

Forecasting biodiversity in breeding birds using best practices

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Biodiversity forecasts are important for conservation, management, and evaluating how well current models characterize natural systems. While the number of forecasts for biodiversity is increasing, there is little information available on how well these forecasts work. Most biodiversity forecasts are not evaluated to determine how well they predict future diversity, fail to account for uncertainty, and do not use time-series data that captures the actual dynamics being studied. We addressed these limitations by using best practices to explore our ability to forecast the species richness of breeding birds in North America. We used hindcasting to evaluate six different modeling approaches for predicting richness. Hindcasts for each method were evaluated annually for a decade at 1,237 sites distributed throughout the continental United States. All models explained more than 50% of the variance in richness, but none of them consistently outperformed a baseline model that predicted constant richness at each site. The best practices implemented in this study directly influenced the forecasts and evaluations. Stacked species distribution models and "naive" forecasts produced poor estimates of uncertainty and accounting for this resulted in these models dropping in the relative performance compared to other models. Accounting for observer effects improved model performance overall, but also changed the rank ordering of models because it did not improve the accuracy of the "naive" model. Considering the forecast horizon revealed that the prediction accuracy decreased across all models as the time horizon of the forecast increased. To facilitate the rapid improvement of biodiversity forecasts, we emphasize the value of specific best practices in making forecasts and evaluating forecasting methods.

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10 **Abstract**

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12 well current models characterize natural systems. While the number of forecasts for
13 biodiversity is increasing, there is little information available on how well these
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18 birds in North America. We used hindcasting to evaluate six different modeling
19 approaches for predicting richness. Hindcasts for each method were evaluated annually
20 for a decade at 1,237 sites distributed throughout the continental United States. All
21 models explained more than 50% of the variance in richness, but none of them
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23 The best practices implemented in this study directly influenced the forecasts and
24 evaluations. Stacked species distribution models and “naive” forecasts produced poor
25 estimates of uncertainty and accounting for this resulted in these models dropping in the
26 relative performance compared to other models. Accounting for observer effects
27 improved model performance overall, but also changed the rank ordering of models
28 because it did not improve the accuracy of the “naive” model. Considering the forecast
29 horizon revealed that the prediction accuracy decreased across all models as the time
30 horizon of the forecast increased. To facilitate the rapid improvement of biodiversity
31 forecasts, we emphasize the value of specific best practices in making forecasts and
32 evaluating forecasting methods.

33 **Introduction**

34 Forecasting the future state of ecological systems is increasingly important for planning
35 and management, and also for quantitatively evaluating how well ecological models

36 capture the key processes governing natural systems (Clark et al. 2001, Dietze 2017,
37 Houlahan et al. 2017). Forecasts regarding biodiversity are especially important, due to
38 biodiversity's central role in conservation planning and its sensitivity to anthropogenic
39 effects (Cardinale et al. 2012, Díaz et al. 2015, Tilman et al. 2017). High-profile studies
40 forecasting large biodiversity declines over the coming decades ~~have~~ played a large role
41 in shaping ~~ecologists'~~ priorities (as well as those of policymakers; e.g. IPCC 2014), but
42 it is inherently difficult to evaluate such long-term predictions ~~before the projected~~
43 ~~biodiversity declines have occurred.~~

44 Previous efforts to predict future patterns of terrestrial species richness, and diversity
45 more generally, ~~have~~ focused primarily on building species distributions models (SDMs;
46 Thomas et al. 2004, Thuiller et al. 2011, Urban 2015). In general, these models
47 describe individual species' occurrence patterns as functions of the environment. Given
48 forecasts for environmental conditions, these models can predict where each species
49 will occur in the future. These species-level predictions are then combined ("stacked")
50 to generate forecasts for species richness (e.g. Calabrese et al. 2014). Alternatively,
51 models that directly relate spatial patterns of species richness to environment conditions
52 ~~have been developed~~ and generally perform equivalently to stacked SDMs (Algar et al.
53 2009, Distler et al. 2015). This approach is sometimes referred to as "macroecological"
54 modeling, because it models the larger-scale pattern (richness) directly (Distler et al.
55 2015).

56 Despite the emerging interest in forecasting species richness and other aspects of
57 biodiversity (Jetz et al. 2007, Thuiller et al. 2011), little is known about how effectively
58 we can anticipate these dynamics. This is due in part to the long time scales over which
59 many ecological forecasts are applied (and the resulting difficulty in assessing whether
60 the predicted changes occurred; Dietze et al. 2016). What we do know comes from a
61 small number of hindcasting studies, where models are built from different time periods
62 and evaluated on their ability to predict biodiversity patterns in contemporary (Algar et

63 al. 2009, Distler et al. 2015) or historic (Blois et al. 2013, Maguire et al. 2016) periods
64 not used for model fitting. These studies are a valuable first step, but lack several
65 components that are important for developing forecasting models with high predictive
66 accuracy, and for understanding how well different methods can predict the future.
67 These “best practices” for effective forecasting and evaluation (Box 1) broadly involve:
68 1) expanding the use of data to include biological and environmental time-series
69 (Tredennick et al. 2016); 2) accounting for uncertainty in observations and processes,
70 (Yu et al. 2010, Harris 2015); and 3) conducting meaningful evaluations of the forecasts
71 by hindcasting, archiving short-term forecasts, and comparing forecasts to baselines to
72 determine whether the forecasts are more accurate than assuming the system is basically
73 static (Perretti et al. 2013).

74 In this paper, we attempt to forecast the species richness of breeding birds at over 1,200
75 of sites located throughout North America, while following best practices for ecological
76 forecasting (Box 1). To do this, we combine 32 years of time-series data on bird
77 distributions from annual surveys with monthly time-series of climate data and
78 satellite-based remote-sensing. Datasets that span a time scale of 30 years or more have
79 only recently become available for large-scale time-series based forecasting. A dataset
80 of this size allows us to model and assess changes a decade or more into the future in
81 the presence of shifts in environmental conditions on par with predicted climate change.
82 We compare traditional distribution modeling based approaches to spatial models of
83 species richness, time-series methods, and two simple baselines that predict constant
84 richness for each site, on average (Figure 1). All of our forecasting models account for
85 uncertainty and observation error, are evaluated across different time lags using
86 hindcasting, and are publicly archived to allow future assessment. We discuss the
87 implications of these practices for our understanding of, and confidence in, the resulting
88 forecasts, and how we can continue to build on these approaches to improve ecological
89 forecasting in the future.

