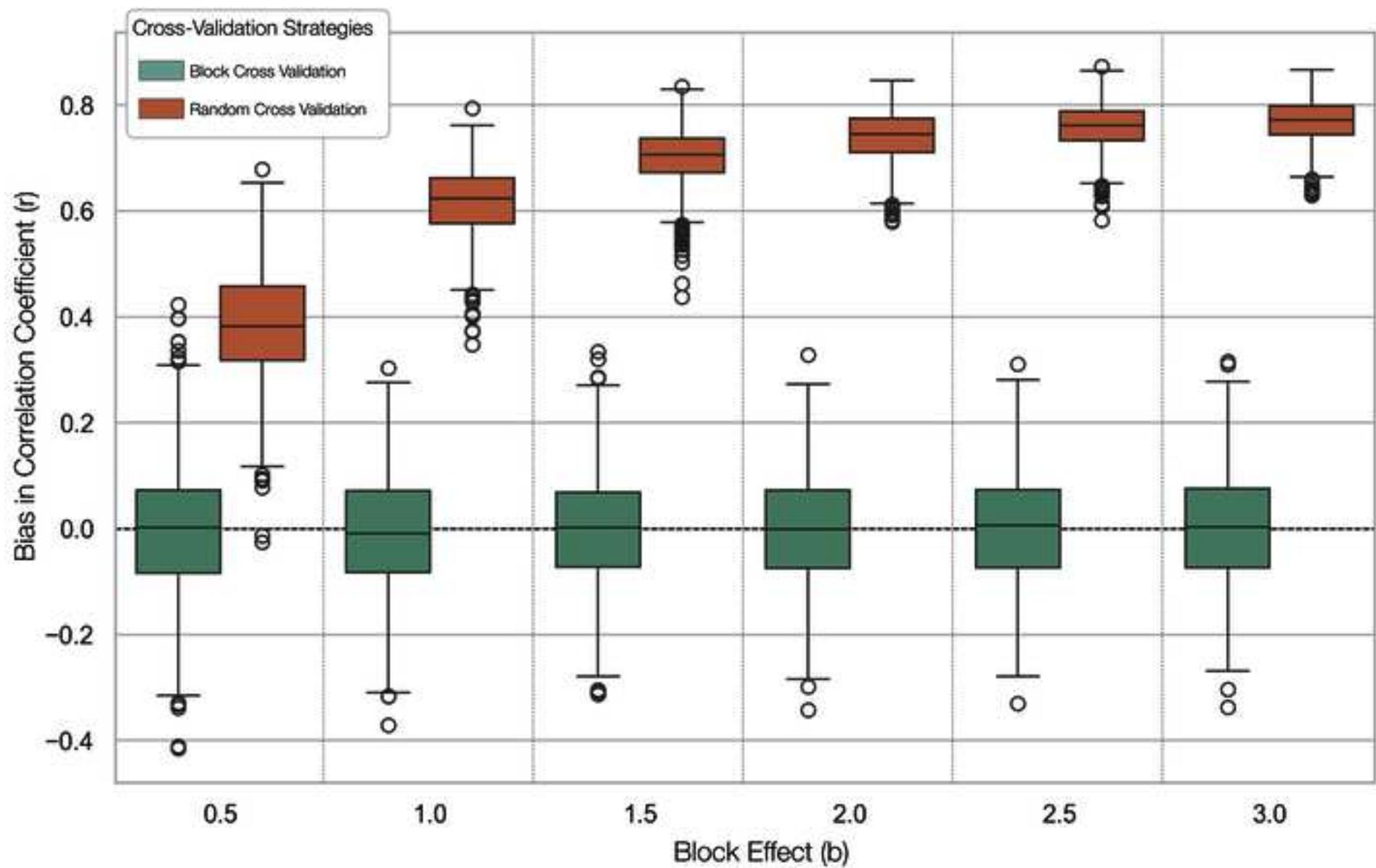


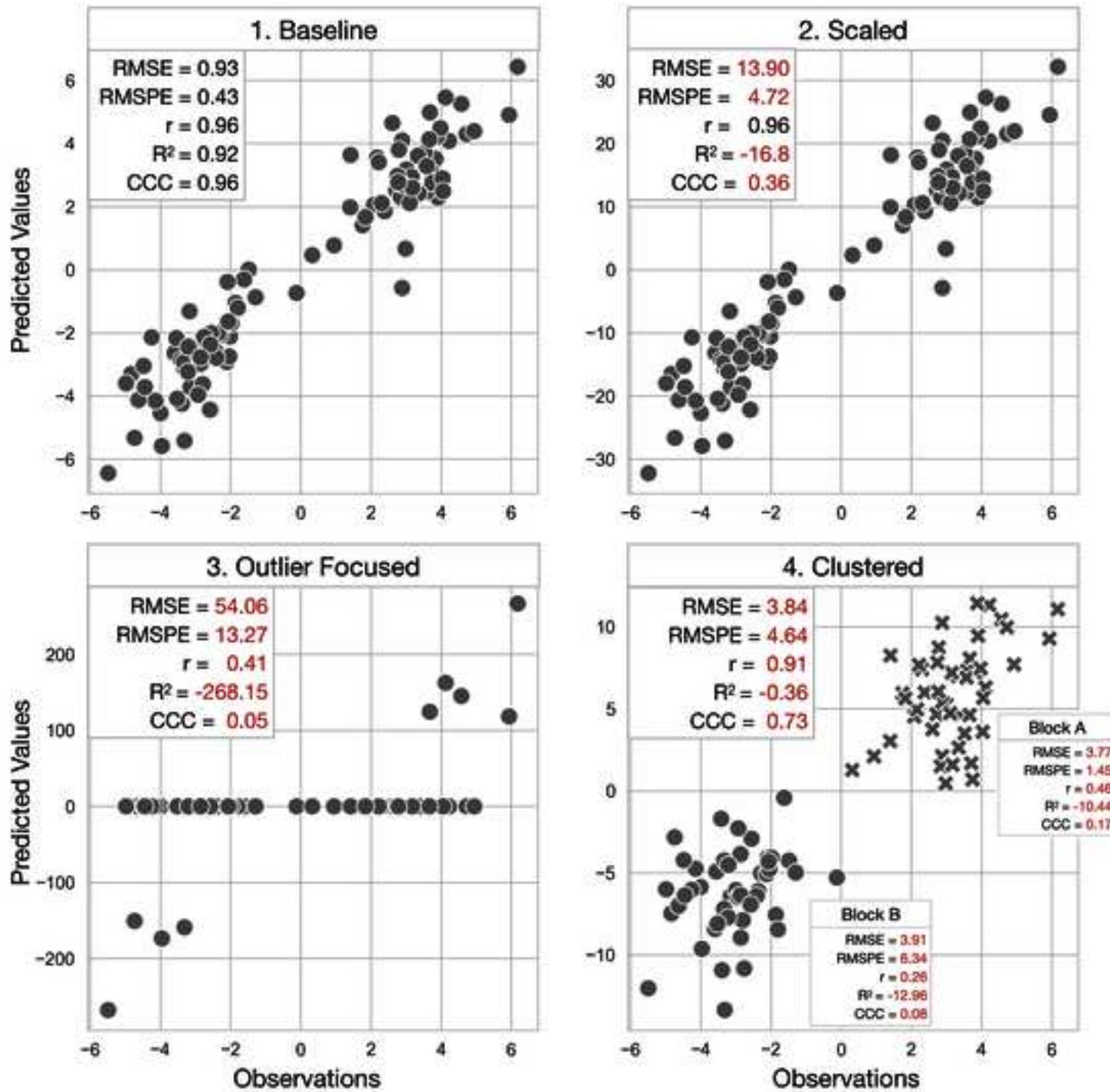












## Ground Truth and Prediction Probability

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Original Labels	
Ground Truth	Prediction Probability
(+)	0.99
(-)	0.70
(+)	0.38
(+)	0.33
(+)	0.26
(-)	0.16
(-)	0.15
(-)	0.14
(-)	0.12
(-)	0.07

