

Guidelines for the prediction of species interactions through binary classification

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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 fine-tuned 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 $\mathbf{p}^T = [\rho, (1 - \rho)]$. 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 $(1-s)$, which allows to regulate how many predictions are wrong,
 132 under the assumption that the bias is the same (*i.e.* the classifier is as likely to make a false positive or a
 133 false negative). The skill-adjusted confusion matrix is $\mathbf{M} \odot \mathbf{S}$, *i.e.*

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

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

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

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

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

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

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

What are the baseline values of performance measures?

In this section, we will change the values of b , s , and ρ , and report how the main measures discussed in 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 a value of 0 for a no-skill classifier, and are forced towards their correct maximal value when skill changes (*i.e.* a more connected networks will have higher values for a skilled classifier, and lower values for a classifier making mostly mistakes).

[Figure 2 about here.]

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

Numerical experiments on training strategy

In the following section, we will generate random bipartite networks, and train four binary classifiers (as well as an ensemble model using the sum of ranged outputs from the component models) on 50% of the interaction data. In practice, testing usually uses 70% of the total data; for ecological networks, where interactions are sparse *and* the number of species is low, this may not be the best solution, as the testing set becomes constrained not by the *proportion* of interactions, but by their *number*. Preliminary

197 experiments using different splits revealed no qualitative change in the results. Networks are generated by
 198 picking a random infectiousness trait v_i for 100 species (from a beta distribution $B(\alpha = 6, \beta = 8)$
 199 distribution), and a resistance trait h_j for 100 species (from $B(\alpha = 2, \beta = 8)$ distribution). There is an
 200 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
 201 connectance of the network (visual exploration of the parameters show that there is an almost 1:1
 202 relationship between ξ and connectance), and varies uniformly in $[0.05, 0.35]$. This model gives fully
 203 interval networks that are close analogues to the bacteria–phage model of Weitz et al. (2005), with both a
 204 modular structure and a non-uniform degree distribution. This dataset is easy for almost any algorithm to
 205 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
 206 models presented below were able to reach almost perfect predictions all the time (data not presented
 207 here) – this is in part because the rule (there is maximum value of the distance between traits for which
 208 there is an interaction) is fixed for all interactions, and any method able to learn non-linear relationships
 209 should infer it without issues. In order to make the problem more difficult to solve, we use $[v_i, h_j]$ as a
 210 feature vector (*i.e.* the traits on which the models are trained), and therefore the models will have to
 211 uncover that the rule for interaction is $\text{abs}(v_i, h_j) \leq \xi$. The models therefore all have the following form,
 212 where $i_{i,j}$ is an interaction from species i to species j :

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

213 The training sample is composed of 50% of the 10^4 possible entries in the network, *i.e.* $n = 5000$. Out of
 214 these interactions, we pick a proportion ν (the training set balance) to be positive, so that the training set
 215 has νn interactions, and $(1 - \nu)n$ non-interactions. We vary ν uniformly in $]0, 1[$. This allows to evaluate
 216 how the measures of binary classification performance respond to artificially rebalanced dataset for a
 217 given network connectance. The rest of the dataset is used as a testing set, on which all further measures
 218 are calculated. Note that although the training set is balanced arbitrarily, the testing set is assembled so
 219 that it has the exact connectance of the entire network; this ensures that the model is evaluated under the
 220 class imbalance where the predictions will be made, which represents a more meaningful evaluation.

221 Note also that although the simulated networks are bipartite, the algorithms have no “knowledge” of the
222 network structure, and simply look at pairs of species; therefore, the approach outlined here would also
223 work for unipartite networks.

224 The dataset used for numerical experiments is composed of a grid of 35 values of connectance (from 0.011
225 to 0.5) and 35 values of ν (from 0.02 to 0.98); for each pair of values, 500 networks are generated and
226 predicted. For each network, we train four machines: a trait-based k-NN (e.g. Desjardins-Proulx et al.,
227 2017), a regression tree, a regression random forest, and a boosted regression tree. Following results from
228 Pichler et al. (2020), linear models have not been considered (in any way, the relationship in the simulated
229 networks is non-linear). The point of these numerical experiments is *not* to recommend the best model
230 (this is likely problem-specific), but to highlight a series of recommendations that would work for
231 supervised learning tasks. All models were taken from the MLJ.jl package (Blaom et al., 2020; Blaom &
232 Vollmer, 2020) in Julia 1.7 (Bezanson et al., 2017). All machines use the default parameterization; this is
233 an obvious deviation from best practices, as the hyperparameters of any machine require training before
234 its application on a real dataset. As we use 612500 such datasets, this would require over 2 millions unique
235 instances of tweaking the hyperparameters, which is prohibitive from a computing time point of view. An
236 important thing to keep in mind is that the problem we simulate has been designed to be simple to solve:
237 we expect all machines with sensible default parameters to fare well — the results presented in the later
238 sections show that this assumption is warranted, and we further checked that the models do not overfit by
239 ensuring that there is never more than 5% of difference between the accuracy on the training and testing
240 sets. All machines return a quantitative prediction, usually (but not necessarily) in $[0, 1]$, which is
241 proportional (but not necessarily linearly) to the probability of an interaction between i and j . The
242 ROC-AUC and PR-AUC (and therefore the thresholds) can be measured by integrating over the domain of
243 the values return by each machine, but in order to make the average-based ensemble model more
244 meaningful, all predictions are expressed in $[0, 1]$.