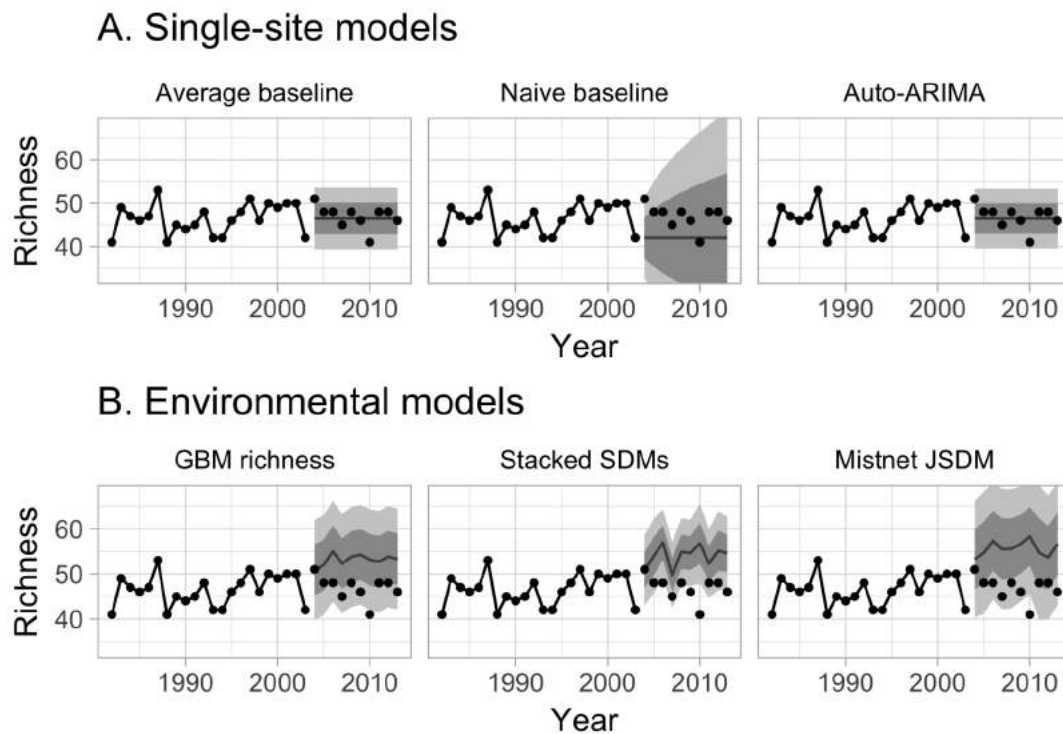




Figure 1: Example predictions from six forecasting models for a single site. Data from 1982 through 2003, connected by solid lines, were used for training the models; the remaining points were used for evaluating the models' forecasts. In each panel, point estimates for each year are shown with lines; the darker ribbon indicates the 68% prediction interval (1 standard deviation of uncertainty), and the lighter ribbon indicates the 95% prediction interval. **A.** Single-site models were trained independently on each site's observed richness values. The first two models ("average" and "naive") served as baselines. **B.** The environmental models were trained to predict richness based on elevation, climate, and NDVI; the environmental models' predictions change from year to year as environmental conditions change.

90 **Methods**

91 We evaluated 6 types of forecasting models (Table 1) by dividing the 32 years of data
92 into 22 years of training data and 10 years of data for evaluating forecasts using
93 hindcasting. Here we use definitions from meteorology, where a hindcast is generally
94 any prediction for an event that has already happened, while forecasts are predictions
95 for actual future events (Jolliffe and Stephenson 2003). We also made long term
96 forecasts by using the full data set for training and making forecasts through the year
97 2050. For both time frames, we made forecasts using each model with and without
98 correcting for observer effects, as described below.


99 **Data**

100 **Richness data.** Bird species richness was obtained from the North American Breeding
101 Bird Survey (BBS) (Pardieck et al. 2017) using the Data Retriever Python package
102 (Morris and White 2013, Senyondo et al. 2017) and rdaretreiver R package (McGlenn
103 et al. 2017). BBS observations are three-minute point counts made at 50 fixed locations
104 along a 40km route. Here we denote each route as a site and summarize richness as the
105 total species observed at all 50 locations in each surveyed year. Prior to summarizing
106 the data was filtered to exclude all nocturnal, **cepuscular**,  aquatic species (since
107 these species are not well sampled by BBS methods; Hurlbert and White 2005), as well
108 as unidentified species, and hybrids. All data from surveys that did not meet BBS
109 quality criteria were also excluded. 

110 We used observed richness values from 1982 (the first year of complete environmental
111 data) to 2003 to train the models, and from 2004 to 2013 to test their performance. We
112 only used BBS routes from the continental United States (i.e. routes where climate data
113 was available PRISM Climate Group (2004)), and we restricted the analysis to routes
114 that were sampled during 70% of the years in the training period (i.e., routes with at
115 least 16 annual observations). The resulting dataset included 34,494 annual surveys of

116 1,279 unique sites, and included 385 species. Site-level richness varied from 8 to 91
117 with an average richness of 51 species.

118 **Past environmental data.** Environmental data included a combination of elevation,
119 bioclimatic variables and a remotely sensed vegetation index (the normalized difference
120 vegetation index; NDVI), all of which are known to influence richness and distribution
121 in the BBS data (Kent et al. 2014). For each year in the dataset, we used the 4 km
122 resolution PRISM data (PRISM Climate Group 2004) to calculate eight bioclimatic
123 variables identified as relevant to bird distributions (Harris 2015): mean diurnal range,
124 isothermality, max temperature of the warmest month, mean temperature of the wettest
125 quarter, mean temperature of the driest quarter, precipitation seasonality, precipitation
126 of the wettest quarter, and precipitation of the warmest quarter. These variables were
127 calculated for the 12 months leading up to the annual survey (July-June) as opposed to
128 the calendar year. Satellite-derived NDVI, a primary correlate of richness in BBS data
129 (Hurlbert and Haskell 2002), was obtained from the NDIV3g dataset with an 8 km
130 resolution (Pinzon and Tucker 2014) and was available from 1981-2013. Average
131 summer (April, May, June) and winter (December, January, February) NDVI values
132 were used as predictors. Elevation was from the SRTM 90m elevation dataset (Jarvis et
133 al. 2008) obtained using the R package raster (Hijmans 2016). Because BBS routes are
134 40-km transects rather than point counts, we used the average value of each
135 environmental variable within a 40 km radius of each BBS route's starting point.

136 **Future environmental projections.** In addition to the analyses presented here, we
137 ~~have~~ also generated and archived long term forecasts from 2014-2050. This will allow
138 future researchers to assess the performance of our six models on longer time horizons
139 as more years of BBS data become available. Precipitation and temperature were
140 forecast using the CMIP5 multi-model ensemble dataset (Brekke et al. 2013). 
141 downscaled model runs (Brekke et al. 2013, see Table S1) using the RCP6.0 scenario
142 were averaged together to create a single ensemble used to calculate the bioclimatic

143 variables for North America. For NDVI, we used the per-site average values from
144 2000-2013 as a simple forecast. For observer effects (see below), each site was set to
145 ~~have~~ zero observer bias. The predictions ~~have been~~ archived at (Harris et al. 2017b).

146 **Accounting for observer effects**

147 Observer effects are inherent in large data sets collected by different observers, and are
148 known to occur in BBS (Sauer et al. 1994). For each forecasting approach, we trained
149 two versions of the corresponding model: one with corrections for differences among
150 observers, and one without (Figure 2). We estimated the observer effects (and
151 associated uncertainty about those effects) using a linear mixed model, with observer as
152 a random effect, built in the Stan probabilistic programming language (Carpenter et al.
153 2017). Because observer and site are strongly related (observers tend to repeatedly
154 sample the same site), site-level random effects were included to ensure that inferred
155 deviations were actually observer-related (as opposed to being related to the sites that a
156 given observer happened to see). The resulting model is described mathematically and
157 with code in Supplement S1. The model partitions the variance in observed richness
158 values into site-level variance, observer-level variance, and residual variance
159 (e.g. variation within a site from year to year).

160 Across our six modeling approaches (described below), we used estimates from the
161 observer model in three different ways. First, the expected values for site-level richness
162 were used directly as our “average” baseline model (see below). For the two models that
163 made species-level predictions, the estimated observer effects were included alongside
164 the environmental variables as predictors. Finally, we trained the remaining models to
165 predict observer-corrected richness values (i.e. observed richness minus the observer
166 effect, or the number of species that would have been recorded by a “typical” observer).
167 Since the site-level and observer-level random effects are not known precisely, we
168 represented the range of possible values using 500 Monte Carlo samples from the

169 posterior distribution over these effects. Each downstream model was then trained 500
 170 times using different possible values for the random effects.

