

Guidelines for the validation of machine learning predictions of species interactions

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Last revision: *June 8, 2022*

1. The prediction of species interactions is gaining momentum as a way to circumvent limitations in data volume. Yet, ecological networks are challenging to predict because they are typically small and sparse. Dealing with extreme class imbalance is a challenge for most binary classifiers, and there are currently no guidelines as to how predictive models can be trained for this specific problem.
2. Using simple mathematical arguments and numerical experiments in which a variety of classifiers (for supervised learning) are trained on simulated networks, we develop a series of guidelines related to the choice of measures to use for model selection, and the degree of unbiasing to apply to the training dataset.
3. Neither classifier accuracy nor the ROC-AUC are informative measures for the performance of interaction prediction. PR-AUC is a fairer assessment of performance. In some cases, even standard measures can lead to selecting a more biased classifier because the effect of connectance is strong. The amount of correction to apply to the training dataset depends on network connectance, on the measure to be optimized, and only weakly on the classifier.
4. These results reveal that training machines to predict networks is a challenging task, and that in virtually all cases, the composition of the training set needs to be experimented on before performing the actual training. We discuss these consequences in the context of the low volume of data.

1 Ecological networks are a backbone for key ecological and evolutionary processes; yet enumerating all of
2 the interactions between S species is a daunting task, as it scales with S^2 , *i.e.* the squared species richness
3 (Martinez, 1992). Recent contributions to the field of ecological network prediction (Becker et al., 2022;
4 Pichler et al., 2020; Strydom et al., 2021) highlight that although interactions can be predicted by adding
5 ecologically relevant information (in the form of, *e.g.* traits), we do not have robust guidelines as to how
6 the predictive ability of these models should be evaluated, nor about how the models should be trained.
7 Here, by relying on simple derivations and a series of simulations, we formulate a number of such
8 guidelines, specifically for the case of binary classifiers derived from thresholded values. Specifically, we
9 conduct an investigation of the models in terms of their skill (ability to make the right prediction), bias
10 (trends towards systematically over-predicting one class), class imbalance (the relative number of cases
11 representing interactions), and show how these effects interact. We conclude on the fact that models with
12 the best interaction-scale predictive score do not necessarily result in the most accurate representation of
13 the network.

14 The prediction of ecological interactions shares conceptual and methodological issues with two fields in
15 biology: species distribution models (SDMs), and genomics. SDMs suffers from issues affecting
16 interactions prediction, namely low prevalence (due to sparsity of observations/interactions) and data
17 aggregation (due to bias in sampling some locations/species). In previous work, Allouche et al. (2006)
18 suggested that κ was a better test of model performance than the True Skill Statistic (TSS; which we refer
19 to as Youden's informedness); these conclusions were later criticized by Somodi et al. (2017), who
20 emphasized that informedness' is affected both by prevalence and bias. Although this work offers
21 recommendations about the comparison of models, it doesn't establishes baselines or good practices for
22 training on imbalanced ecological data, or ways to remedy the imbalance. Steen et al. (2021) show that,
23 when applying spatial thinning (a process that has no analogue in networks), the best approach to train
24 ML-based SDMs varies according to the balancing of the dataset, and the evaluation measures used. This
25 suggests that there is no single "recipe" that is guaranteed to give the best model. By contrast to networks,
26 SDMs have the advantage of being able to both thin datasets to remove some of the sampling bias (*e.g.*
27 Inman et al., 2021), but also to create pseudo-absences to inflate the number of supposed negatives in the
28 dataset (*e.g.* Iturbide et al., 2015).

29 An immense body of research on machine learning application to life sciences is focused on genomics
30 (which has very specific challenges, see a recent discussion by Whalen et al., 2021); this sub-field has

31 generated recommendations that do not necessarily match the current best-practices for SDMs, and
32 therefore hint at the importance of domain-specific guidelines. Chicco & Jurman (2020) suggest using
33 Matthews correlation coefficient (MCC) over F_1 , as a protection against over-inflation of predicted results;
34 Delgado & Tibau (2019) advocate against the use of Cohen's κ , again in favor of MCC, as the relative
35 nature of κ means that a worse classifier can be picked over a better one; similarly, Boughorbel et al.
36 (2017) recommend MCC over other measures of performance for imbalanced data, as it has more
37 desirable statistical properties. More recently, Chicco et al. (2021) temper the apparent supremacy of the
38 MCC, by suggesting it should be replaced by Youden's informedness (also known as J , bookmaker's
39 accuracy, and the True-Skill Statistic) when the imbalance in the dataset may not be representative of the
40 actual imbalance.

41 Species interaction networks are often under-sampled (Jordano, 2016a, 2016b), and this under-sampling is
42 structured taxonomically (Beauchesne et al., 2016), structurally (de Aguiar et al., 2019) and spatially
43 (Poisot, Bergeron, et al., 2021; Wood et al., 2015). As a consequence, networks suffer from data
44 deficiencies both within and between datasets. This implies that the comparison of classifiers across
45 space, when undersampling varies locally (see *e.g.* McLeod et al., 2021) is non-trivial. Furthermore, the
46 baseline value of classifiers performance measures under various conditions of skill, bias, and prevalence,
47 has to be identified to allow researchers to evaluate whether their interaction prediction model is indeed
48 learning. Taken together, these considerations highlight three specific issues for ecological networks.
49 First, what values of performance measures are indicative of a classifier with no skill? This is particularly
50 important as it can evaluate whether low prevalence can lull us into a false sense of predictive accuracy.
51 Second, independently of the question of model evaluation, is low prevalence an issue for *training* or
52 *testing*, and can we remedy it? Finally, because the low amount of data on interaction makes a lot of
53 imbalance correction methods (see *e.g.* Branco et al., 2015) hard to apply, which indicators can be
54 optimized by sacrificing least amount of positive interaction data?

55 It may sound counter-intuitive to care so deeply about how good a classifier with no-skill is, as by
56 definition, it has no skill. The necessity of this exercise has its roots in the paradox of accuracy: when the
57 desired class ("two species interact") is rare, a model that gets less ecologically performant by only
58 predicting the opposite class ("these two species do not interact") sees its accuracy increase; because most
59 of the guesses have "these two species do not interact" as a correct answer, a model that never predicts
60 interactions would be right an overwhelming majority of the time; it would also be utterly useless. Herein

61 lies the core challenge of predicting species interactions: the extreme imbalance between classes makes
62 the training of predictive models difficult, and their validation even more so as we do not reliably know
63 which negatives are true. The connectance (the proportion of realized interactions, usually the number of
64 interactions divided by the number of species pairs) of empirical networks is usually well under 20%, with
65 larger networks having a lower connectance (MacDonald et al., 2020), and therefore being increasingly
66 difficult to predict.

67 **A primer on binary classifier evaluation**

68 Binary classifiers, which it to say, machine learning algorithms whose answer is a categorical value, are
69 usually assessed by measuring properties of their confusion matrix, *i.e.* the contingency table reporting
70 true/false positive/negative hits. A confusion matrix is laid out as

$$\begin{pmatrix} \text{tp} & \text{fp} \\ \text{fn} & \text{tn} \end{pmatrix}.$$

71 In this matrix, tp is the number of times the model predicts an interaction that exists in the network (true
72 positive), fp is the number of times the model predicts an interaction that does not exist in the network
73 (false positive), fn is the number of times the model fails to predict an interaction that actually exists in the
74 network (false negatives), and tn is the number of times the model correctly predicts that an interaction
75 does not exist (true negatives). From these values, we can derive a number of measures of model
76 performance (see Strydom et al., 2021 for a review of their interpretation in the context of networks). At a
77 coarse scale, a classifier is *accurate* when the trace of the matrix divided by the sum of the matrix is close
78 to 1, with other measures informing us on how the predictions fail.