Inverted Labels	
Ground Truth	Prediction Probability
(-)	0.01
(+)	0.30
(-)	0.62
(-)	0.67
(-)	0.74
(+)	0.84
(+)	0.85
(+)	0.86
(+)	0.88
(+)	0.93

## Confusion Matrixes

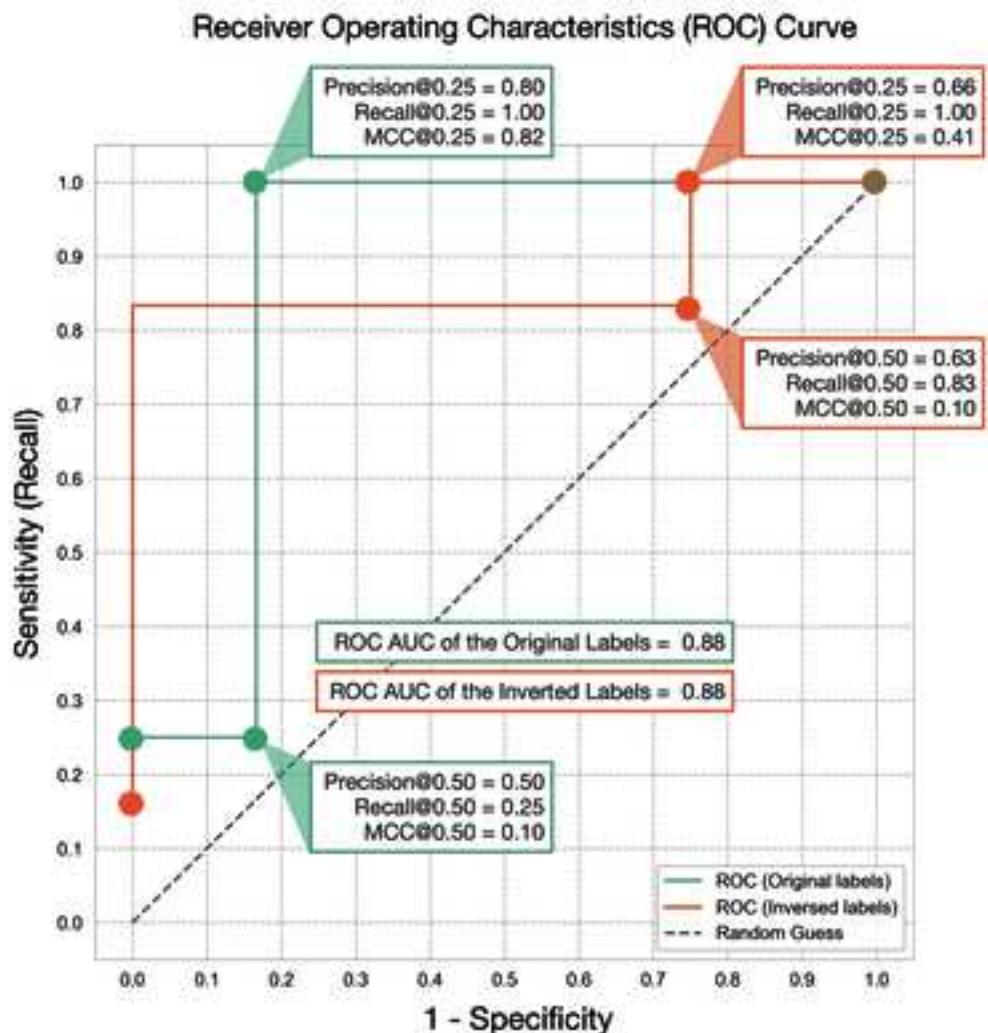