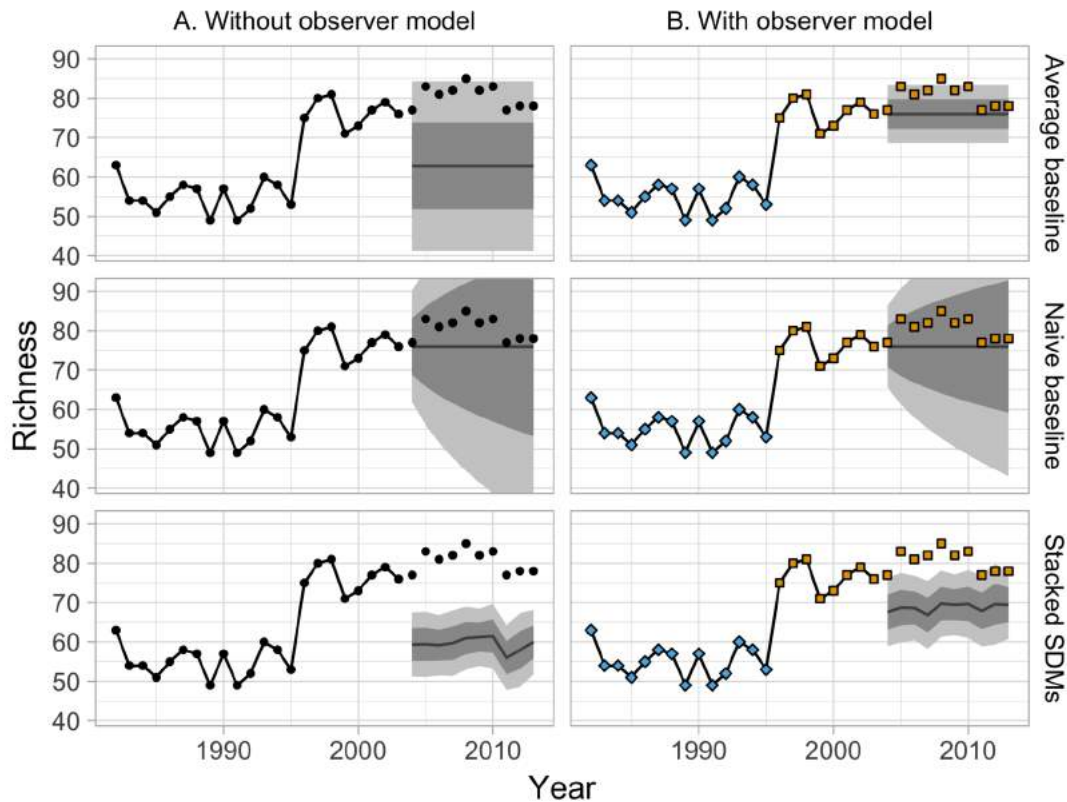


Figure 2: **A.** Model predictions for Pennsylvania route 35 when all observers are treated the same (black points). **B.** Model predictions for the same route when accounting for systematic differences between observers (represented by the points' colors). In this example most models are made more robust to observer turnover by including an observer model. Note that the “naive” model is less sensitive to observer turnover, and does not benefit as much from modeling it.

171 **Models: site-level models**

172 Three of the models used in this study were fit to each site separately, with no
 173 environmental information (Table 1). These models were fit to each BBS route twice:
 174 once using the residuals from the observer model, and once using the raw richness
 175 values. When correcting for observer effects, we averaged across 500 models that were
 176 fit separately to the 500 Monte Carlo estimates of the observer effects, to account for

Table 1: Six forecasting models. Single-site models were trained site-by-site, without environmental data. Environmental models were trained at the continental scale, using only environmental variables (as opposed to site or time series information) as predictors. Most of the models were trained to predict richness directly. This mirrors the standard application of these techniques. Separate random forest SDMs were fit for each species and used to predict the probability of that species occurring at each site. The species-level probabilities at a site were summed to predict richness. The mistnet JSMD was trained to predict the full species composition at each site, and the number of species in its predictions was used as an estimate of richness.

Model	Response variable	Predictors		
		Site id	Time	Environment
Single-site models				
Average baseline	richness	✓		
Naive baseline	richness	✓	✓	
Auto-ARIMA	richness	✓	✓	
Environmental models				
GBM richness	richness			✓
Stacked SDMs	species-level presence			✓
Mistnet JSMD	species composition			✓

177 our uncertainty in the true values of those effects. All of these models use a Gaussian
 178 error distribution (rather than a count distribution) for reasons discussed below (see
 179 “Model evaluation”).

180 **Baseline models.** We used two simple baseline models as a basis for comparison with
 181 the more complex models (Figure 2A). The first baseline, called the “average” model,
 182 treated site-level richness observations as uncorrelated noise around a site-level
 183 constant:

$$y_t = \mu + \epsilon_t.$$

184 Predictions from the “average” model are thus centered on μ , which could either be the
 185 mean of the raw training richness values, or an output from the observer model. ~~This~~
 186 ~~model’s confidence intervals have a constant width~~ that depends on the standard

187 deviation of ϵ , which can either be the standard deviation of the raw training richness
188 values, or σ^{residual} from the observer model; see supplement).

189 The second baseline, called the “naive” model (Hyndman and Athanasopoulos 2014),
190 was a simple autoregressive process with a single year of history, i.e. an ARIMA(0,1,0)
191 model:

$$y_t = y_{t-1} + \epsilon_t,$$

192 where the standard deviation of ϵ is a free parameter for each site. In contrast to the
193 “average” model, whose predictions are based on the average richness across the whole
194 time series, the “naive” model predicts that future observations will be similar to the
195 *final* observed value (e.g., in our hindcasts the value observed in 2003). Moreover,
196 because the ϵ values accumulate over time, the confidence intervals expand rapidly as
197 the predictions extend farther into the future. Despite these differences, both models’
198 richness predictions are centered on a constant value, so neither model can anticipate
199 any trends in richness or any responses to future environmental changes.

200 **Time series models.** We used Auto-ARIMA models (based on the `auto.arima`
201 function in the package `forecast`; Hyndman 2017) to represent an array of different
202 time-series modeling approaches. These models can include an autoregressive
203 component (as in the “naive” model, but with the possibility of longer-term
204 dependencies in the underlying process), a moving average component (where the noise
205 ~~can have serial autocorrelation~~) and an integration/differencing component (so that the
206 analysis could be performed on sequential differences of the raw data, accommodating
207 more complex patterns including trends). The `auto.arima` function chooses whether
208 to include each of these components (and how many terms to include for each one)
209 using AICc (Hyndman 2017). Since there is no seasonal component to the BBS
210 time-series, we did not include a season component in these models. Otherwise we used
211 the default settings for this function (See supplement for details).

212 **Models: environmental models**

213 In contrast to the single-site models, most attempts to predict species richness focus on
214 using correlative models based on environmental variables. We tested three common
215 variants of this approach: direct modeling of species richness; stacking individual
216 species distribution models; and joint species distribution models (JSDMs). Following
217 the standard approach, site-level random effects were not included in these models as
218 predictors, meaning that this approach implicitly assumes that two sites with identical
219 Bioclim, elevation, and NDVI values ~~should~~ have identical richness distributions. As
220 above, we included observer effects and the associated uncertainty by running these
221 models 500 times (once per MCMC sample).

222 **“Macroecological” model: richness GBM.** We used a boosted regression tree model
223 using the `gbm` package (Ridgeway *et al.* 2017) to directly model species richness as a
224 function of environmental variables. Boosted regression trees are a form of tree-based
225 modeling that work by fitting thousands of small tree-structured models sequentially,
226 with each tree optimized to reduce the error of its predecessors. They are flexible
227 models that are considered well suited for prediction (Elith *et al.* 2008). This model was
228 optimized using a Gaussian likelihood, with a maximum interaction depth of 5,
229 shrinkage of 0.015, and up to 10,000 trees. The number of trees used for prediction was
230 selected using the “out of bag” estimator; this number averaged 6,700 for the
231 non-observer data and 7,800 for the observer-corrected data.

232 **Species Distribution Model: stacked random forests.** Species distribution models
233 (SDMs) predict individual species’ occurrence probabilities using environmental
234 variables. Species-level models are used to predict richness by summing the predicted
235 probability of occupancy across all species at a site. This avoids known problems with
236 the use of thresholds for determining whether or not a species will be present at a site
237 (Pellissier *et al.* 2013, Calabrese *et al.* 2014). Following Calabrese *et al.* (2014), we
238 calculated the uncertainty in our richness estimate by treating richness as a sum over

239 independent Bernoulli random variables: $\sigma_{richness}^2 = \sum_i p_i(1 - p_i)$, where i indexes
240 species. By itself, this approach is known to underestimate the true community-level
241 uncertainty because it ignores the uncertainty in the species-level probabilities
242 (Calabrese et al. 2014). To mitigate this problem, we used an ensemble of 500 estimates
243 for each of the species-level probabilities instead of just one, propagating the
244 uncertainty forward. We obtained these estimates using random forests (Liaw and
245 Wiener 2002), a common approach in the species distribution modeling literature.
246 Random forests are constructed by fitting hundreds of independent regression trees to
247 randomly-perturbed versions of the data (Cutler et al. 2007, Caruana et al. 2008). When
248 correcting for observer effects, each of the 500 trees in our species-level random forests
249 used a different Monte Carlo estimate of the observer effects as a predictor variable.