79 There is an immense diversity of measures to evaluate the performance of classification tasks (Ferri et al.,
80 2009). Here we will focus on five of them with high relevance for imbalanced learning (He & Ma, 2013).
81 The choice of metrics with relevance to class-imbalanced problems is fundamental, because as Japkowicz
82 (2013) unambiguously concluded, “relatively robust procedures used for unskewed data can break down
83 miserably when the data is skewed.” Following Japkowicz (2013), we focus on two ranking metrics (the
84 areas under the Receiver Operating Characteristic and Precision Recall curves), and three threshold

85 metrics (κ , informedness, and MCC; we will briefly discuss F_1 but show early on that it has undesirable
86 properties).

87 The κ measure (Landis & Koch, 1977) establishes the extent to which two observers (the network and the
88 prediction) agree, and is measured as

$$2 \frac{tp \times tn - fn \times fp}{(tp + fp) \times (fp + tn) + (tn + fp) \times (tn + fn)}.$$

89 Informedness (Youden, 1950) (also known as bookmaker informedness or the True Skill Statistic) is
90 $TPR + TNR - 1$, where $TPR = tp/(tp + fn)$ and $TNR = tn/(tn + fp)$. Informedness can be used to find
91 the optimal cutpoint in thresholding analyses (Schisterman et al., 2005); indeed, the maximal
92 informedness corresponds to the point on the ROC curve that is closest to the perfect classifier point. The
93 formula for informedness is

$$\frac{tp}{tp + fn} + \frac{tn}{tn + fp} - 1.$$

94 The MCC is defined as

$$\frac{tp \times tn - fn \times fp}{\sqrt{(tp + fp) \times (tp + fn) \times (tn + fp) \times (tn + fn)}}.$$

95 Finally, F_1 is the harmonic mean of precision (the chance that interaction was correctly detected as such)
96 and sensitivity (the ability to correctly classify interactions), and is defined as

$$2 \frac{tp}{2 \times tp + fp + fn}.$$

97 A lot of binary classifiers are built by using a regressor (whose task is to guess the value of the interaction,
98 and can therefore return a value considered to be a pseudo-probability); in this case, the optimal value
99 below which predictions are assumed to be negative (*i.e.* the interaction does not exist) can be determined
100 by picking a threshold maximizing some value on the ROC or the PR curve. The area under these curves
101 (ROC-AUC and PR-AUC henceforth) give ideas on the overall goodness of the classifier, and the ideal
102 threshold is the point on these curves that minimizes the tradeoff represented in these curves. Saito &

Rehmsmeier (2015) established that the ROC-AUC is biased towards over-estimating performance for imbalanced data; on the contrary, the PR-AUC is able to identify classifiers that are less able to detect positive interactions correctly, with the additional advantage of having a baseline value equal to prevalence. Therefore, it is important to assess whether these two measures return different results when applied to ecological network prediction. The ROC curve is defined by the false positive rate on the x axis, and the true positive rate on the y axis, and the PR curve is defined by the true positive rate on the x axis, and the positive predictive value on the y axis. By comparison with the previous paragraph, it is obvious that F_1 and MCC have ties to the PR curve (being close to the expected PR-AUC), and that informedness has ties to the ROC curve (whereby the threshold maximizing informedness is also the point of maximal inflection on the ROC curve). One important difference between ROC and PR is that the later does not prominently account for the size of the true negative compartments: in short, it is more sensitive to the correct positive predictions. In a context of strong imbalance, PR-AUC is therefore a more stringent test of model performance.

Baseline values for the threshold metrics

In this section, we will assume a network of connectance ρ , *i.e.* having ρS^2 interactions (where S is the species richness), and $(1 - \rho)S^2$ non-interactions. Therefore, the vector describing the *true* state of the network (assumed to be an unweighted, directed network) is a column vector $\mathbf{o}^T = [\rho, (1 - \rho)]$ (we can safely drop the S^2 terms, as we will work on the confusion matrix, which ends up expressing *relative* values). We will apply skill and bias to this matrix, and measure how a selection of performance metrics respond to changes in these values, in order to assess their suitability for model evaluation.

Confusion matrix with skill and bias

In order to write the values of the confusion matrix for a hypothetical classifier, we need to define two characteristics: its skill, and its bias. Skill, here, refers to the propensity of the classifier to get the correct answer (*i.e.* to assign interactions where they are, and to not assign them where they are not). A no-skill classifier guesses at random, *i.e.* it will guess interactions with a probability ρ . The predictions of a no-skill classifier can be expressed as a row vector $\mathbf{p} = [\rho(1 - \rho)]$. The confusion matrix \mathbf{M} for a no-skill classifier is given by the element-wise (Hadamard, outer) product of these vectors $\mathbf{o} \odot \mathbf{p}$, *i.e.*

$$\mathbf{M} = \begin{pmatrix} \rho^2 & \rho(1-\rho) \\ (1-\rho)\rho & (1-\rho)^2 \end{pmatrix}.$$

130 In order to regulate the skill of this classifier, we can define a skill matrix \mathbf{S} with diagonal elements equal
 131 to s , and off-diagonal elements equal to $(1-s)$, and re-express the skill-adjusted confusion matrix as
 132 $\mathbf{M} \odot \mathbf{S}$, *i.e.*

$$\begin{pmatrix} \rho^2 & \rho(1-\rho) \\ (1-\rho)\rho & (1-\rho)^2 \end{pmatrix} \odot \begin{pmatrix} s & (1-s) \\ (1-s) & s \end{pmatrix}.$$

133 When $s = 0$, $\text{Tr}(\mathbf{M}) = 0$ (the classifier is *always* wrong), when $s = 0.5$, the classifier is no-skill and guesses
 134 at random, and when $s = 1$, the classifier is perfect.

135 The second element we can adjust in this hypothetical classifier is its bias, specifically its tendency to
 136 over-predict interactions. Like above, we can do so by defining a bias matrix \mathbf{B} , where interactions are
 137 over-predicted with probability b , and express the final classifier confusion matrix as $\mathbf{M} \odot \mathbf{S} \odot \mathbf{B}$, *i.e.*

$$\begin{pmatrix} \rho^2 & \rho(1-\rho) \\ (1-\rho)\rho & (1-\rho)^2 \end{pmatrix} \odot \begin{pmatrix} s & (1-s) \\ (1-s) & s \end{pmatrix} \odot \begin{pmatrix} b & b \\ (1-b) & (1-b) \end{pmatrix}.$$

138 The final expression for the confusion matrix in which we can regulate the skill and the bias is

$$\mathbf{C} = \begin{pmatrix} s \times b \times \rho^2 & (1-s) \times b \times \rho(1-\rho) \\ (1-s) \times (1-b) \times (1-\rho)\rho & s \times (1-b) \times (1-\rho)^2 \end{pmatrix}.$$

139 In all further simulations, the confusion matrix \mathbf{C} is transformed so that it sums to unity, *i.e.* the entries
 140 are the *proportions* of guesses.

141 **What are the baseline values of performance measures?**

142 In this section, we will change the values of b , s , and ρ , and report how the main measures discussed in
 143 the introduction (MCC, F_1 , κ , and informedness) respond. Before we do so, it is important to explain why

we will not focus on accuracy too much. Accuracy is the number of correct predictions ($\text{Tr}(\mathbf{C})$) divided by the sum of the confusion matrix. For a no-skill, no-bias classifier, accuracy is equal to $\rho^2 + (1 - \rho)^2$; for $\rho = 0.05$, this is ≈ 0.90 , and for $\rho = 0.01$, this is equal to ≈ 0.98 . In other words, the values of accuracy are high enough to be uninformative (for ρ small, $\rho^2 \ll (1 - \rho)^2$). More concerning is the fact that introducing bias changes the response of accuracy in unexpected ways. Assuming a no-skill classifier, the numerator of accuracy becomes $b\rho^2 + (1 - b)(1 - \rho)^2$, which increases when b is low, which specifically means that at equal skill, a classifier that under-predicts interactions will have higher accuracy than an un-biased classifier (because the value of accuracy is dominated by the size of tn, which will increase). These issues are absent from balanced accuracy, but should nevertheless lead us to not report accuracy as the primary measure of network prediction success; moving forward, we will focus on other measures.