245 In order to pick the best confusion matrix for a given trained machine, we performed a thresholding
246 approach using 500 steps on predictions from the testing set, and picking the threshold that maximized
247 Youden’s informedness. During the thresholding step, we measured the area under the receiver operating
248 characteristic (ROC-AUC) and precision-recall (PR-AUC) curves, as measures of overall performance over
249 the range of returned values. We report the ROC-AUC and PR-AUC, as well as a suite of other measures as
250 introduced in the next section, for the best threshold. The ensemble model was generated by summing the

251 predictions of all component models on the testing set (ranged in $[0, 1]$), then put through the same
252 thresholding process. The complete code to run the simulations is available at [10.17605/OSF.IO/JKEWD](https://doi.org/10.17605/OSF.IO/JKEWD).
253 After the simulations were completed, we removed all runs (*i.e.* triples of model, ξ , and ν) for which at
254 least one of the following conditions was met: the accuracy was 0, the true positive or true negative rates
255 were 0, the connectance was larger than 0.25. This removes both the obviously failed model runs, and the
256 networks that are more densely connected compared to the connectance of empirical food webs (and are
257 therefore less difficult to predict, being less imbalanced; preliminary analyses of data with a connectance
258 larger than 0.3 revealed that all machines reached consistently high performance).

259 **Effect of training set balance on performance**

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

275 [Figure 3 about here.]

276 Based on the results presented in fig. 3, it seems that informedness and ROC-AUC are not necessarily able
277 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.

Do better classification accuracy result in more realistic networks?

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

[Figure 6 about here.]

The trained models were then thresholded (again by optimising informedness), and their predictions transformed back into networks for analysis; specifically, we measured the connectance, nestedness (η ; Bastolla et al., 2009), modularity (Q ; Barber, 2007), asymmetry (A ; Delmas et al., 2018), and Jaccard network dissimilarity (Canard et al., 2014). This process was repeated 250 times, and the results are presented in tbl. 1. The k-NN model is an interesting instance here: it produces the network that looks the most like the original dataset, despite having the lowest PR-AUC, suggesting it hits high recall at the cost of low precision. The ensemble was able to reach a very high PR-AUC (and a very high ROC-AUC), which translated into more accurate reconstructions of the structure of the network (with the exception of modularity, which is underestimated by 0.03). This result bears elaborating. Measures of model performance capture how much of the interactions and non-interactions are correctly identified. As long as these predictions are not perfect, some interactions will be predicted at the “wrong” position in the network; these measures cannot describe the structural effect of these mistakes. On the other hand, measures of network structure can have the same value with interactions that fall at drastically different positions; this is in part because a lot of these measures covary with connectance, and in part because as long as these values are not 0 or their respective maximum, there is a large number of network configurations that can have the same value. That ROC-AUC is consistently larger than PR-AUC may be a case of this measure masking models that are not, individually, strong predictors (Jeni et al., 2013). In this

specif example, the combination of individually “adequate” models resulted in an extremely strong ensemble, suggesting that the correct prediction of interactions (as measured by MCC, Inf., ROC-AUC, and PR-AUC) *and* network properties is indeed a feasible task under appropriately hyper-parameterized models.

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
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

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, whereas 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.

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

348 than the one we simulated here may respond in different ways, especially if the rules are fuzzier than the
349 simple rule we used here. In a recent multi-model comparison involving supervised and unsupervised
350 learning, Becker et al. (2022) found that the ensemble was *not* the best model, and was specifically
351 under-performing compared to models using biological traits. This may be because the dataset of Becker
352 et al. (2022) was known to be under-sampled, and so the network itself contained less information than
353 the network and species traits. There is no general conclusion to draw from either these results or ours,
354 besides reinforcing the need to be pragmatic about which models should be included in the ensemble, and
355 whether to use an ensemble at all. In a sense, the surprising performance of the ensemble model should
356 form the basis of the first broad recommendation: optimal training set balance and its interaction with
357 connectance and the specific binary classifier used is, in a sense, an hyperparameter that should be
358 assessed. The distribution of results in fig. 4 and fig. 5 show that there are variations around the trend, and
359 multiple models should probably be trained on their “optimal” training/testing set, as opposed to the same
360 ones.