250 **Joint Species Distribution Model: mistnet.** Joint species distribution models
251 (JSDMs) are a new approach that makes predictions about the full composition of a
252 community instead of modeling each species independently as above (Warton et al.
253 2015). JSDMs remove the assumed independence among species and explicitly account
254 for the possibility that a site will be much more (or less) suitable for birds in general (or
255 particular groups of birds) than one would expect based on the available environmental
256 measurements alone. As a result, JSDMs do a better job of representing uncertainty
257 about richness than stacked SDMs (Harris 2015, Warton et al. 2015). We used the
258 `mistnet` package (Harris 2015) because it is the only JSDM that describes species'
259 environmental associations with nonlinear functions.

260 **Model evaluation**

261 We defined model performance for all models in terms of continuous Gaussian errors,
262 instead of using discrete count distributions. Variance in species richness within sites
263 was lower than predicted by several common count models, such as the Poisson or
264 binomial (i.e. richness was underdispersed for individual sites), ~~so these count models~~

265 ~~would have had difficulty fitting the data~~ (cf. Calabrese et al. 2014). The use of a
266 continuous distribution is adequate here, since richness had a relatively large mean (51)
267 and all models produce continuous richness estimates. When a model was run multiple
268 times for the purpose of correcting for observer effects, we used the mean of those runs'
269 point estimates as our final point estimate and we calculated the uncertainty using the
270 law of total variance (i.e. $\text{Var}(\bar{y}) + \mathbb{E} [\text{Var}(y)]$), or the variance in point estimates plus
271 the average residual variance).

272 We evaluated each model's forecasts using the data for each year between 2004 and
273 2013. We used three metrics for evaluating performance: 1) root-mean-square error
274 (RMSE) to determine how far, on average, the models' predictions were from the
275 observed value; 2) the 95% prediction interval coverage to determine how well the
276 models predicted the range of possible outcomes; and 3) deviance (i.e. negative 2 times
277 the Gaussian log-likelihood) as an integrative measure of fit that incorporates both
278 accuracy and uncertainty. In addition to evaluating forecast performance in general, we
279 evaluated how performance changed as the time horizon of forecasting increased by
280 plotting performance metrics against year. Finally, we decomposed each model's
281 squared error into two components: the squared error associated with site-level means
282 and the squared error associated with annual fluctuations in richness within a site. This
283 decomposition describes the extent to which each model's error depends on consistent
284 differences among sites versus changes in site-level richness from year to year.

285 All analyses were conducted using R (R Core Team 2017). Primary R packages used in
286 the analysis included dplyr (Wickham et al. 2017), tidyr (Wickham 2017), gimms
287 (Detsch 2016), sp (Pebesma and Bivand 2005, Bivand et al. 2013), raster (Hijmans
288 2016), prism (PRISM Climate Group 2004), rdataretriever (McGlinn et al. 2017),
289 forecast (Hyndman and Khandakar 2008, Hyndman 2017), git2r (Widgren and others
290 2016), ggplot (Wickham 2009), mistnet (Harris 2015), viridis (Garnier 2017), rstan
291 (Stan Development Team 2016), yaml (Stephens 2016), purrr (Henry and Wickham

292 2017), gbm (Ridgeway *et al.* 2017), randomForest (Liaw and Wiener 2002). Code to
293 fully reproduce this analysis is available on GitHub
294 (<https://github.com/weecology/bbs-forecasting>) and archived on Zenodo (Harris et al.
295 2017a).

296 **Results**

297 The site-observer mixed model found that 70% of the variance in richness in the
298 training set could be explained by differences among sites, and 21% could be explained
299 by differences among observers. The remaining 9% represents residual variation, where
300 a given observer might report a different number of species in different years. In the
301 training set, the residuals had a standard deviation of about 3.6 species. After correcting
302 for observer differences, there was little temporal autocorrelation in these residuals
303 (i.e. the residuals in one year explain 1.3% of the variance in the residuals of the
304 following year), suggesting that richness was approximately stationary between 1982
305 and 2003.

306 When comparing forecasts for richness across sites all methods performed well (Figure
307 3; all $R^2 > 0.5$). However SDMs (both stacked and joint) and the macroecological
308 model all failed to successfully forecast the highest-richness sites, resulting in a notable
309 clustering of predicted values near ~60 species and the poorest model performance
310 ($R^2=0.52-0.78$, versus $R^2=0.67-0.87$ for the within-site methods).

311 While all models generally performed well in absolute terms (Figure 3), none
312 consistently outperformed the “average” baseline (Figure 4). The auto-ARIMA was
313 generally the best-performing non-baseline model, but in many cases (67% of the time),
314 the auto.arima procedure selected a model with only an intercept term (i.e. no
315 autoregressive terms, no drift, and no moving average terms), making it similar to the
316 “average” model. All five alternatives to the “average” model achieved lower error on
317 some of the sites in some years, but each one had a higher mean absolute error and

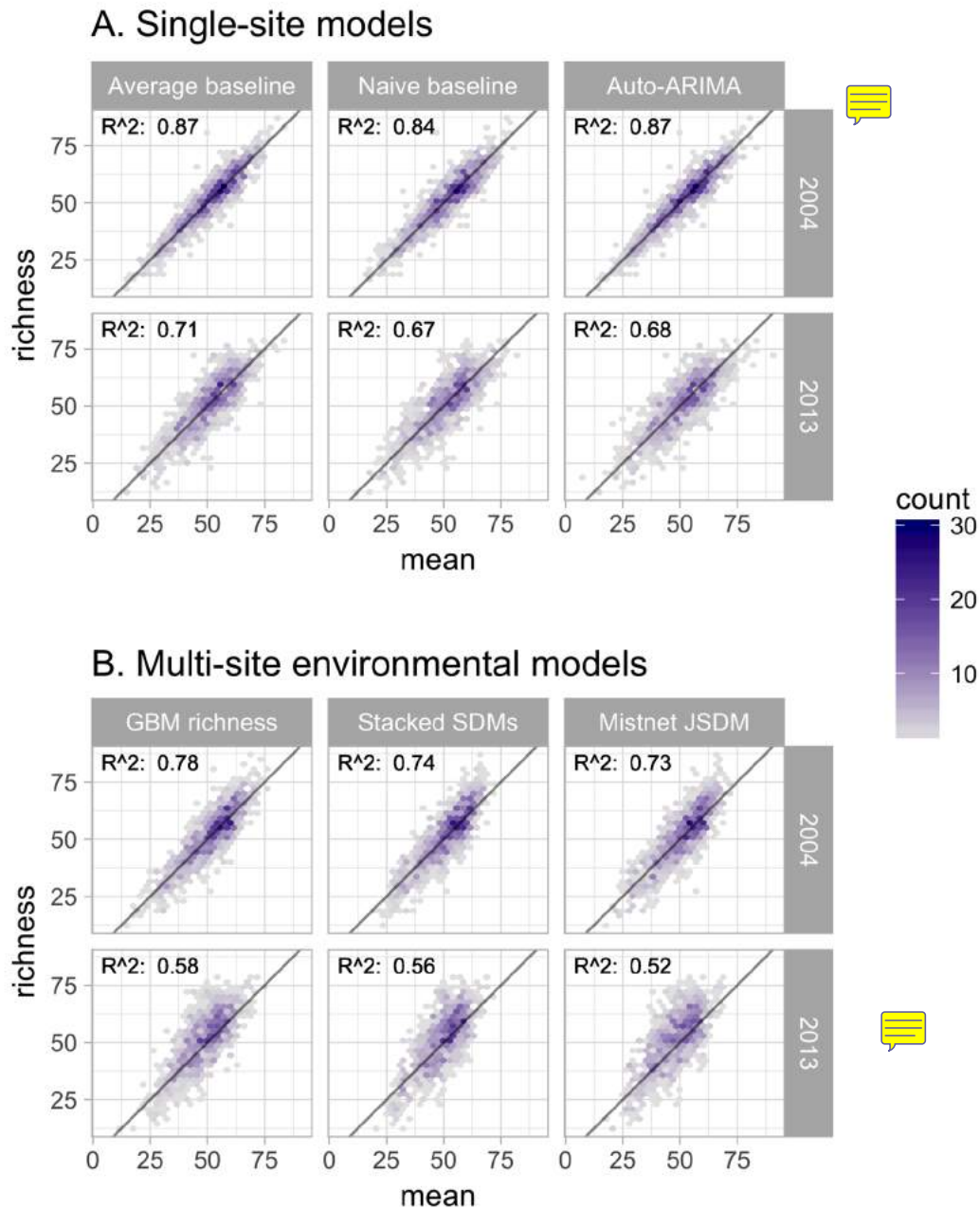


Figure 3: Performance of six forecasting models for predicting species richness one year (2004) and ten years into the future (2013). Plots show observed vs. predicted values for species richness. Models were trained with data from 1982-2003. In general, the single-site models (**A**) outperformed the environmental models (**B**). The accuracy of the predictions generally declined as the timescale of the forecast was extended from 2004 to 2013.