In order to examine how MCC, F_1 , κ , and informedness change w.r.t. the imbalance, skill, and bias, we performed a grid exploration of the values of $\text{logit}(s)$ and $\text{logit}(b)$ linearly from -10 to 10 ; $\text{logit}(x) = -10$ means that x is essentially 0, and $\text{logit}(x) = 10$ means it is essentially 1 – this choice was motivated by the fact that most responses are non-linear with regards to bias and skill. The values of ρ were taken linearly in $]0, 0.5]$, which is within the range of connectance for species interaction networks. Note that at this point, there is no network model to speak of; the confusion matrix we discuss can be obtained for any classification task. Based on the previous discussion, the desirable properties for a measure of classifier success should be: an increase with classifier skill, especially at low bias; a hump-shaped response to bias, especially at high skill, and ideally centered around $\text{logit}(b) = 0$; an increase with prevalence up until equiprevalence is reached.

[Figure 1 about here.]

In fig. 1, we show that none of the four measures satisfy all the considerations at once: F_1 increases with skill, and increases monotonously with bias; this is because F_1 does not account for true negatives, and the increase in positive detection masks the over-prediction of interactions. Informedness varies with skill, reaching 0 for a no-skill classifier, but is entirely unsensitive to bias. Both MCC and κ have the same behavior, whereby they increase with skill. κ peaks at increasing values of bias for increasing skill, *i.e.* is likely to lead to the selection of a classifier that over-predicts interactions. By contrast, MCC peaks at the same value, regardless of skill, but this value is not $\text{logit}(b) = 0$: unless at very high classifier skill, MCC risks leading to a model that over-predicts interactions. In fig. 2, we show that all measures except F_1 give

173 a value of 0 for a no-skill classifier, and are forced towards their correct maximal value when skill changes
174 (*i.e.* a more connected networks will have higher values for a skilled classifier, and lower values for a
175 classifier making mostly mistakes).

176 [Figure 2 about here.]

177 These two analyses point to the following recommendations: MCC is indeed more appropriate than κ , as
178 although sensitive to bias, it is sensitive in a consistent way. Informedness is appropriate at discriminating
179 between different skills, but confounded by bias. As both of these measures bring valuable information on
180 the model behavior, we will retain them for future analyses. F_1 is increasing with bias, and should not be
181 prioritized to evaluate the performance of the model. The discussion of sensitivity to bias should come with
182 a domain-specific caveat: although it is likely that interactions documented in ecological networks are
183 correct, a lot of non-interactions are simply unobserved; as predictive models are used for data-inflation
184 (*i.e.* the prediction of new interactions), it is not necessarily a bad thing in practice to select models that
185 predict more interactions than the original dataset, because the original dataset misses some interactions.
186 Furthermore, the weight of positive interactions could be adjusted if some information about the extent of
187 undersampling exists (*e.g.* Branco et al., 2015). In a recent large-scale imputation of interactions in the
188 mammal-virus networks, Poisot, Ouellet, et al. (2021) for example estimated that 93% of interactions are
189 yet to be documented.

190 Numerical experiments on training strategy

191 In the following section, we will generate random bipartite networks, and train four binary classifiers (as
192 well as an ensemble model using the sum of ranged outputs from the component models) on 50% of the
193 interaction data. In practice, testing usually uses 70% of the total data; for ecological networks, where
194 interactions are sparse *and* the number of species is low, this may not be the best solution, as the testing
195 set becomes constrained not by the *proportion* of interactions, but by their *number*. Preliminary
196 experiments using different splits revealed no qualitative change in the results. Networks are generated by
197 picking a random infectiousness trait v_i for 100 species (from a beta distribution $B(\alpha = 6, \beta = 8)$
198 distribution), and a resistance trait h_j for 100 species (from $B(\alpha = 2, \beta = 8)$ distribution). There is an
199 interaction between i and j when $v_i - \xi/2 \leq h_j \leq v_i + \xi/2$, where ξ is a constant regulating the

connectance of the network (visual exploration of the parameters show that there is an almost 1:1 relationship between ξ and connectance), and varies uniformly in $[0.05, 0.35]$. This model gives fully interval networks that are close analogues to the bacteria–phage model of Weitz et al. (2005), with both a modular structure and a non-uniform degree distribution. This dataset is easy for almost any algorithm to learn: when trained with features $[v_i, h_j, \text{abs}(v_i, h_j)]^T$ to predict the interactions between i and j , all four models presented below were able to reach almost perfect predictions all the time (data not presented here) – this is in part because the rule (there is maximum value of the distance between traits for which there is an interaction) is fixed for all interactions, and any method able to learn non-linear relationships should infer it without issues. In order to make the problem more difficult to solve, we use $[v_i, h_j]$ as a feature vector (*i.e.* the traits on which the models are trained), and therefore the models will have to uncover that the rule for interaction is $\text{abs}(v_i, h_j) \leq \xi$. The models therefore all have the following form, where $i_{i,j}$ is an interaction from species i to species j :

$$\begin{bmatrix} i_{1,1} \\ i_{1,2} \\ \vdots \\ i_{m,n-1} \\ i_{m,n} \end{bmatrix} \propto \begin{bmatrix} v_1 & h_1 \\ v_1 & h_2 \\ \vdots & \vdots \\ v_m & h_{n-1} \\ v_m & h_n \end{bmatrix}$$

The training sample is composed of 50% of the 10^4 possible entries in the network, *i.e.* $n = 5000$. Out of these interactions, we pick a proportion ν (the training set balance) to be positive, so that the training set has νn interactions, and $(1 - \nu)n$ non-interactions. We vary ν uniformly in $]0, 1[$. This allows to evaluate how the measures of binary classification performance respond to artificially rebalanced dataset for a given network connectance. The rest of the dataset is used as a testing set, on which all further measures are calculated. Note that although the training set is balanced arbitrarily, the testing set is assembled so that it has the exact connectance of the entire network; this ensures that the model is evaluated under the class imbalance where the predictions will be made, which represents a more meaningful evaluation. Note also that although the simulated networks are bipartite, the algorithms have no “knowledge” of the network structure, and simply look at pairs of species; therefore, the approach outlined here would also work for unipartite networks.

The dataset used for numerical experiments is composed of a grid of 35 values of connectance (from 0.011

224 to 0.5) and 35 values of ν (from 0.02 to 0.98); for each pair of values, 500 networks are generated and
225 predicted. For each network, we train four machines: a trait-based k-NN (e.g. Desjardins-Proulx et al.,
226 2017), a regression tree, a regression random forest, and a boosted regression tree. Following results from
227 Pichler et al. (2020), linear models have not been considered (in any way, the relationship in the simulated
228 networks is non-linear). The point of these numerical experiments is *not* to recommend the best model
229 (this is likely problem-specific), but to highlight a series of recommendations that would work for
230 supervised learning tasks. All models were taken from the MLJ.jl package (Blaom et al., 2020; Blaom &
231 Vollmer, 2020) in Julia 1.7 (Bezanson et al., 2017). All machines use the default parameterization; this is
232 an obvious deviation from best practices, as the hyperparameters of any machine require training before
233 its application on a real dataset. As we use 612500 such datasets, this would require over 2 millions unique
234 instances of tweaking the hyperparameters, which is prohibitive from a computing time point of view. An
235 important thing to keep in mind is that the problem we simulate has been designed to be simple to solve:
236 we expect all machines with sensible default parameters to fare well — the results presented in the later
237 sections show that this assumption is warranted, and we further checked that the models do not overfit by
238 ensuring that there is never more than 5% of difference between the accuracy on the training and testing
239 sets. All machines return a quantitative prediction, usually (but not necessarily) in $[0, 1]$, which is
240 proportional (but not necessarily linearly) to the probability of an interaction between i and j . The
241 ROC-AUC and PR-AUC (and therefore the thresholds) can be measured by integrating over the domain of
242 the values return by each machine, but in order to make the average-based ensemble model more
243 meaningful, all predictions are expressed in $[0, 1]$.