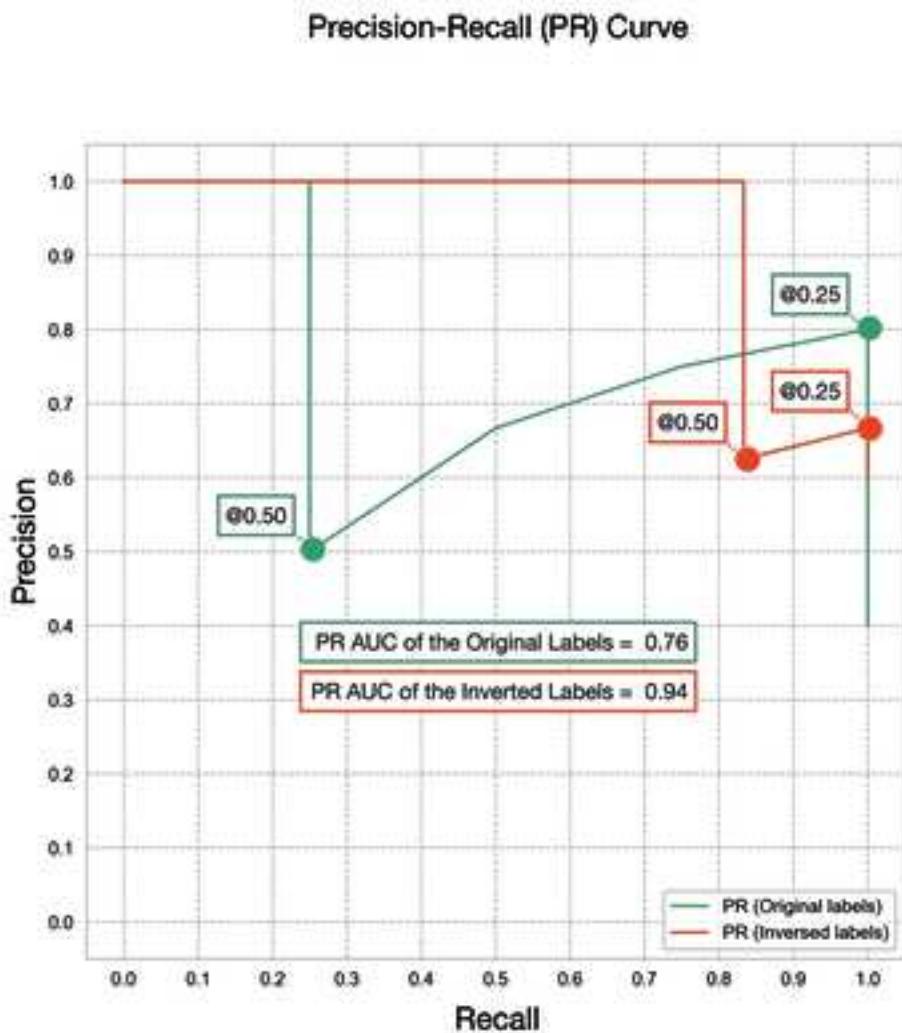
		Predictions		
		(+)	(-)	Recall or Sensitivity
Ground Truth	(+)	TP	FN	$\rightarrow \frac{TP}{TP + FN}$
	(-)	FP	TN	$\rightarrow \frac{FP}{FP + TN}$
		Precision		
		$\frac{TP}{TP + FP}$		