318 higher mean deviance (Figure 4).

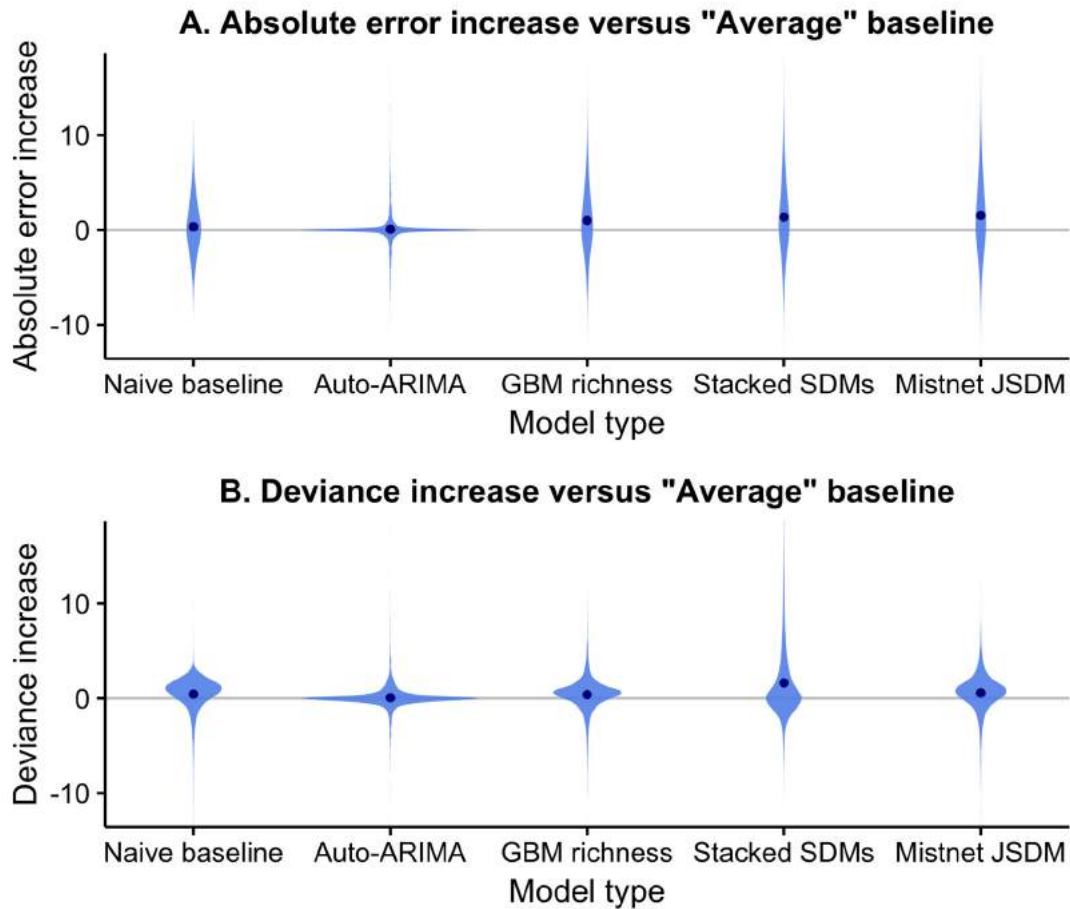


Figure 4: Difference between the forecast error of models and the error of the average baseline using both absolute error (A.) and deviance (B.). Differences are taken for each site and testing year so that errors for the same forecast are directly compared. The error of the average baseline is by definition zero and is indicated by the horizontal gray line. None of the five models provided a consistent improvement over the average baseline. The absolute error of the models was generally similar or larger than that of the “average” model, with large outliers in both directions. The deviance of the models was also generally higher than the “average” baseline.

319 Most models produced confidence intervals that were too narrow, indicating
 320 overconfident predictions (Figure 5C). The random forest-based SDM stack was the
 321 most overconfident model, with only 72% of observations falling inside its 95%
 322 confidence intervals. This stacked SDM’s narrow predictive distribution caused it to
 323 have notably higher deviance (Figure 5B) than the next-worst model, even though its
 324 point estimates were not unusually bad in terms of RMSE (5A). As discussed elsewhere

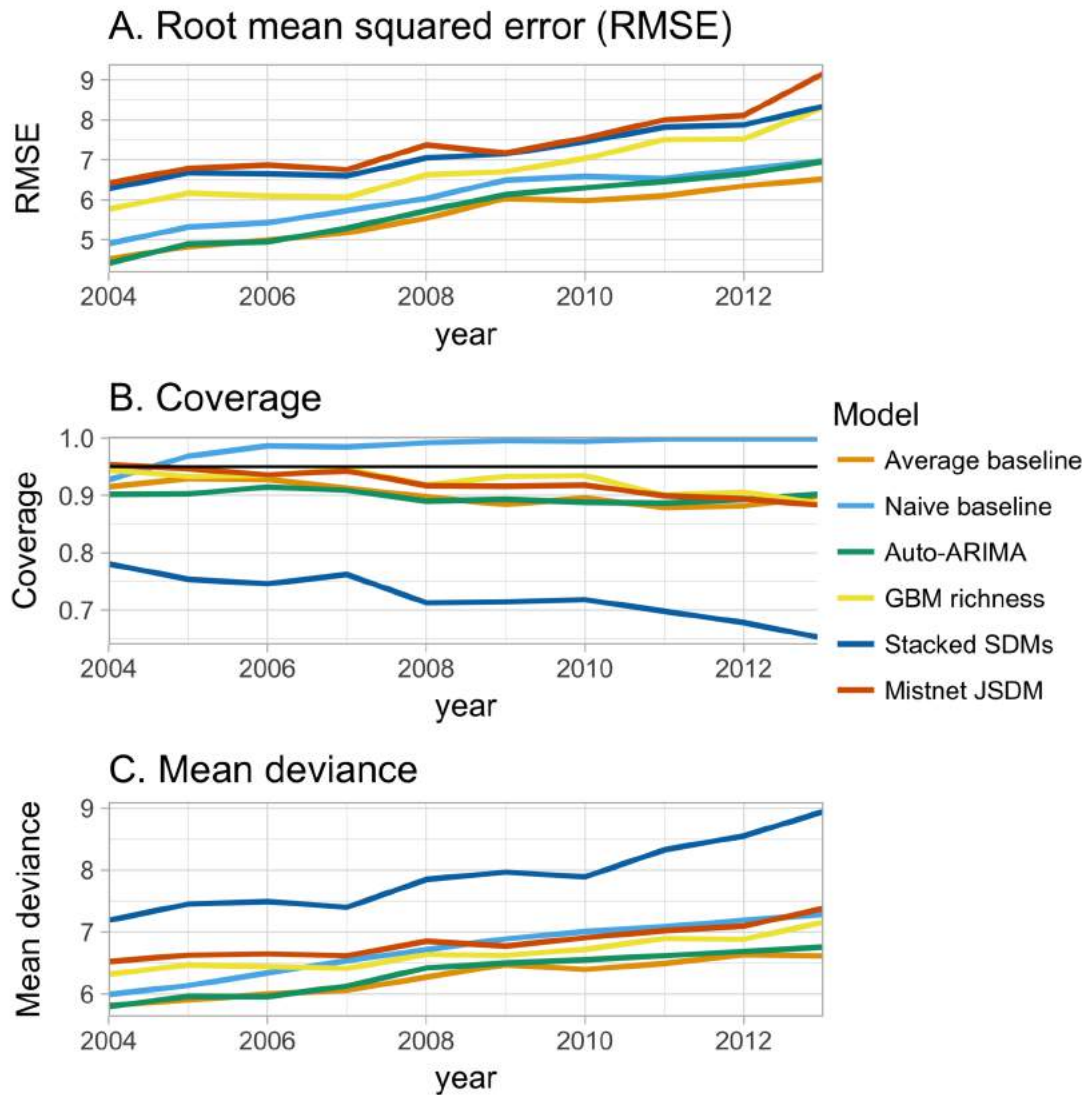


Figure 5: Change in performance of the six forecasting models with the time scale of the forecast (1-10 years into the future). **A.** Root mean square error (rmse; the error in the point estimates) shows the three environmental models tending to show the largest errors at all time scales and the models getting worse as they forecast further into the future at approximately the same rate. **B.** Deviance (lack of fit of the entire predictive distribution) shows the stacked species distribution models with much higher error than other models and shows that the “naive” model’s deviance grows relatively quickly. **C.** Coverage of a model’s 95% confidence intervals (how often the observed values fall inside the predicted range; the black line indicates ideal performance) shows that the “naive” model’s predictive distribution is too wide (capturing almost all of the data) and the stacked SDM’s predictive distribution is too narrow (missing almost a third of the observed richness values by 2014).