244 In order to pick the best adjacency matrix for a given trained machine, we performed a thresholding
245 approach using 500 steps on predictions from the testing set, and picking the threshold that maximized
246 Youden’s informedness. During the thresholding step, we measured the area under the receiver operating
247 characteristic (ROC-AUC) and precision-recall (PR-AUC) curves, as measures of overall performance over
248 the range of returned values. We report the ROC-AUC and PR-AUC, as well as a suite of other measures as
249 introduced in the next section, for the best threshold. The ensemble model was generated by summing the
250 predictions of all component models on the testing set (ranged in $[0, 1]$), then put through the same
251 thresholding process. The complete code to run the simulations is available at [10.17605/OSF.IO/JKEWD](https://github.com/10.17605/OSF.IO/JKEWD).

252 After the simulations were completed, we removed all runs (i.e. triples of model, ξ , and ν) for which at
253 least one of the following conditions was met: the accuracy was 0, the true positive or true negative rates

were 0, the connectance was larger than 0.25. This removes both the obviously failed model runs, and the networks that are more densely connected compared to the connectance of empirical food webs (and are therefore less difficult to predict, being less imbalanced; preliminary analyses of data with a connectance larger than 0.3 revealed that all machines reached consistently high performance).

Effect of training set balance on performance

In fig. 3, we present the response of two thresholding measures (PR-AUC and ROC-AUC) and two ranking measures (Informedness and MCC) to a grid of 35 values of training set balance, and 35 values of connectance, for the four component models as well as the ensemble. ROC-AUC is always high, and does not vary with training set balance. On the other hand, PR-AUC shows very strong responses, increasing with training set balance. It is notable here that two classifiers that seemed to be performing well (Decision Tree and Random Forest) based on their MCC are not able to reach a high PR-AUC even at higher connectances. All models reached a higher performance on more connected networks, and using more balanced training sets. In all cases, informedness was extremely high, which is an expected consequence of the fact that this is the value we optimized to determine the cutoff. MCC increased with training set balance, although this increase became less steep with increasing connectance. Three of the models (kNN, decision tree, and random forest) only increased their PR-AUC sharply when the training set was heavily imbalanced towards more interactions. Interestingly, the ensemble almost always outclassed its component models. For larger connectances (less difficult networks to predict, as they are more balanced), MCC and informedness started decreasing when the training set bias got too close to one, suggesting that a training set balance of 0.5 may often be appropriate if these measures are the one to optimize.

[Figure 3 about here.]

Based on the results presented in fig. 3, it seems that informedness and ROC-AUC are not necessarily able to discriminate between good and bad classifiers (although this result may be an artifact for informedness, as it has been optimized when thresholding). On the other hand, MCC and PR-AUC show a strong response to training set balance, and may therefore be more useful at model comparison.

Required amount of positives to get the best performance

The previous results revealed that the measure of classification performance responds both to the bias in the training set *and* to the connectance of the network; from a practical point of view, assembling a training set requires one to withhold positive information, which in ecological networks are very scarce (and typically more valuable than negatives, on which there is a doubt). For this reason, across all values of connectance, we measured the training set balance that maximized a series of performance measures. When this value is high, the training set needs to skew more positive in order to get a performant model; when this value is about 0.5, the training set needs to be artificially balanced to optimize the model performance. These results are presented in fig. 4.

[Figure 4 about here.]

The more “optimistic” measures (ROC-AUC and informedness) required a biasing of the dataset from about 0.4 to 0.75 to be maximized, with the amount of bias required decreasing only slightly with the connectance of the original network. MCC and PR-AUC required values of training set balance from 0.75 to almost 1 to be optimized, which is in line with the results of the previous section, *i.e.* they are more stringent tests of model performance. These results suggest that learning from a dataset with very low connectance can be a different task than for more connected networks: it becomes increasingly important to capture the mechanisms that make an interaction *exist*, and therefore having a slightly more biased training dataset might be beneficial. As connectance increases, the need for biased training sets is less prominent, as learning the rules for which interactions *do not* exist starts gaining importance.

[Figure 5 about here.]

When trained at their optimal training set balance, connectance still had a significant impact on the performance of some machines fig. 5. Notably, Decision Tree, and k-NN, as well as Random forest to a lower extent, had low values of PR-AUC. In all cases, the Boosted Regression Tree was reaching very good predictions (especially for connectances larger than 0.1), and the ensemble was almost always scoring perfectly. This suggests that all the models are biased in different ways, and that the averaging in the ensemble is able to correct these biases. We do not expect this last result to have any generality, and provide a discussion of a recent example in which the ensemble was performing worse than its components models.

307 Do better classification accuracy result in more realistic networks?

308 In this last section, we generate a network using the same model as before, with $S_1, S_2 = 50, 80$ species, a
 309 connectance of ≈ 0.16 ($\xi = 0.19$), and a training set balance of 0.5, as fig. 4 suggests this is the optimal
 310 training set balance for this range of connectance. The prediction made on the complete dataset is
 311 presented in fig. 6.

312 [Figure 6 about here.]

313 The trained models were then thresholded (again by optimising informedness), and their predictions
 314 transformed back into networks for analysis; specifically, we measured the connectance, nestedness
 315 (REF), modularity (REF), asymmetry, and network dissimilarity (REF). This process was repeated 250
 316 times, and the results are presented in tbl. 1. The random forest model is an interesting instance here: it
 317 produces the network that looks the most like the original dataset, despite having a very low PR-AUC,
 318 suggesting it hits high recall at the cost of low precision. Although the ensemble was able to reach a very
 319 high PR-AUC (and a very high ROC-AUC), this did not necessarily translate into more accurate
 320 reconstructions of the structure of the network. This result bears elaborating. Measures of model
 321 performance capture how much of the interactions and non-interactions are correctly identified. As long
 322 as these predictions are not perfect, some interactions will be predicted at the “wrong” position in the
 323 network; these measures cannot describe the structural effect of these mistakes. On the other hand,
 324 measures of network structure can have the same value with interactions that fall at drastically different
 325 positions; this is in part because a lot of these measures covary with connectance, and in part because as
 326 long as these values are not 0 or their respective maximum, there is a large number of network
 327 configurations that can have the same value. That ROC-AUC is consistently larger than PR-AUC may be a
 328 case of this measure masking models that are not, individually, strong predictors (Jeni et al., 2013).

Table 1: Values of four performance metrics, and five network structure metrics, for 500 independent predictions similar to the ones presented in fig. 6. The values in **bold** indicate the best value for each column (including ties). Because the values have been rounded, values of 1.0 for the ROC-AUC column indicate an average ≥ 0.99 .

Model	MCC	Inf.	ROC-AUC	PR-AUC	Conn.	η	Q	A	Jaccard
Decision tree	0.59	0.94	0.97	0.04	0.17	0.64	0.37	0.42	0.1

	Model	MCC	Inf.	ROC-AUC	PR-AUC	Conn.	η	Q	A	Jaccard
	BRT	0.46	0.91	0.97	0.36	0.2	0.78	0.29	0.41	0.19
	Random Forest	0.72	0.98	0.99	0.1	0.16	0.61	0.38	0.42	0.06
	k-NN	0.71	0.98	0.99	0.02	0.16	0.61	0.39	0.42	0.06
	<i>Ensemble</i>	0.74	0.98	1.0	0.79	0.16	0.61	0.38	0.42	0.06
	<i>Data</i>					0.16	0.56	0.41	0.42	0.0

Guidelines for the assessment of network predictive models

It is noteworthy that the ensemble model was systematically better than the component models. We do not expect that ensembles will *always* be better than single models. Networks with different structures than the one we simulated here may respond in different ways, especially if the rules are fuzzier than the simple rule we used here. In a recent multi-model comparison involving supervised and unsupervised learning, Becker et al. (2022) found that the ensemble was *not* the best model. There is no general conclusion to draw from this besides reinforcing the need to be pragmatic about which models should be included in the ensemble, or whether to use an ensemble at all. In a sense, the surprising performance of the ensemble model should form the basis of the last recommendation: optimal training set balance and its interaction with connectance and the specific binary classifier used is, in a sense, an hyperparameter that should be assessed. The distribution of results in fig. 4 and fig. 5 show that there are variations around the trend. TODO fix this sentence

We establish that due to the low prevalence of interactions, even poor classifiers applied to food web data will reach a high accuracy; this is because the measure is dominated by the accidentally correct predictions of negatives. On simulated confusion matrices with ranges of imbalance that are credible for ecological networks, MCC had the most desirable behavior, and informedness is a linear measure of classifier skill. By performing simulations with four models and an ensemble, we show that informedness and ROC-AUC are consistently high on network data, and that MCC and PR-AUC are more accurate measures of the effective performance of the classifier. Finally, by measuring the structure of predicted networks, we highlight an interesting paradox: the models with the best performance measures are not necessarily the models with the closest reconstructed network structure. We discuss these results in the

context of establishing guidelines for the prediction of ecological interactions.