361 The results presented here highlight an interesting paradox: although the k-NN model was ultimately able
362 to get a correct estimate of network structure (see tbl. 1 and fig. 6), it ultimately remains a poor classifier,
363 as evidenced by its low PR-AUC. This suggests that the goal of predicting *interactions* and predicting
364 *networks* may not always be solvable in the same way – of course a perfect classifier of interactions would
365 make a perfect network prediction; indeed, the best scoring predictor of interactions (the ensemble model)
366 had the best prediction of network structure. The tasks of predicting networks structure and of predicting
367 interactions within networks are essentially two different ones. For some applications (*e.g.* comparison of
368 network structure across gradients), one may care more about a robust estimate of the structure, at the
369 cost at putting some interactions at the wrong place. For other applications (*e.g.* identifying pairs of
370 interacting species), one may conversely care more about getting as many pairs right, even though the
371 mistakes accumulate in the form of a slightly worse estimate of network structure. How these two
372 approaches can be reconciled is something to evaluate on a case-by-case basis, especially since there is no
373 guarantee that an ensemble model will always be the most precise one. Despite this apparent tension at the
374 heart of the predictive exercise, we can use the results presented here to suggest a number of guidelines.

375 First, because we have more trust in reported interactions than in reported absences of interactions (which
376 are overwhelmingly *pseudo*-absences), we can draw on previous literature to recommend informedness as
377 a measure to decide on a threshold for binary classification (Chicco et al., 2021); this being said, because

378 informedness is insensitive to bias (although it is a linear measure of skill), the overall model performance
379 is better evaluated through the use of MCC (figs. 4, 5). Because F_1 is monotonously sensitive to classifier
380 bias fig. 1 and network connectance fig. 2, MCC should be preferred as a measure of model evaluation and
381 comparison. When dealing with multiple models, we therefore suggest to find the optimal threshold using
382 informedness, and to pick the best model using MCC (assuming one does not want to use an ensemble
383 model).

384 Second, accuracy alone should not be the main measure of model performance, but rather an expectation
385 of how well the model should behave given the class balance in the set on which predictions are made;
386 this is because, as derived earlier, the expected accuracy for a no-skill no-bias classifier is $\rho^2 + (1 - \rho)^2$
387 (where ρ is the class balance), which will most often be large. This pitfall is notably illustrated in a recent
388 food-web model (Caron et al., 2022) wherein the authors, using a training set of $n = 10^4$ with only 100
389 positive interactions (representing 0.1% of the total interactions), reached a good accuracy. Reporting a
390 good accuracy is not informative, especially when accuracy isn't (i) compared to the baseline expected
391 value under the given class balance, and (ii) interpreted in the context of a measure that is not sensitive to
392 the chance prediction of many negatives (like MCC).

393 Third, because the PR-AUC responds more to network connectance (fig. 5) and training set imbalance
394 (fig. 4) than ROC-AUC, it should be used as a measure of model performance over the ROC-AUC. This is
395 not to say that ROC-AUC should be discarded (in fact, a low ROC-AUC is undoubtedly a sign of an issue
396 with the model), but that its interpretation should be guided by the PR-AUC value. Specifically, a high
397 ROC-AUC is not informative, as it can be associated to a low PR-AUC (see e.g. Random Forest in tbl. 1)
398 This again echoes recommendations from other fields (Jeni et al., 2013; Saito & Rehmsmeier, 2015). We
399 therefore expect to see high ROC-AUC values, and then to pick the model that maximizes the PR-AUC
400 value. Taken together with the previous two guidelines, we strongly encourage to (i) ensure that accuracy
401 and ROC-AUC are high (in the case of accuracy, higher than expected under no-skill no-bias situation),
402 and (ii) to discuss the performance of the model in terms of the most discriminant measures, i.e. PR-AUC
403 and MCC.

404 Finally, network connectance (i.e. the empirical class imbalance) should inform the composition of the
405 training and testing set, because it is an ecologically relevant value. In the approach outlined here, we treat
406 the class imbalance of the training set as an hyper-parameter, but *test* the model on a set that has the same
407 class imbalance as the actual dataset. This is an important distinction, as it ensure that the prediction

environment matches the testing environment (as we cannot manipulate the connectance of the empirical dataset on which the predictions will be made), and so the values measured on the testing set (or validation set if the data volume allows one to exist) can be directly compared to the values for the actual prediction. A striking result from fig. 4 is that Informedness was almost always maximal at 50/50 balance (regardless of connectance), whereas MCC required *more* positives to be maximized when connectance *increases*, matching the idea that it is a more stringent measure of performance. This has an important consequence in ecological networks, for which the pool of positive cases (interactions) to draw from is typically small: the most parsimonious measure (*i.e.* the one requiring to discard the least amount of interactions to train the model) will give the best validation potential, and in this light is very likely informedness (maximizing informedness is, in fact, the generally accepted default for imbalanced classification regardless of the problem domain; Schisterman et al., 2005). This last result further strengthens the assumption that the amount of bias is an hyper-parameter that must be fine-tuned, as using the wrong bias can lead to models with lower performance; for this reason, it makes sense to not train all models on the same training/testing set, but rather to optimize the set composition for each of them.