325 (Harris 2015), this overconfidence is a product of the assumption in stacked SDMs that
326 errors in the species-level predictions are independent. The GBM-based
327 “macroecological” model and the mistnet JSDM had the best calibrated uncertainty
328 estimates (Figure 5B) and therefore their relative performance was higher in terms of
329 deviance than in terms of RMSE. The “naive” model was the only model whose
330 confidence intervals were too wide (Figure 5C), which can be attributed to the rapid rate
331 at which these intervals expand (Figure 1).

332 Partitioning each model’s squared error shows that the majority of the residual error was
333 attributed to errors in estimating site-level means, rather than errors in tracking
334 year-to-year fluctuations (Figure 6). The “average” model, which was based entirely on
335 site-level means, had the lowest error in this regard. In contrast, the three environmental
336 models showed larger biases at the site level, though they still explained most of the
337 variance in this component. This makes sense, given that they could not explicitly
338 distinguish among sites with similar climate, NDVI, and elevation. Interestingly, the
339 environmental models had higher squared error than the baselines did for tracking
340 year-to-year fluctuations in richness as well.

341 Accounting for differences among observers generally improved measures of model fit
342 (Figure 7). Improvements primarily resulted from a small number of forecasts where
343 observer turnover caused a large shift in the reported richness values. The naive
344 baseline was less sensitive to these shifts, because it largely ignored the richness values
345 reported by observers that had retired by the end of the training period (Figure 1). The
346 average model, which gave equal weight to observations from the whole training period,
347 showed a larger decline in performance when not accounting for observer effects –
348 especially in terms of coverage. The performance of the mistnet JSDM was notable
349 here, because its prediction intervals retained good coverage even when not correcting
350 for observer differences, which we attribute to the JSDM’s ability to model this
351 variation with its latent variables.

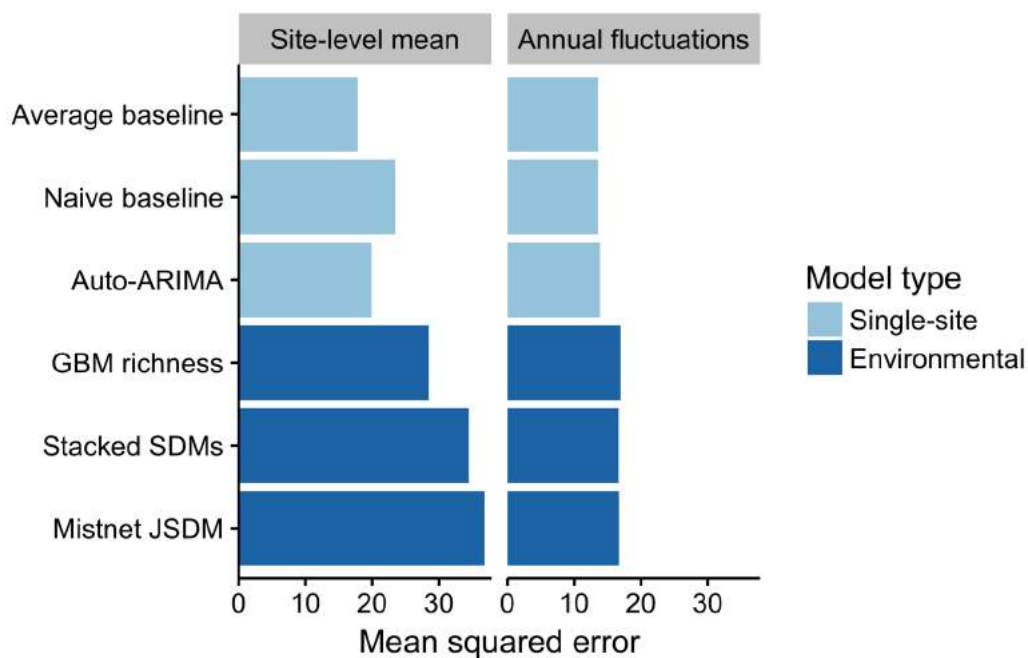


Figure 6: Partitioning of the squared error for each model into site and year components. The site-level mean component shows consistent over or under estimates of richness at a site across years. The annual fluctuation component shows errors in predicting fluctuations in a site's richness over time. Both components of the mean squared error were lower for the single-site models than for the environmental models.

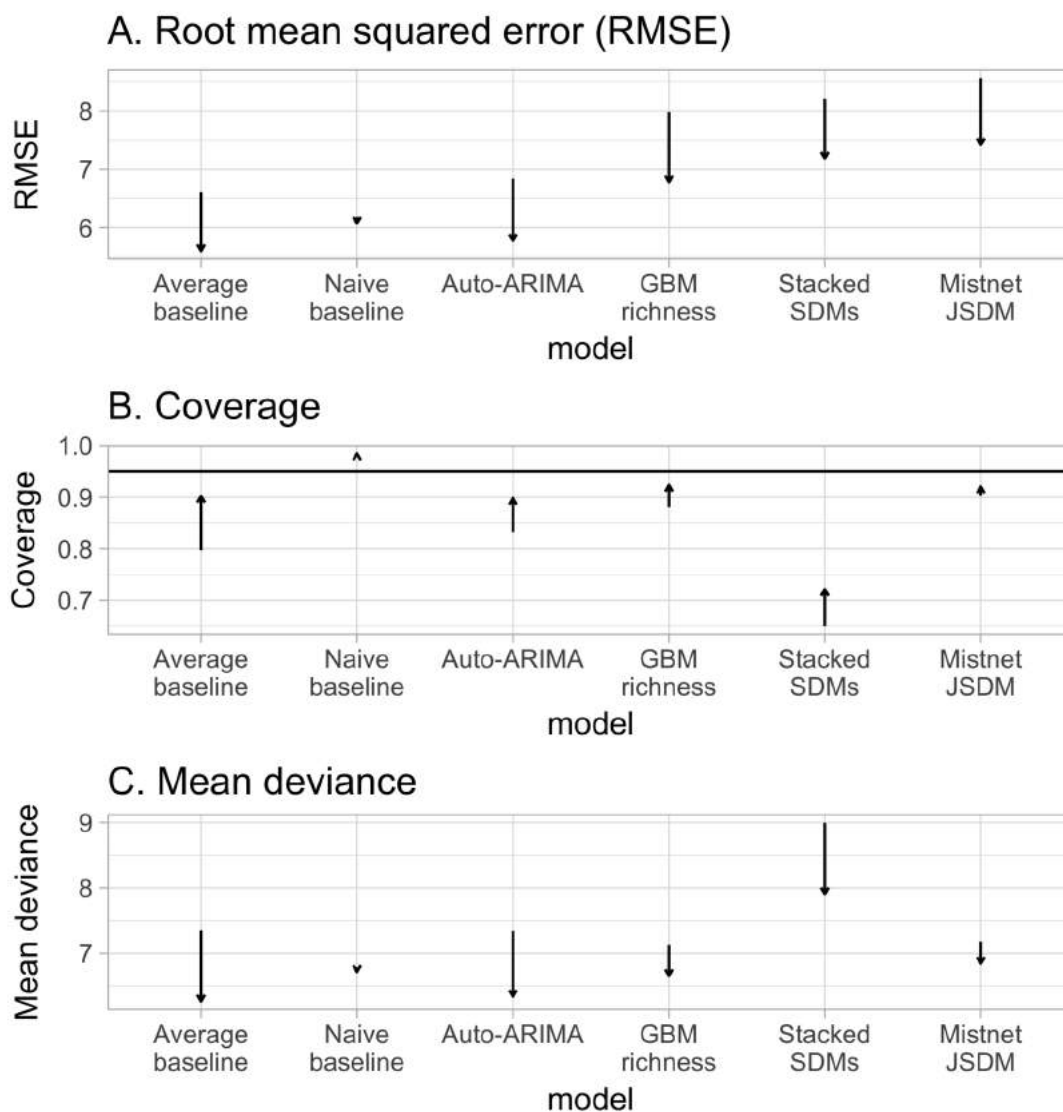


Figure 7: Controlling for differences among observers generally improved each model's predictions, on average. The magnitude of this effect was negligible for the Naive baseline, however.