The results presented here highlight an interesting paradox: although the Random Forest was ultimately able to get a correct estimate of network structure [tbl. 1](#), it ultimately remains a poor classifier, as evidenced by its low PR-AUC. This suggests that the goal of predicting *interactions* and predicting *networks* may not be solvable in the same way – of course a perfect classifier of interactions would make a perfect network prediction; but even the best scoring predictor of interactions (the ensemble model) had not necessarily the best prediction of network structure. The tasks of predicting networks structure and of predicting interactions within networks are essentially two different ones. For some applications (*e.g.* comparison of network structure across gradients), one may care more about a robust estimate of the structure, at the cost of putting some interactions at the wrong place. For other applications (*e.g.* identifying pairs of interacting species), one may conversely care more about getting as many pairs right, even though the mistakes accumulate in the form of a slightly worse estimate of network structure. How these two approaches can be reconciled is undoubtedly a task for further research. Despite this apparent tension at the heart of the predictive exercise, we can use the results presented here to suggest a number of guidelines.

First, because we should have more trust in reported interactions than in reported absences of interactions, we can draw on previous literature to recommend informedness as a measure to decide on a threshold ([Chicco et al., 2021](#)); this being said, because informedness is insensitive to bias, the model performance is better evaluated through the use of MCC [fig. ??](#). Because F_1 is monotonously sensitive to classifier bias [fig. 1](#) and network connectance [fig. 2](#), MCC should be preferred as a measure of model evaluation and comparison.

Second, because the PR-AUC responds more to network connectance [fig. 5](#) and training set imbalance [fig. ??](#), it should be used as a measure of model performance over the ROC-AUC. This is not to say that ROC-AUC should be discarded (in fact, a low ROC-AUC is a sign of an issue with the model), but that its interpretation should be guided by the PR-AUC value. Specifically, a high ROC-AUC is not informative, as it can be associated to a low PR-AUC (see *e.g.* Random Forest in [tbl. 1](#)) This again echoes recommendations from other fields ([Jeni et al., 2013](#); [Saito & Rehmsmeier, 2015](#)).

Third, for the same reasons that should give more weight to PR-AUC than ROC-AUC, accuracy alone should not be used as a measure of model performance, but rather as an expectation of how well the model should behave given the balance in the set on which predictions are made; this is because, as

380 derived earlier, the expected accuracy for a no-skill no-bias classifier is $\rho^2 + (1 - \rho)^2$ (where ρ is the class
 381 balance), which will very easily by large. This pitfall is notably illustrated in a recent model (Caron et al.,
 382 2022) wherein the authors claim that using a training set of $n = 10^4$ with only 100 positive interactions
 383 (representing 0.1% of the total interactions) reached a good accuracy. Reporting a good accuracy isn't
 384 informative is this accuracy isn't (i) compared to the baseline expected value under the class balance, and
 385 (ii) interpreted in the context of a measure that isn't sensitive to true negatives (like MCC).

386 Finally, network connectance (*i.e.* the empirical class imbalance) should inform the composition of the
 387 training and testing set. In the approach outlined here, we treat the class imbalance as an
 388 hyper-parameter, but *test* the model on a set that has the same class imbalance as the actual dataset. This
 389 is an important change to the overall methodology so far, as it ensure that the prediction environment
 390 matches the testing environment, and so the values on the testing set can be directly compared to the
 391 values for the actual prediction. A striking result fig. 4 is that Informedness was almost always maximal at
 392 50/50 balance (regardless of connectance), whereas MCC required *more* positives to be maximized when
 393 connectance *increases*. This has an important consequence in ecological networks, for which the pool of
 394 positive cases (interactions) to draw from is typically small: the most parsimonious measure (*i.e.* the one
 395 requiring to discard the least amount of information to train the model) will give the best validation
 396 potential, and is probably the informedness (maximizing informedness is the generally accepted default
 397 for imbalanced classification; Schisterman et al., 2005). This last result further strengthens the assumption
 398 that the amount of bias *is* an hyper-parameter that must be fine-tuned, as using the wrong bias can lead to
 399 models with excess bias or lower skill.

400 One key element for real-life data that can make the prediction exercise more tractable is that some
 401 interactions can safely be assumed to be impossible; indeed, a lot of networks admit a stochastic block
 402 model as a good approximation (*e.g.* Xie et al., 2017). In ecological networks, this can be due to spatial
 403 constraints (Valdovinos, 2019), or to the long-standing knowledge that some links are “forbidden” due to
 404 traits (Olesen et al., 2011) or abundances (Canard et al., 2014). The matching rules (Olito & Fox, 2015;
 405 Strona & Veech, 2017) can be incorporated in the model either by adding compatibility traits, or by *only*
 406 training the model on pairs of species that are not likely to be forbidden links; having this information
 407 would allow to assemble training/testing sets that have true negatives, and in this situation, it may be
 408 possible to use the more usual 70/30 split. Besides forbidden links, a real-life case that may arise is
 409 multi-interaction or multi-layer networks (Pilosof et al., 2017). These can be studied using the same

410 general approach outlined here, either by assuming that pairs of species can interact in more than one way
411 (wherein one would train a model for each type of interaction, based on the relevant predictors), or by
412 assuming that pairs of species can only have one type of interaction (wherein this becomes a multi-label
413 classification problem).

414 **Acknowledgements:** We acknowledge that this study was conducted on land within the traditional
415 unceded territory of the Saint Lawrence Iroquoian, Anishinabewaki, Mohawk, Huron-Wendat, and
416 Omàmiwininiwak nations. We thank Colin J. Carlson, Michael D. Catchen, Giulio Valentino Dalla Riva,
417 and Tanya Strydom for inputs on earlier versions of this manuscript. This research was enabled in part by
418 support provided by Calcul Québec (www.calculquebec.ca) and Compute Canada
419 (www.computecanada.ca) through the Narval general purpose cluster. TP is supported by a NSERC
420 Discovery Grant and Discovery Acceleration Supplement, by funding to the Viral Emergence Research
421 Initiative (VERENA) consortium including NSF BII 2021909, and by a grant from the Institut de
422 Valorisation des Données (IVADO).