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

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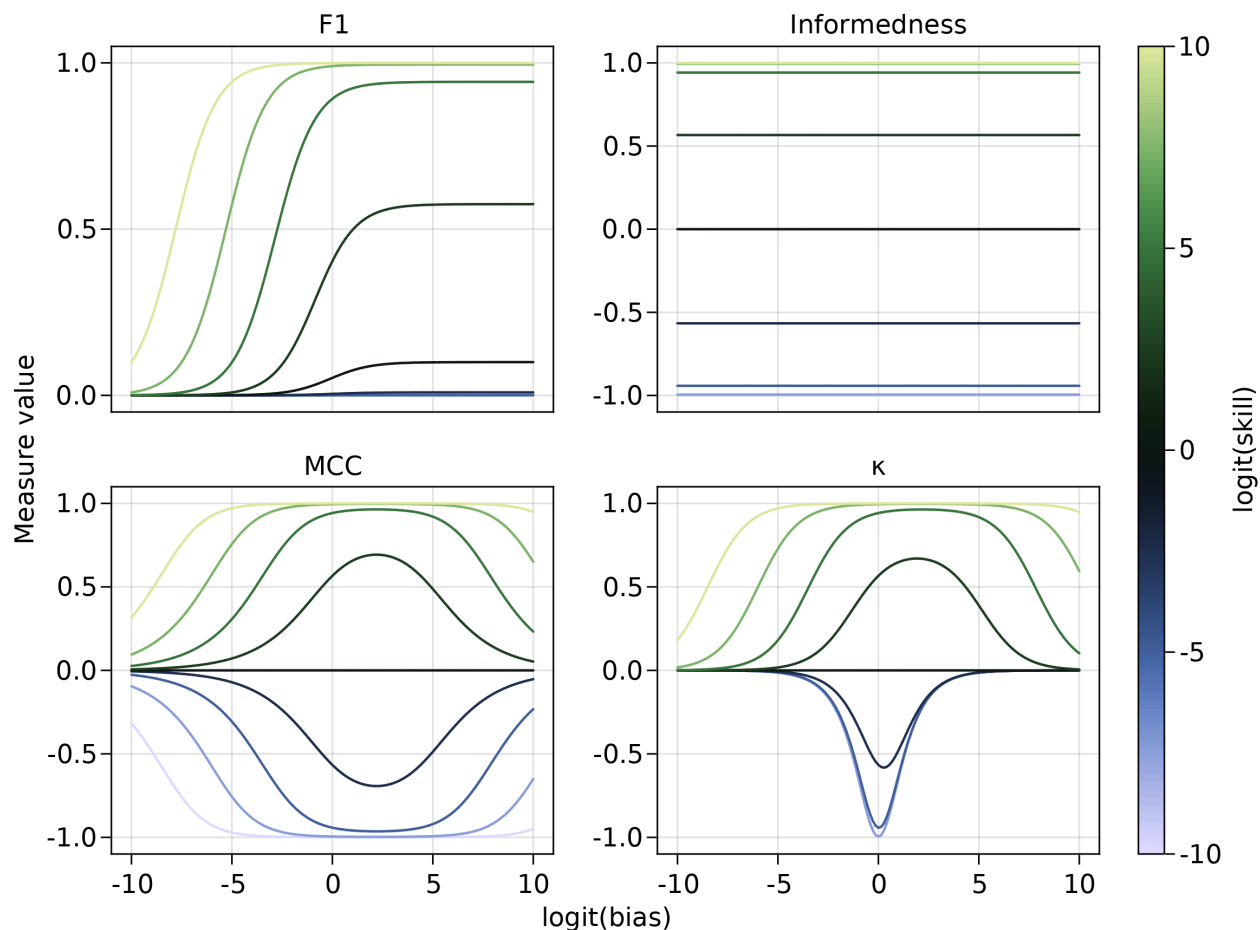


Figure 1: Consequences of changing the classifier skills (s) and bias (b) for a connectance $\rho = 0.15$, on F_1 , informedness, MCC, and κ . Accuracy increases with skill, but also increases when the bias tends towards estimating *fewer* interactions (this follows from the derivations in the text, not shown in the figure). 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.

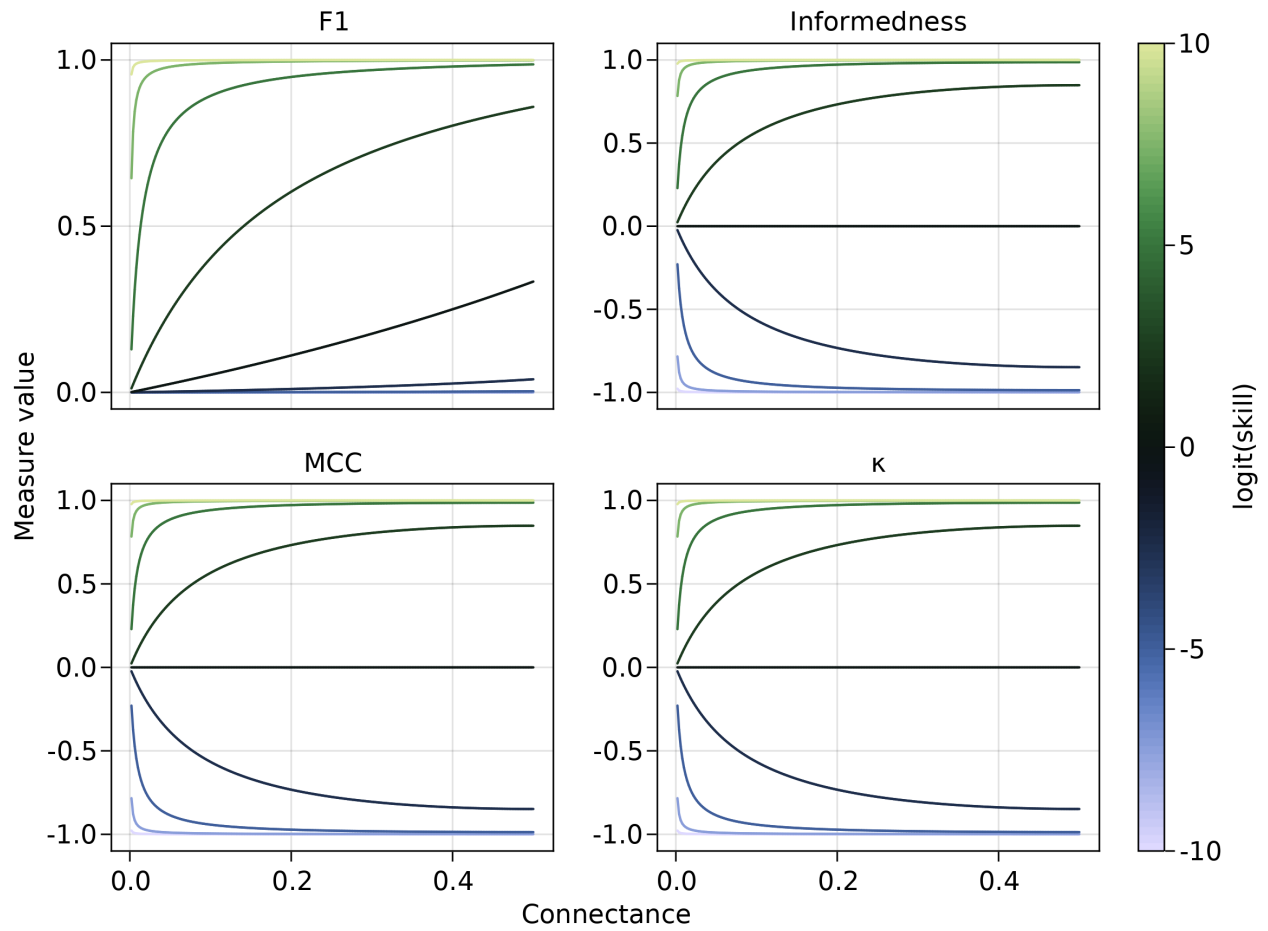


Figure 2: As in fig. 1, consequences of changing connectance for different levels of classifier skill, assuming no classifier bias. Informedness, κ , and MCC do increase with connectance, but only when the classifier is not no-skill; by way of contrast, a more connected network will give a higher F_1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

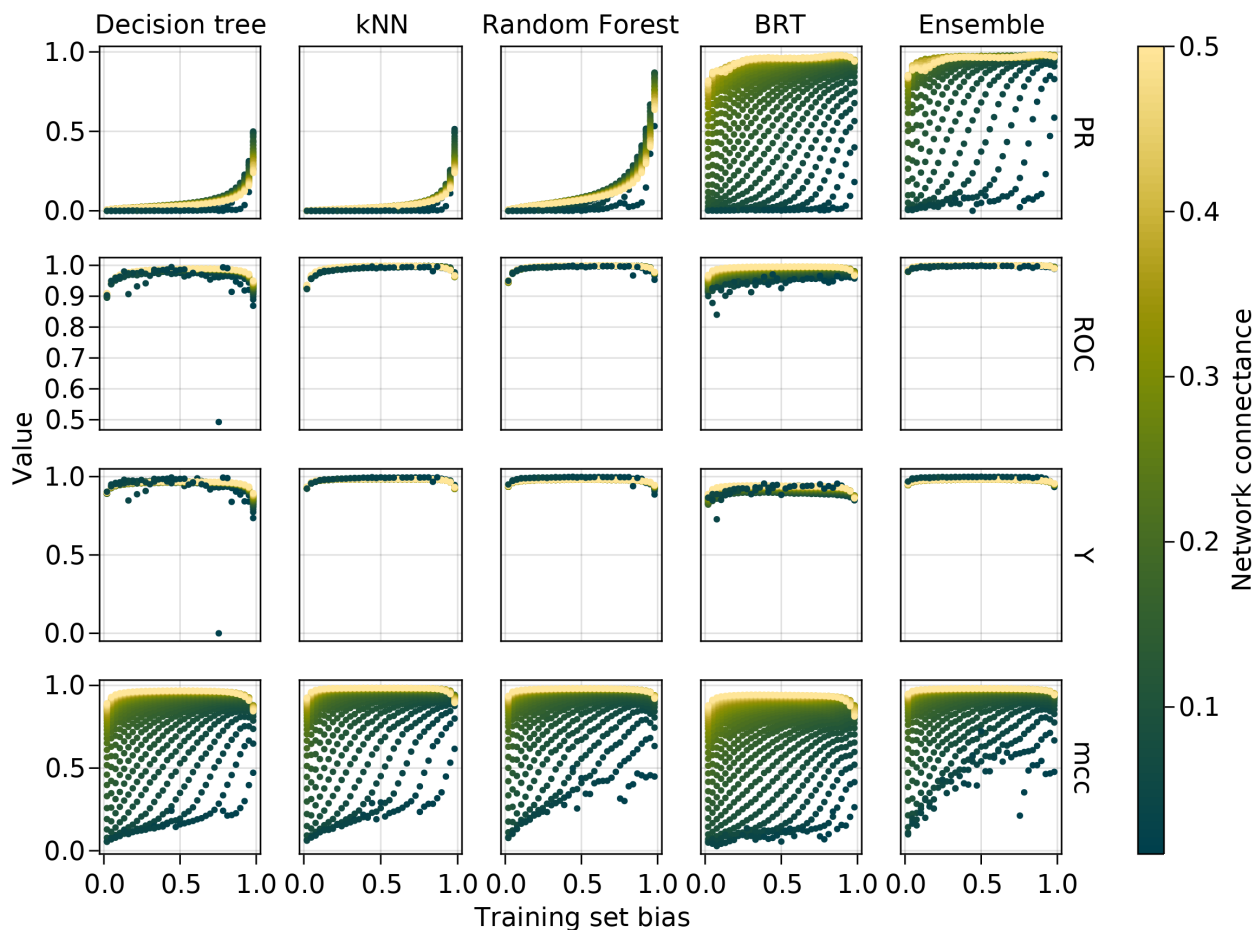


Figure 3: Response of MCC, Informedness, ROC-AUC, and PR-AUC to changes in the training set balance (on the x axis) for a series of increasing connectances (color). All of these values approach 1 for a good model, but should be lower when the prediction is more difficult. Informedness is consistently 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.

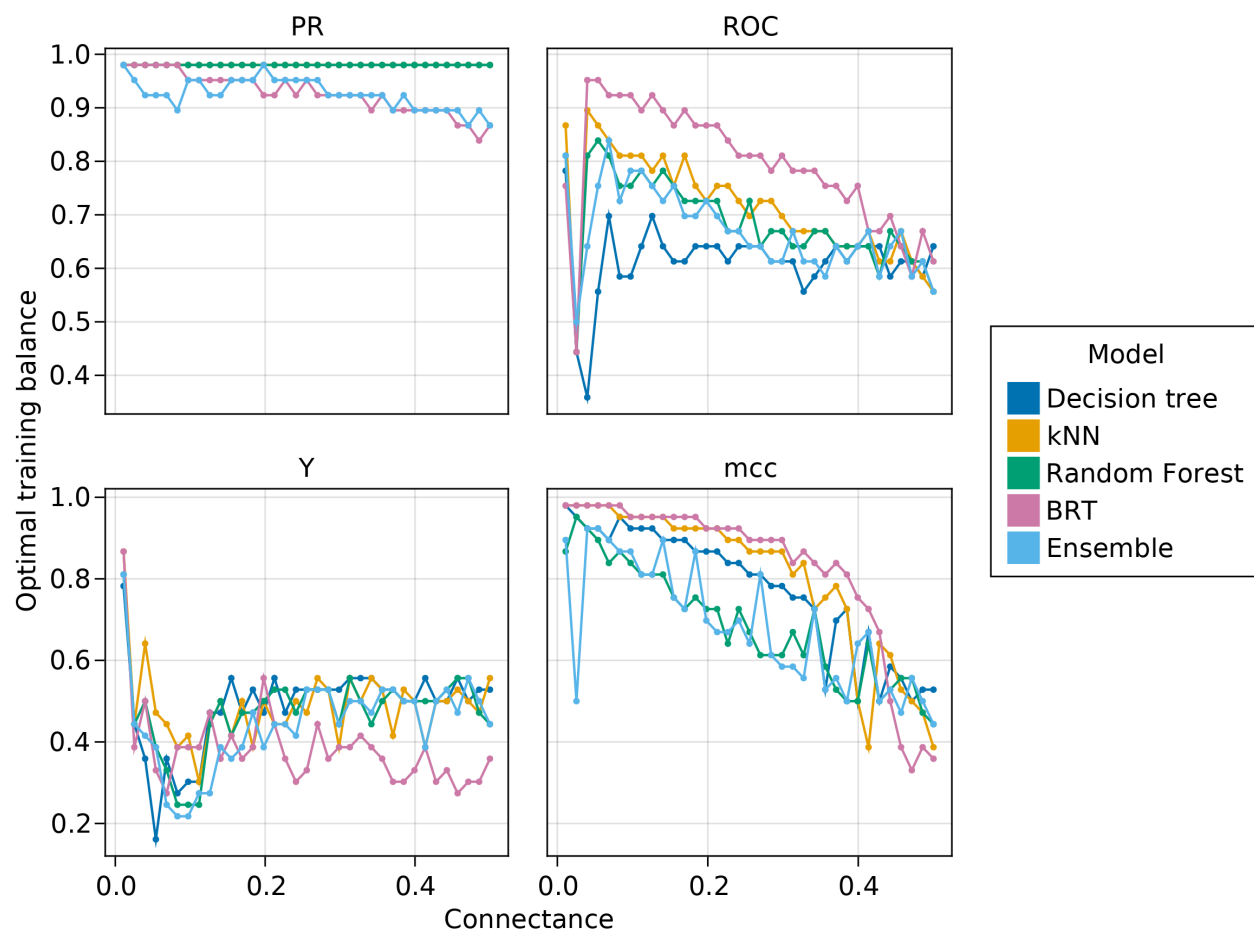


Figure 4: Value of the optimal training set balance for the different models and measures evaluated here, over a range of connectances. Informedness was reliably maximized for balanced training sets, and kept this behavior across models. For other measures, larger connectances in the true network allowed 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.

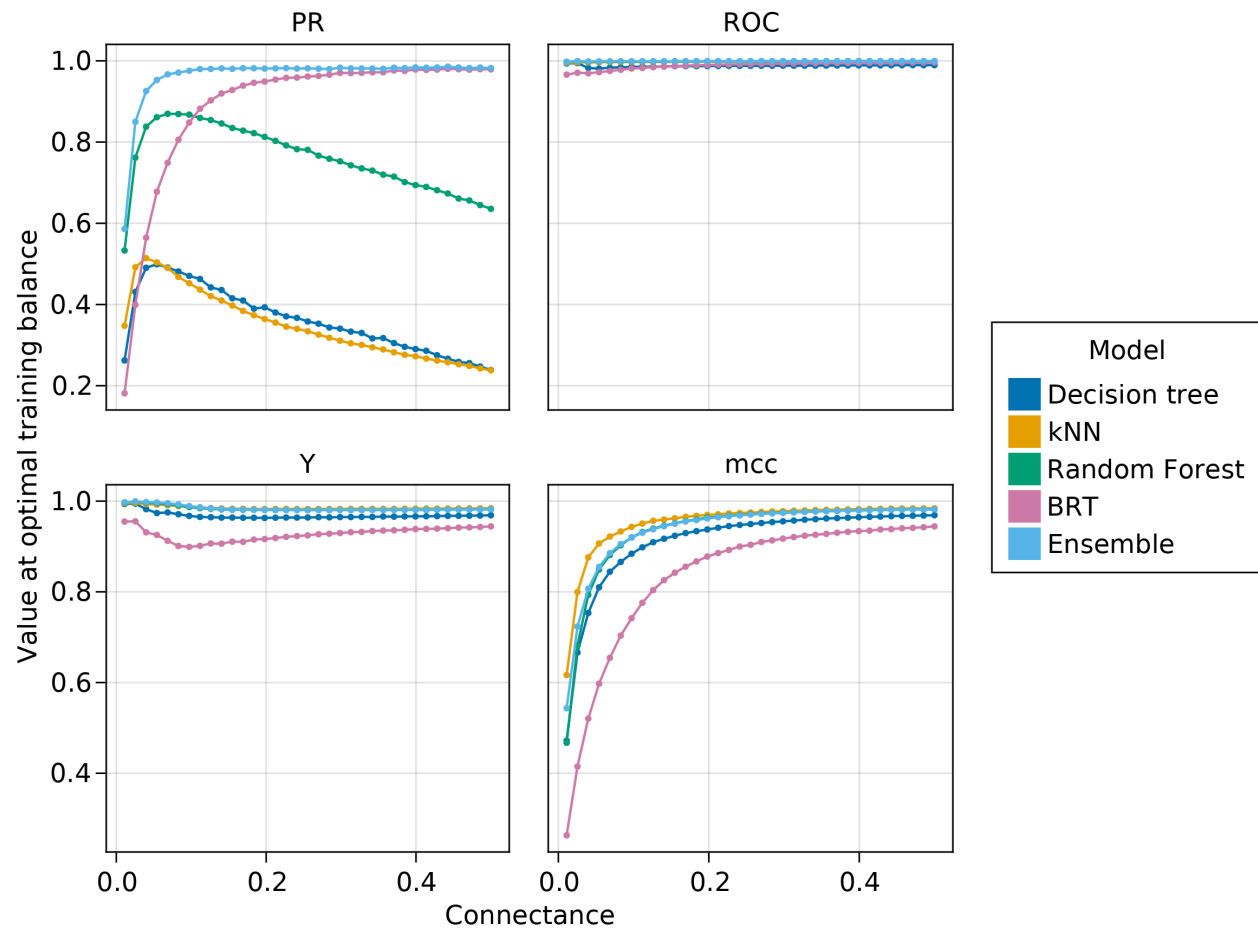


Figure 5: When trained on their optimally biased training set, most models were able to maximize their performance; this is not true when measuring PR-AUC for decision tree, k-NN, and to a lower extent RF. The ensemble had a consistently high performance despite incorporating low-performing models.

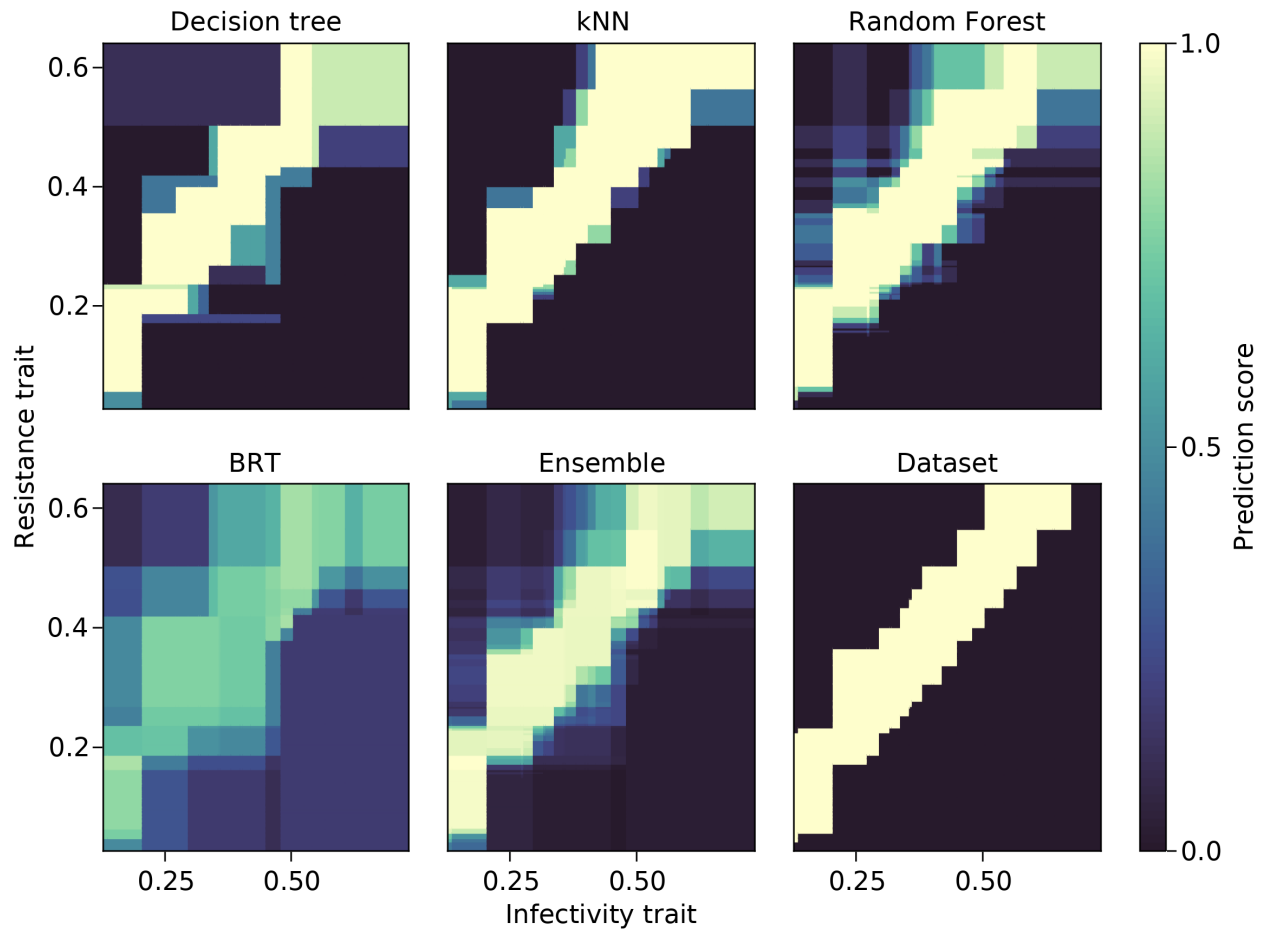


Figure 6: Visualisation of the raw (un-thresholded) models predictions for one instance of a network prediction problem (shown in the “Dataset” panel). Increasing the value of the ξ parameter would make the diagonal structure “broader,” leading to more interactions. A visual inspection of the results 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.