352 Discussion

353 Forecasting is an emerging imperative in ecology; as such, the field needs to develop
354 and follow best practices for conducting and evaluating ecological forecasts (Clark et al.
355 2001). We ~~have~~ used a number of these practices (Box 1) in a single study that builds
356 and evaluates forecasts of biodiversity in the form of species richness. The results of
357 this effort are both promising and humbling. When comparing predictions across sites,
358 many different approaches produce reasonable forecasts (Figure 3). If a site is predicted
359 to have ~~a high number of~~ species in the future, relative to other sites, it generally does.
360 However, none of the methods evaluated reliably determined how site-level richness
361 changes over time (Figure 6), which is generally the stated purpose of these forecasts.
362 As a result, baseline models, which did not attempt to anticipate changes in richness
363 over time, generally provided the best forecasts for future biodiversity. While this study
364 is restricted to breeding birds in North America, its results are consistent with a growing
365 literature on the limits of ecological forecasting, as discussed below.

366 The most commonly used methods for forecasting future biodiversity, SDMs and
367 macroecological models, both produced worse forecasts than time-series models and
368 simple baselines. This weakness suggests that predictions about future biodiversity
369 change should be viewed with skepticism unless the underlying models ~~have been~~
370 validated temporally, via hindcasting and comparison with simple baselines. Since
371 site-level richness is relatively stable, spatial validation is not enough: a model can ~~have~~
372 ~~high accuracy across spatial gradients without being able to predict changes over time.~~
373 This gap between spatial and temporal accuracy is ~~known to be~~ important for
374 species-level predictions (Rapacciuolo et al. 2012, Oedekoven et al. 2017); our results
375 indicate that it is substantial for higher-level patterns like richness as well. **SDMs' poor**
376 **temporal predictions are particularly sobering, as these models have been one of the**
377 **main foundations for estimates of the predicted loss of biodiversity to climate change**
378 **over the past decade or so** (Thomas et al. 2004, Thuiller et al. 2011, Urban 2015). Our

379 results also highlight the importance of comparing multiple modeling approaches when
380 conducting ecological forecasts, and in particular, the value of comparing results to
381 simple baselines to avoid over-interpreting the information present in these forecasts
382 [Box 1]. Disciplines ~~that have~~ more mature forecasting cultures often do this by
383 reporting “forecast skill”, i.e., the improvement in the forecast relative to a simple
384 baseline (Jolliffe and Stephenson 2003). We recommend following the example of
385 Perretti et al. (2013) and adopting this approach in future ecological forecasting
386 research.

387 When comparing different methods for forecasting our results demonstrate the
388 importance of considering uncertainty (Box 1; Clark et al. 2001, Dietze et al. 2016).
389 Previous comparisons between stacked SDMs and macroecological models reported
390 that the methods yielded equivalent results for forecasting diversity (Algar et al. 2009,
391 Distler et al. 2015). While our results support this equivalence for point estimates, they
392 also show that stacked SDMs dramatically underestimate the range of possible
393 outcomes; after ten years, more than a third of the observed richness values fell outside
394 the stacked SDMs’ 95% prediction intervals. Consistent with Harris (2015) and Warton
395 et al. (2015), we found that JSMDs’ wider prediction intervals enabled them to avoid
396 this problem. Macroecological models appear to share this advantage, while being
397 considerably easier to implement.

398 We ~~have~~ only evaluated annual forecasts up to a decade into the future, but forecasts are
399 often made with a lead time of 50 years or more. These long-term forecasts are difficult
400 to evaluate given the small number of century-scale datasets, but are important for
401 understanding changes in biodiversity at some of the lead times relevant for
402 conservation and management. Two studies ~~have~~ assessed models of species richness at
403 longer lead times (Algar et al. 2009, Distler et al. 2015), but the results were not
404 compared to baseline or time-series models (in part due to data limitations) making
405 them difficult to compare to our results directly. Studies on shorter time scales, such as

406 ours, provide one way to evaluate our forecasting methods without having to wait
407 several decades to observe the effects of environmental change on biodiversity (Petchey
408 et al. 2015, Dietze et al. 2016, Tredennick et al. 2016), but cannot fully replace
409 longer-term evaluations (Tredennick et al. 2016). In general, drivers of species richness
410 can differ at different temporal scales (Rosenzweig 1995, White 2004, 2007, Blonder et
411 al. 2017), so different methods may perform better for different lead times. In particular,
412 we might expect environmental and ecological information to become more important
413 at longer time scales, and thus for the performance of simple baseline forecasts to
414 degrade faster than forecasts from SDMs and other similar models. We did observe a
415 small trend in this direction: deviance for the auto-ARIMA models and for the average
416 baseline grew faster than for two of the environmental models (the JSMD and the
417 macroecological model), although this growth was not statistically significant for the
418 average baseline.

419 While it is possible that models that include species' relationships to their environments
420 or direct environmental constraints on richness will provide better fits at longer lead
421 times, it is also possible that they will continue to produce forecasts that are worse than
422 baselines that assume the systems are static. This would be expected to occur if richness
423 in these systems is not changing over the relevant multi-decadal time scales, which
424 would make simpler models with no directional change more appropriate. Recent
425 suggestions that local scale richness in some systems is not changing directionally at
426 multi-decadal scales supports this possibility (Brown et al. 2001, Ernest and Brown
427 2001, Vellend et al. 2013, Dornelas et al. 2014). A lack of change in richness may be
428 expected even in the presence of substantial changes in environmental conditions and
429 species composition at a site due to replacement of species from the regional pool
430 (Brown et al. 2001, Ernest and Brown 2001). On average, the Breeding Bird Survey
431 sites used in this study show little change in richness (site-level SD of 3.6 species, after
432 controlling for differences among observers; see also La Sorte and Boecklen 2005). The
433 absence of rapid change in this dataset is beneficial for the absolute accuracy of

434 forecasts across different sites: when a past year's richness is already known, it is easy
435 to estimate future richness. Ward et al. (2014) found similar patterns in time series of
436 fisheries stocks, where relatively stable time series were best predicted by simple
437 models and more complex models were only beneficial with dynamic time series. The
438 site-level stability of the BBS data also explains why SDMs and macroecological
439 models perform relatively well at predicting future richness, despite failing to capture
440 changes in richness over time.

441 The relatively stable nature of the BBS richness time-series also makes it difficult to
442 improve forecasts relative to simple baselines, since those baselines are already close to
443 representing what is actually occurring in the system. It is possible that in systems
444 exhibiting directional changes in richness and other biodiversity measures that models
445 based on spatial patterns may yield better forecasts. Future research in this area should
446 determine if regions or time periods exhibiting strong directional changes in
447 biodiversity are better predicted by these models and also extend our forecast horizon
448 analyses to longer timescales where possible. Our results also suggest that future efforts
449 to understand and forecast biodiversity should incorporate species composition, since
450 lower-level processes are expected to be more dynamic (Ernest and Brown 2001,
451 Dornelas et al. 2014) and contain more information about how the systems are changing
452 (Harris 2015). More generally, determining the forecastability of different aspects of
453 ecological systems under different conditions is an important next step for the future of
454 ecological forecasting.

455 Future biodiversity forecasting efforts also need to address the uncertainty introduced
456 by the error in forecasting the environmental conditions that are used as predictor
457 variables. In this, and other hindcasting studies, the environmental conditions for the
458 "future" are known because the data has already been observed. However, in real
459 forecasts the environmental conditions themselves ~~have to~~ be predicted, and
460 environmental forecasts will also have uncertainty and bias. Ultimately, ecological

461 forecasts that use environmental data will therefore be more uncertain than our current
462 hindcasting efforts, and it is important to correctly incorporate this uncertainty into our
463 models (Clark et al. 2001, Dietze 2017). Limitations in forecasting future
464 environmental conditions—particularly at small scales—will present continued
465 challenges for models incorporating environmental variables, and this may result in a
466 continued advantage for simple single-site approaches.

467 In addition to comparing and improving the process models used for forecasting it is
468 important to consider the observation models. When working with any ecological
469 dataset, there are imperfections in the sampling process that have the potential to
470 influence results. With large scale surveys and citizen science datasets, such as the
471 Breeding Bird Survey, these issues are potentially magnified by the large number of
472 different observers and by major differences in the habitats and species being surveyed
473 (Sauer et al. 1994). Accounting for differences in observers reduced the average error in
474 our point estimates and also improved the coverage of the confidence intervals. In
475 addition, controlling for observer effects resulted in changes in which models performed
476 best, most notably improving most models' point estimates relative to the naive baseline.
477 This demonstrates that modeling observation error can be important for properly
478 estimating and reducing uncertainty in forecasts and can also lead to changes in the best
479 methods for forecasting [Box 1]. This suggests that, prior to accounting for observer
480 effects, the naive model performed well largely because it was capable of
481 accommodating rapid shifts in estimated richness introduced by changes in the observer.
482 These kinds of rapid changes were difficult for the other single-site models to
483 accommodate. Another key aspect of an ideal observation model is imperfect detection.
484 In this study, we did not address differences in detection probability across species and
485 sites (Boulinier et al. 1998) since there is no clear way to address this issue using North
486 American Breeding Bird Survey data without making strong assumptions about the data
487 (i.e., assuming there is no biological variation in stops along a route; White and Hurlbert
488 2010), but this would be a valuable addition to future forecasting models.

489 The science of forecasting biodiversity remains in its infancy and it is important to
490 consider weaknesses in current forecasting methods in that context. In the beginning,
491 weather forecasts were also worse than simple baselines, but these forecasts have
492 continually improved throughout the history of the field (McGill 2012, Silver 2012,
493 Bauer et al. 2015). One practice that lead to improvements in weather forecasts was that
494 large numbers of forecasts were made publicly, allowing different approaches to be
495 regularly assessed and refined (McGill 2012, Silver 2012). To facilitate this kind of
496 improvement, it is important for ecologists to start regularly making and evaluating real
497 ecological forecasts, even if they perform poorly, and to make these forecasts openly
498 available for assessment (McGill 2012, Dietze et al. 2016). These forecasts should
499 include both short-term predictions, which can be assessed quickly, and mid- to
500 long-term forecasts, which can help ecologists to assess long time-scale processes and
501 determine how far into the future we can successfully forecast (Dietze et al. 2016,
502 Tredennick et al. 2016). We ~~have~~ openly archived forecasts from all six models through
503 the year 2050 (Harris et al. 2017b), so that we and others can assess how well they
504 perform. We plan to evaluate these forecasts and report the results as each new year of
505 BBS data becomes available, and make iterative improvements to the forecasting
506 models in response to these assessments.

507 Making successful ecological forecasts will be challenging. Ecological systems are
508 complex, our fundamental theory is less refined than for simpler physical and chemical
509 systems, and we currently lack the scale of data that often produces effective forecasts
510 through machine learning. Despite this, we believe that progress can be made if we
511 develop an active forecasting culture in ecology that builds and assesses forecasts in
512 ways that will allow us to improve the effectiveness of ecological forecasts more rapidly
513 (Box 1; McGill 2012, Dietze et al. 2016). This includes expanding the scope of the
514 ecological and environmental data we work with, paying attention to uncertainty in both
515 model building and forecast evaluation, and rigorously assessing forecasts using a
516 combination of hindcasting, archived forecasts, and comparisons to simple baselines.

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525 the clarity of this manuscript.

526 **Box 1: Best practices for making and evaluating ecological forecasts**

527 **1. Compare multiple modeling approaches**

528 Typically ecological forecasts use one modeling approach or a small number of related
529 approaches. By fitting and evaluating multiple modeling approaches we can learn more
530 rapidly about the best approaches for making predictions for a given ecological quantity
531 (Clark et al. 2001, Ward et al. 2014). This includes comparing process-based (e.g.,
532 Kearney and Porter 2009) and data-driven models (e.g., Ward et al. 2014), as well as
533 comparing the accuracy of forecasts to simple baselines to determine if the modeled
534 forecasts are more accurate than the naive assumption that the world is static (Jolliffe
535 and Stephenson 2003, Perretti et al. 2013).

536 **2. Use time-series data when possible**

537 Forecasts describe how systems are expected to change through time. While some areas
538 of ecological forecasting focus primarily on time-series data (Ward et al. 2014), others
539 primarily focus on using spatial models and space-for-time substitutions (Blois et al.

540 2013). Using ecological and environmental time-series data allows the consideration of
541 actual dynamics from both a process and error structure perspective (Tredennick et al.
542 2016).

543 **3. Pay attention to uncertainty**

544 Understanding uncertainty in a forecast is just as important as understanding the
545 average or expected outcome. Failing to account for uncertainty can result in
546 overconfidence in uncertain outcomes leading to poor decision making and erosion of
547 confidence in ecological forecasts (Clark et al. 2001). Models should explicitly include
548 sources of uncertainty and propagate them through the forecast where possible (Clark et
549 al. 2001, Dietze 2017). Evaluations of forecasts should assess the accuracy of models'
550 estimated uncertainties as well as their point estimates (Dietze 2017).

551 **4. Use predictors related to the question**

552 Many ecological forecasts use data that is readily available and easy to work with.
553 While ease of use is a reasonable consideration it is also important to include predictor
554 variables that are expected to relate to the ecological quantity being forecast.
555 Time-series of predictors, instead of long-term averages, are also preferable to match
556 the ecological data (see #2). Investing time in identifying and acquiring better predictor
557 variables may ~~have at least as many benefits as~~ using more sophisticated modeling
558 techniques (Kent et al. 2014).

559 **5. Address unknown or unmeasured predictors**

560 Ecological systems are complex and many biotic and abiotic aspects of the environment
561 are not regularly measured. As a result, some sites may deviate in consistent ways from
562 model predictions. Unknown or unmeasured predictors can be incorporated in models

563 using site-level random effects (potentially spatially autocorrelated) or by using latent
564 variables that can identify unmeasured gradients (Harris 2015).

565 **6. Assess how forecast accuracy changes with time-lag**

566 In general, the accuracy of forecasts decreases with the length of time into the future
567 being forecast (Petchey et al. 2015). This decay in accuracy should be considered when
568 evaluating forecasts. In addition to simple decreases in forecast accuracy the potential
569 for different rates of decay to result in different relative model performance at different
570 lead times should be considered.

571 **7. Include an observation model**

572 Ecological observations are influenced by both the underlying biological processes
573 (e.g. resource limitation) and how the system is sampled. When possible, forecasts
574 should model the factors influencing the observation of the data (Yu et al. 2010,
575 Hutchinson et al. 2011, Schurr et al. 2012).

576 **8. Validate using hindcasting**

577 Evaluating a model's predictive performance across time is critical for understanding if
578 it is useful for forecasting the future. Hindcasting uses a temporal out-of-sample
579 validation approach to mimic how well a model would have performed had it been run
580 in the past. For example, using occurrence data from the early 20th century to model
581 distributions which are validated with late 20th century occurrences. Dense time series,
582 such as yearly observations, are desirable to also evaluate the forecast horizon (see #6),
583 but this is not a strict requirement.

584 **9. Publicly archive forecasts**

585 Forecast values and/or models should be archived so that they can be assessed after new
586 data is generated (McGill 2012, Silver 2012, Dietze et al. 2016). Enough information
587 should be provided in the archive to allow unambiguous assessment of each forecast's
588 performance (Tetlock and Gardner 2016).

589 **10. Make both short-term and long-term predictions**

590 Even in cases where long-term predictions are the primary goal, short-term predictions
591 should also be made to accommodate the time-scales of planning and management
592 decisions and to allow the accuracy of the forecasts to be quickly evaluated (Dietze et al.
593 2016, Tredennick et al. 2016).

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