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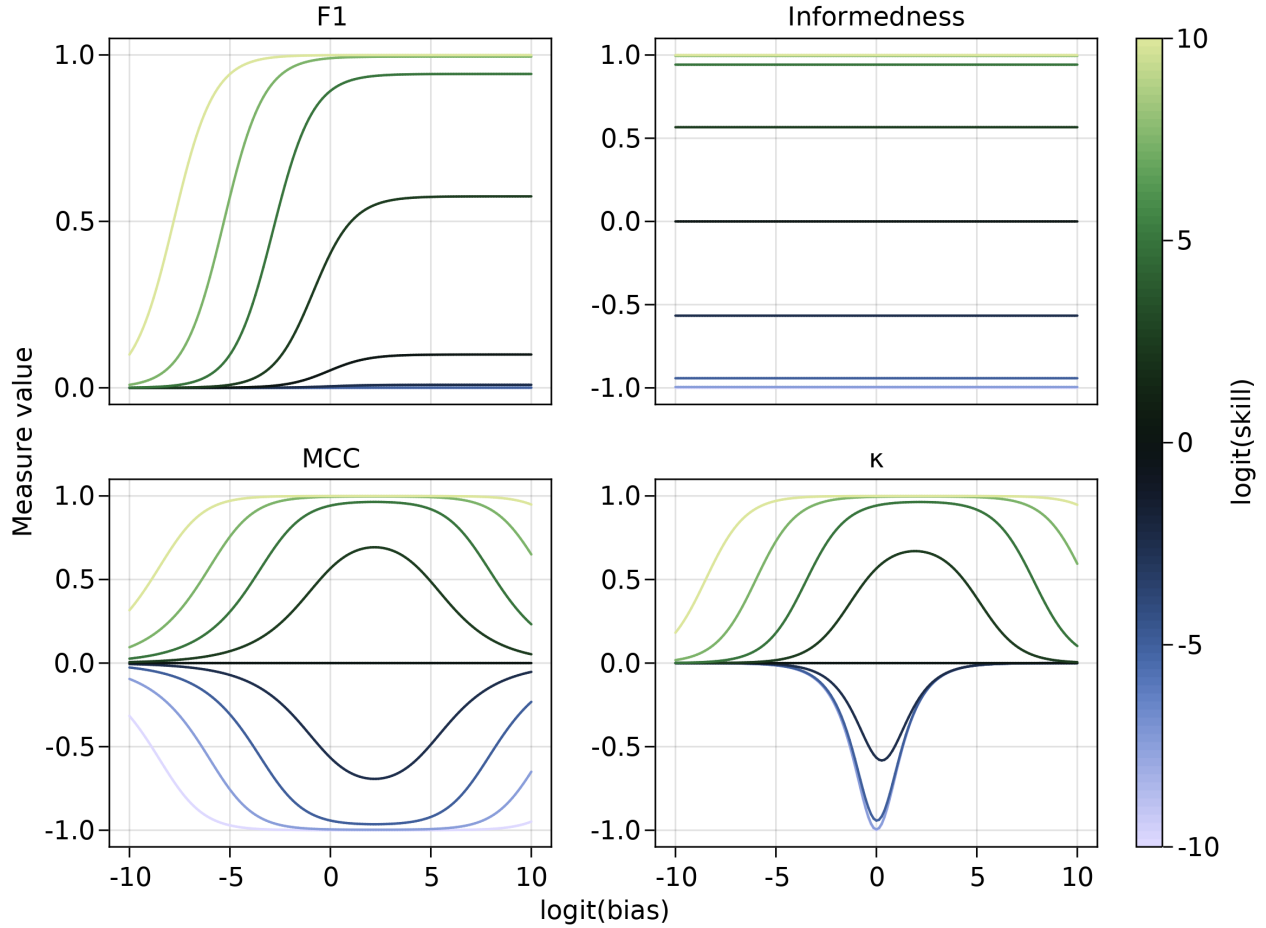


Figure 1: Consequences of changing the classifier skills (s) and bias (b) for a connectance $\rho = 0.15$, on F_1 , informedness, MCC, and κ . Accuracy increases with skill, but also increases when the bias tends towards estimating *fewer* interactions. The F_1 score increases with skill but also increases when the bias tends towards estimating *more* interactions. Interestingly, κ responds as expected to skill (being negative whenever $s < 0.5$), and peaks for values of $b \approx 0.5$; nevertheless, the value of bias for which κ is maximized is *not* $b = 0.5$, but instead increases with classifier skill. In other words, at equal skill, maximizing κ would lead to select a *more* biased classifier.

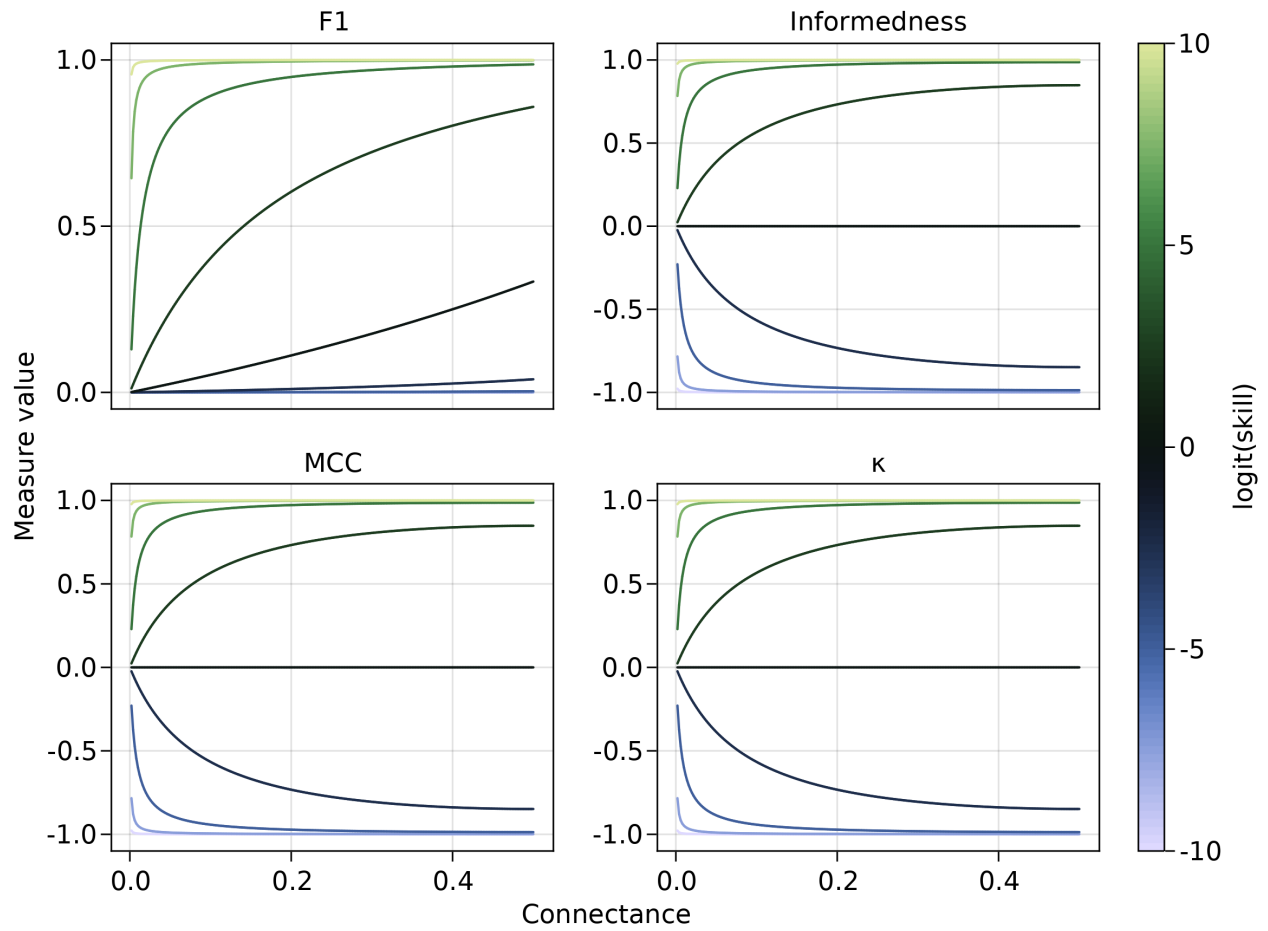


Figure 2: As in fig. 1, consequences of changing connectance for different levels of classifier skill, assuming no classifier bias. Informedness, κ , and MCC do increase with connectance, but only when the classifier is not no-skill; by way of contrast, a more connected network will give a higher F_1 value even with a no-skill classifier.

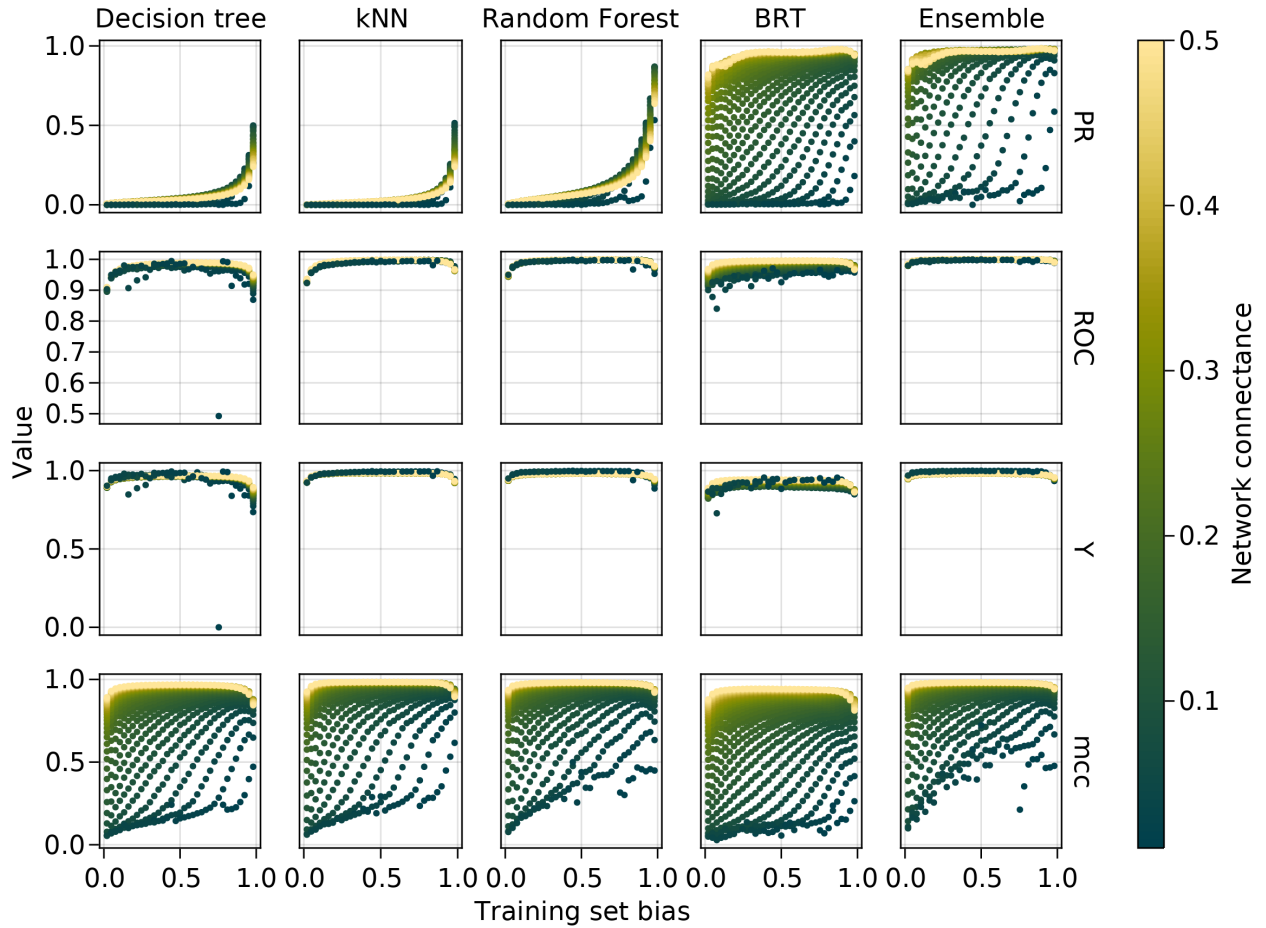


Figure 3: Response of MCC, Informedness, ROC-AUC, and PR-AUC to changes in the training set balance (on the x axis) for a series of increasing connectances (color). All of these values approach 1 for a good model, but should be lower when the prediction is more difficult. Informedness is consistently high, and by contrast, MCC increases with additional training set balance. Across all models, training on a more connected network is easier. ROC-AUC is consistently high, and therefore not properly able to separate good from poor classifiers. On the other hand, PR-AUC responds to changes in the training set.

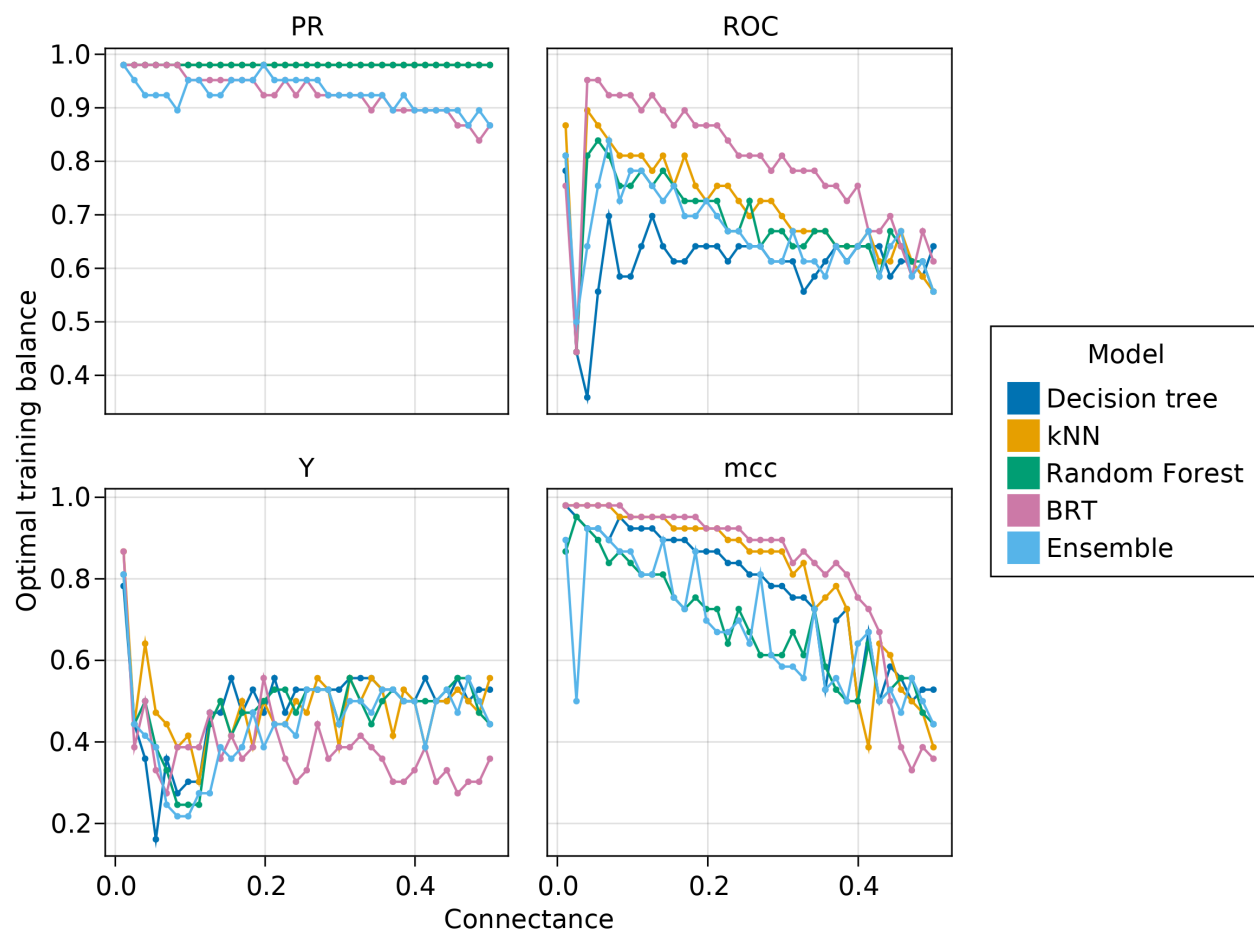


Figure 4: Value of the optimal training set balance for the different models and measures evaluated here, over a range of connectances. Informedness was reliably maximized for balanced training sets, and kept this behavior across models. For other measures, larger connectances in the true network allowed lower biases in the training set. In a large number of cases, “over-correcting” by having training sets with more than half instances representing interactions would maximize the values of the model performance measures.

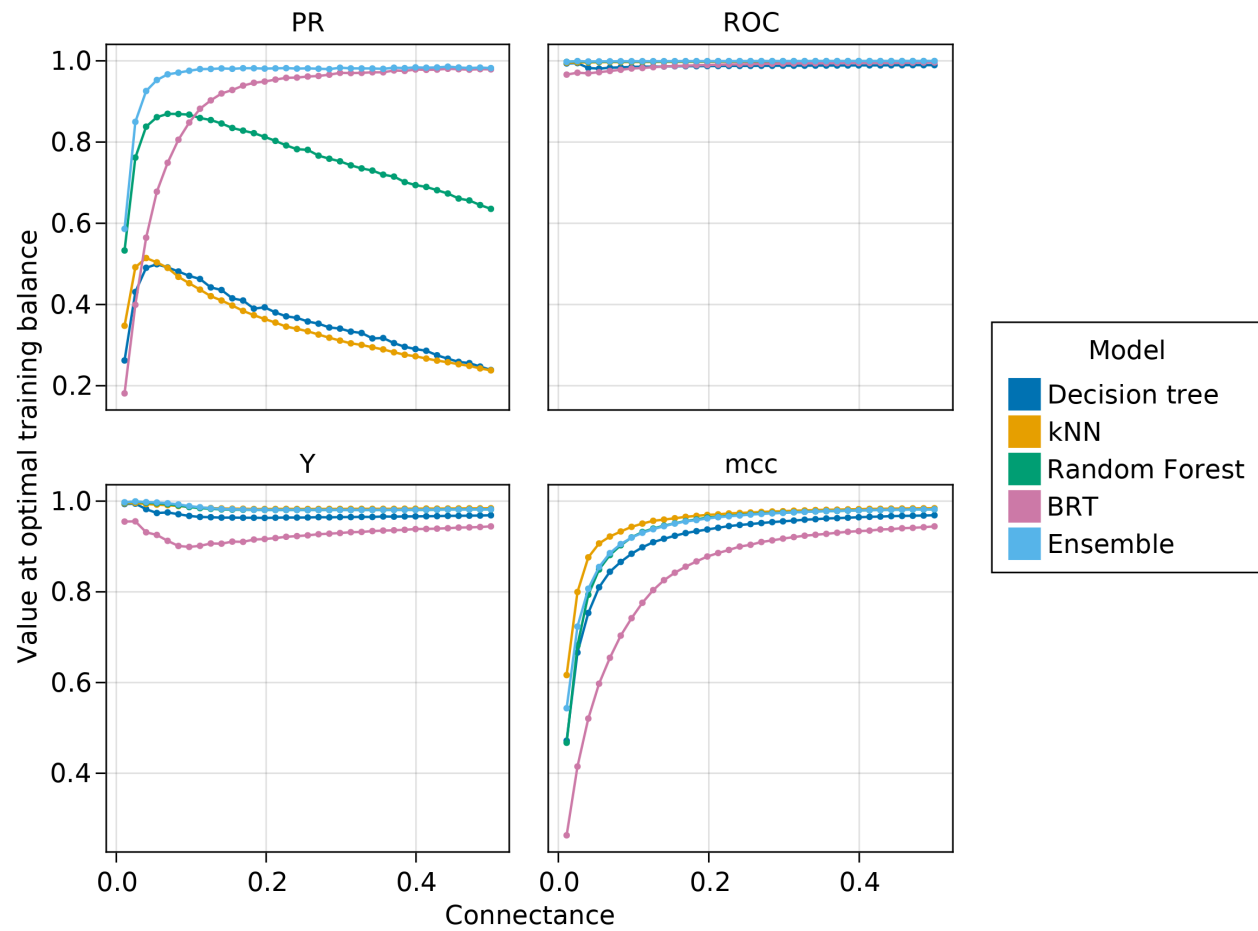


Figure 5: When trained on their optimally biased training set, most models were able to maximize their performance; this is not true for decision tree, k-NN, and to a lower extent RF. The ensemble had a consistently high performance despite incorporating poor models.

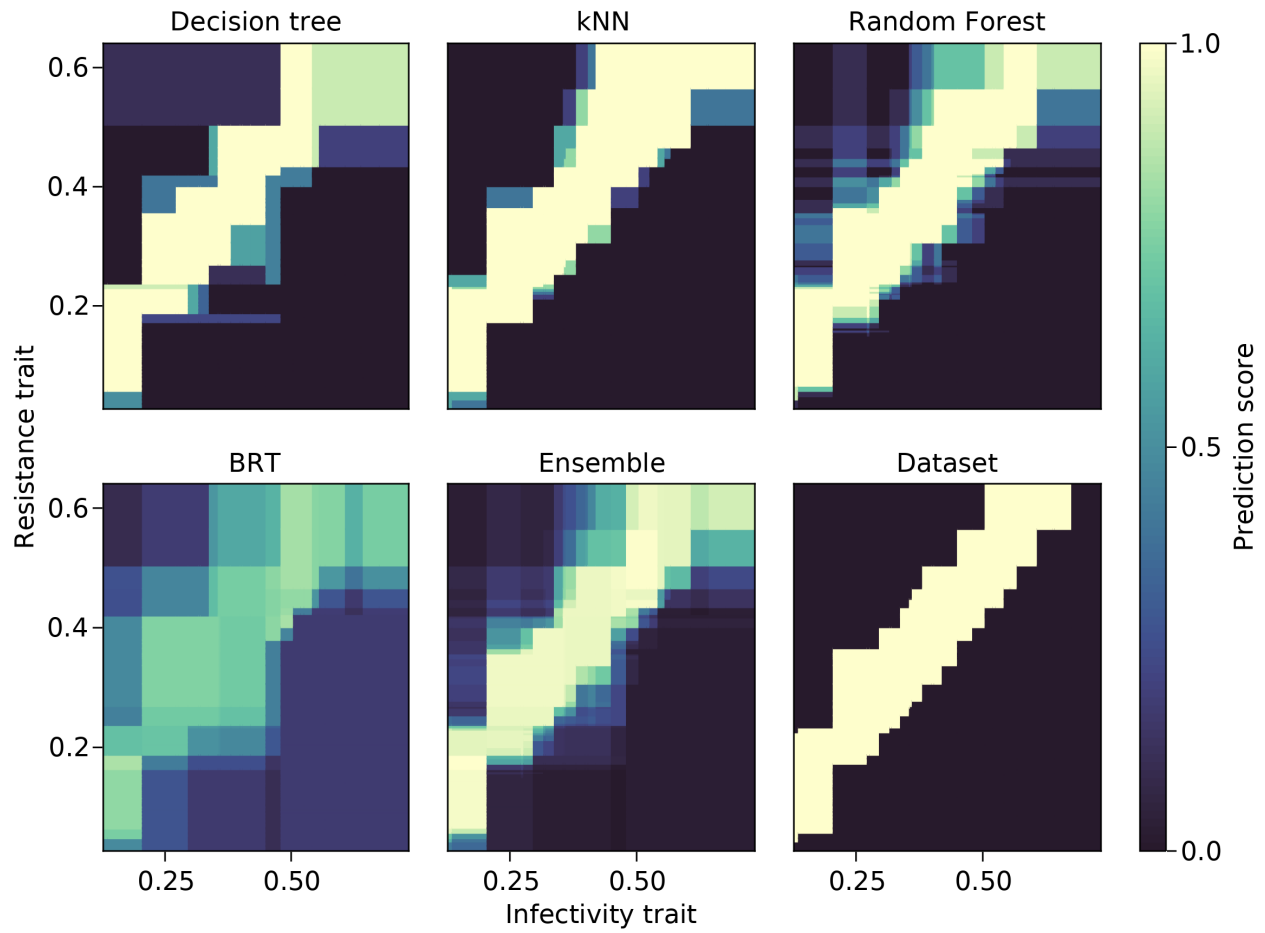


Figure 6: Visualisation of the raw (un-thresholded) models predictions for one instance of a network prediction problem (shown in the “Dataset” panel). Increasing the value of the ξ parameter would make the diagonal structure “broader,” leading to more interactions. A visual inspection of the results is important, as it highlights how some models can “miss” parts of the network; by combining them in an ensemble, these gaps compensate one another, and lead (in this case) to a better prediction.