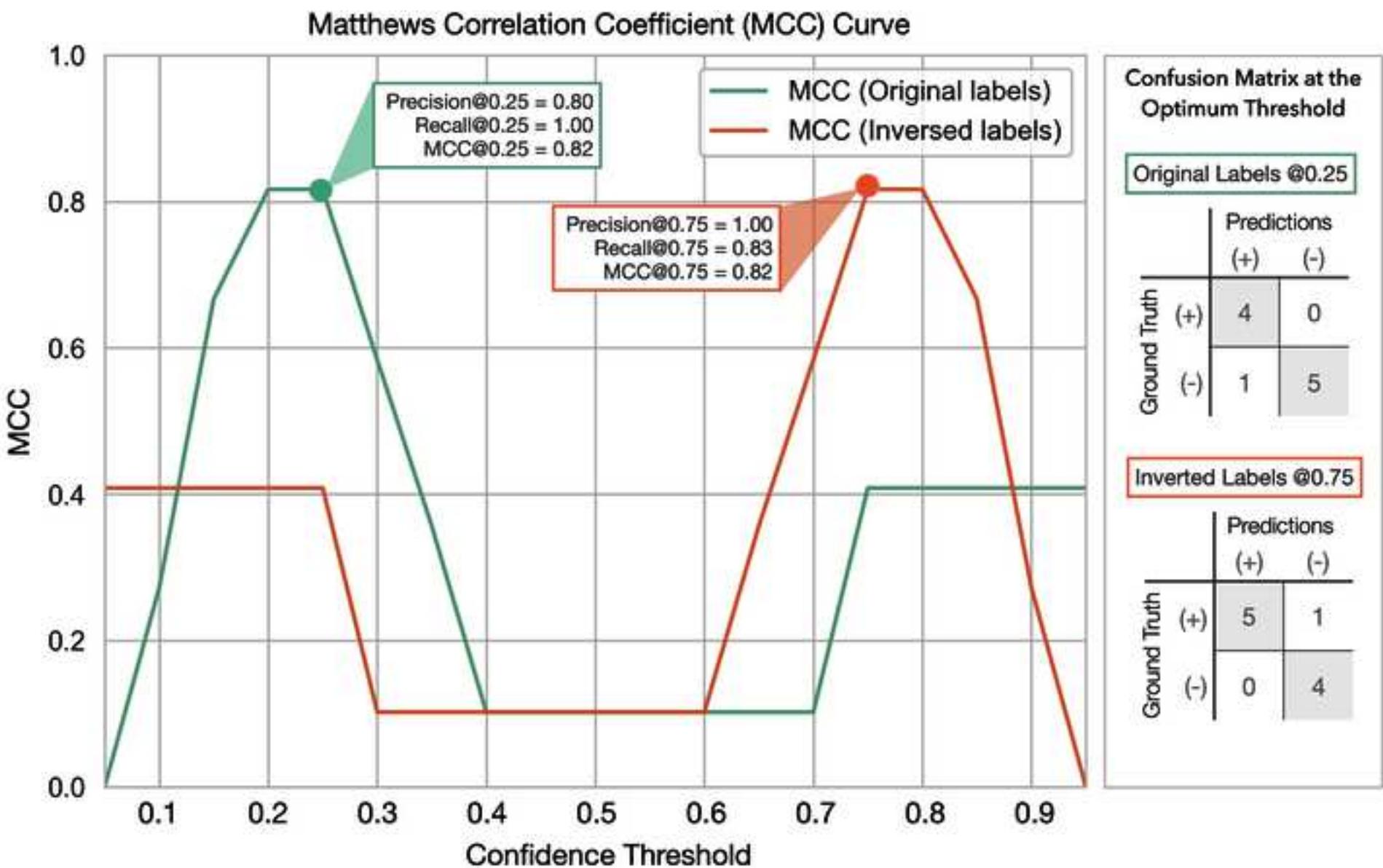
		Predictions	
		(+)	(-)
Ground Truth	(+)	1	3
	(-)	1	5

Accuracy@0.50 = 0.60  
Precision@0.50 = 0.50  
Recall@0.50 = 0.25  
MCC@0.50 = 0.10  
PR AUC = 0.76  
ROC AUC = 0.87

Inverted Labels @0.50		
Ground Truth	Predictions	
	(+)	(-)
(+)	5	1
(-)	3	1

Accuracy@0.50 = 0.600  
 Precision@0.50 = 0.625  
 Recall@0.50 = 0.833  
 MCC@0.50 = 0.100  
 PR AUC = 0.941  
 ROC AUC = 0.875





**Declaration of interests**

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: