# Multimodel approaches are not the best way to understand multifactorial systems

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**Abstract**

Information-theoretic (IT) and multi-model averaging (MMA) statistical approaches are widely used but suboptimal tools for pursuing a multifactorial approach (also known as the method of multiple working hypotheses) in ecology. (1) Conceptually, IT encourages ecologists to perform tests on sets of artificial models. (2) MMA improves on IT model selection by implementing a simple form of *shrinkage estimation* (a way to make accurate predictions from a model with many parameters, by "shrinking" parameter estimates toward zero). However, other shrinkage estimators such as penalized regression or Bayesian hierarchical models with regularizing priors are more computationally efficient and better supported theoretically. (3) In general the procedures for extracting confidence intervals from MMA are overconfident, giving overly narrow intervals. If researchers want to accurately estimate the strength of multiple competing ecological processes along with reliable confidence intervals, the current best approach is to use full (maximal) statistical models after making principled, *a priori* decisions about which predictors to include.

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Many modern ecological and evolutionary studies try to quantify the importance of multiple processes in ecological systems: for example, the effects of herbivory and fertilization on standing biomass (Gruner et al. 2008);

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the effects of bark, wood density, and fire on tree mortality (Brando et al. 2012); or the effects of taxonomic and genomic position on evolutionary rates (Ghenu et al. 2016). This *multifactorial* approach (McGill 2016) complements, rather than replacing, the traditional hypothesis-testing or strong-inferential framework (Platt 1964; Fox 2016).[1]

A standard approach to analyzing multifactorial systems, particularly common in wildlife and conservation ecology, goes as follows: (1) Construct a full model that encompasses as many of the processes (and their interactions) as is feasible. (2) Fit the full model and make sure that it describes the data reasonably well (e.g. by computing $R^2$ values or estimating degree of overdispersion). (3) Construct possible submodels of the full model by setting subsets of parameters to zero. (4) Compute information-theoretic (IT) measures of quality, such as the Akaike or Bayesian/Schwarz information criteria (IC), for every submodel. (5) Use multi-model averaging (MMA) to estimate model-averaged parameters and confidence intervals (CIs); possibly draw conclusions about the importance of different processes by summing the IC weights (Burnham and Anderson 2002). We argue that this approach, even if used sensibly as advised by proponents of the approach (e.g. with reasonable numbers of candidate submodels), is a poor way to approach multifactorial problems.

Our goal is to tease apart the contributions of many processes, *all* of which we believe are affecting our study system to some degree. If our scientific question is (something like) "How important is this factor, in an absolute sense or relative to other factors?", not "Which of these factors are actually doing *anything at all* in my system?", why are we working so hard to fit many models of which only one (the full model) incorporates all of the factors? If we do not have particular, *a priori* discrete hypotheses (such as "A influences the outcome but B does not") about our system (and a multifactorial approach would suggest that we should not), why does so much of our data-analytic effort go into various ways to test between, or combine and reconcile, multiple discrete models? In software engineering, this would be called an "XY problem"[2]: rather than thinking about the best way to solve our real problem $X$ (understanding multifactorial systems), we have gotten bogged down in the details of how to make a particular tool, $Y$ (multimodel approaches) provide the answers we need. Most critiques

[1] While there is much interesting debate over the best methods for gathering evidence to distinguish among two or more particular, *intrinsically* discrete hypotheses (Taper and Ponciano 2015), that is not my focus here.

[2] http://www.perlmonks.org/?node=XY+Problem

of MMA address technical concerns such as the influence of unobserved heterogeneity (Brewer, Butler, and Cooksley 2016), or criticize the misuse of IT methods by ecologists (Cade 2015), but do not ask why we are comparing discrete models in the first place. (Ecological statisticians are also beginning to emphasize the importance of *causal inference* (Fieberg and Johnson 2015; Laubach et al. 2021; Kimmel et al. 2021; Arif and MacNeil 2022); while this is important, it is not the focus here.)

One legitimate reason to fit multiple models is as a step in a null-hypothesis significance testing (NHST) procedure. While much maligned, NHSTs are a useful part of data analysis — *not* to decide whether we really think a null hypothesis is false (they almost always are), but to see if we can distinguish signal from noise. Another interpretation is that NHSTs can test whether we can reliably determine the *direction of effects* — that is, not whether the effect of a predictor on some process is zero, but whether we can tell unequivocally that it is positive (or negative, Jones and Tukey 2000). We can perform these tests by statistically comparing a full model to a reduced model that pretends the effect is exactly zero.

However, ecologists pursuing multimodel approaches are not fitting one-step-reduced models to test hypotheses; they are fitting a wide range of submodels, typically in the hope that multimodel averaging will help them deal with insufficient data in a multifactorial world. If we had enough information (even "big data" doesn't always provide as much information as we need), we could fit just the full model, drawing our conclusions from the estimates and CIs with all of the factors considered simultaneously. But we nearly always have too many predictors, and not enough data; we don't want to overfit (which will inflate our CIs and p-values to the point where we can't tell anything for sure), but at the same time we are scared of neglecting potentially important effects.

Stepwise regression, the original strategy for separating signals from noise, is now widely deprecated (Harrell 2001; Whittingham et al. 2006)[3]. Information-theoretic tools mitigate the instability of stepwise approaches, allow simultaneous comparison of many, non-nested models, and avoid the stigma of NHST. A further step forward, multi-model averaging (Burnham and Anderson 2002), accounts for model uncertainty and avoids focusing on a single best model. Some forms of model averaging provide simple *shrinkage estimators*; averaging the strength of effects between models where

[3]Although it may sometimes be adequate for selecting a single best model for prediction (Murtaugh 2009).

they are included and models where they are absent "shrinks" the estimated effects toward zero (Cade 2015). More recently, however, model averaging is experiencing a backlash, as statistically savvy ecologists point out that multimodel averaging may run into trouble when variables are collinear (Freckleton (2011; but cf. Walker 2017)); when we are careless about the meaning of main effects in the presence of interactions; when we average model parameters rather than model predictions (Cade 2015); or when we use summed model weights to assess the relative importance of predictors (Galipaud et al. (2014; but cf. Zhang, Zou, and Carroll 2015)).

IC were introduced into ecological science from applied ecology, by quantitative ecologists who were focused on making the best possible predictions to inform conservation and management. Now, however, rather than using IC as tools to identify the best predictive model, or to obtain the best overall (model-averaged) predictions, most users of information criteria use them either to quantify variable importance, or, by multimodel averaging, to have their cake and eat it too — to avoid either over- or underfitting while quantifying effects in multifactorial systems. Such users of IC encounter two problems, one conceptual and one practical.

The conceptual problem with model averaging reflects the original sin of unnecessarily discretizing a continuous world. Suppose we want to understand the effects of temperature and precipitation on biodiversity. The model-comparison or model-averaging approach would construct five models: a null model with no effects of either temperature or precipitation, two single-factor models, an additive model, and a full model allowing for interactions between temperature and precipitation. We would then fit all (or many) of these models and then model-average their parameters. We might be doing this in an effort to get good predictions, or to to test our confidence that we know the signs of particular effects (measured in the context of whatever processes are included in the reduced and the full models), but they are only means to an end, and we shouldn't fool ourselves into thinking that we are using the method of multiple working hypotheses. For example, Chamberlin (1897, reprinted as Raup and Chamberlin (1995)) argued that in teaching about the origin of the Great Lakes we should urge students "to conceive of three or more great agencies [pre-glacial erosion, glacial erosion, crust deformation] working successively or simultaneously, and to estimate how much was accomplished by each of these agencies." Chamberlin was *not* suggesting that we test which individual mechanism or combination of mechanisms fits the data best (in whatever sense), but instead that we acknowledge that the world is multifactorial.

The technical problem with model averaging is its computational inefficiency. Individual models can take minutes or hours to fit, and we may have to fit dozens or scores of sub-models in the multi-model averaging process. There are efficient tools available for fitting "right-sized" models that avoid many of the technical problems of model averaging. Penalized methods such as ridge and lasso regression (Dahlgren 2010) are well known outside of ecology; in a Bayesian setting, informative priors centered at zero have the same effect of *regularizing* — pushing weak effects toward zero and controlling model complexity (more or less synonymous with the *shrinkage* of estimates described above). Developed specifically for optimal (predictive) fitting in models with many parameters, these models have well-understood statistical properties; they avoid the pitfalls of model-averaging correlated or nonlinear parameters; and, by avoiding the need to fit many sub-models in the model-averaging processes, they are much faster.[4]

Here I am not tackling the issue of whether 'truth' is included in our model set (it isn't), and how this matters to our inference (Bernardo and Smith 1994; Barker and Link 2015). I am claiming the opposite, that our full model is usually as close to truth as we can get; we don't really believe any of the less complex models. If we are trying to get the best predictions, or to compare the strength of various processes in a multifactorial context, there may be better ways to do it. In situations where we really want to compare qualitatively different, non-nested hypotheses (Luttbeg, Langen, and Adams 2004), AIC or BIC or any appropriate model-comparison tool is fine; however, if the models are *really* qualitatively different, perhaps we shouldn't be trying to merge them by averaging.

Penalized models have their own difficulties. A big advantage of IC-based methods is that, like wrapper methods for feature selection in machine learning (Chandrashekar and Sahin 2014), we can use model averaging as long as we can fit component models and extract the log-likelihood and number of parameters — we never need to develop any additional software. Although powerful computational tools exist for fitting penalized versions of linear and generalized linear models (e.g. the `glmnet` package for R) and mixed models (`glmmLasso`), software for some of the more exotic models used by ecologists (e.g. zero-inflated models) may not be readily available. Fitting these models requires the user to choose the degree of penalization. Although this process is conveniently automated in tools like

[4] Although they often require a computationally expensive cross-validation step in order to choose the degree of penalization.

`glmnet`, correctly assessing out-of-sample accuracy (and hence the correct level of penalization) is tricky for data that are correlated in space or time (Wenger and Olden 2012; Roberts et al. 2016).

Finally, inference (computing p-values and CIs) for parameters in penalized models — one of the most basic outputs we need from a statistical analysis of a multifactorial system — is a current research problem; statisticians have proposed a variety of methods (Pötscher and Schneider 2010; Javanmard and Montanari 2014; Lockhart et al. 2014; Taylor and Tibshirani 2018), but they are far from being standard options in software. Ecologists should encourage their quantitatively savvy friends to build tools that make penalized approaches easier to use.

Statisticians derived confidence intervals for ridge regression long ago (Obenchain 1977) — but, surprisingly, they are identical to the confidence intervals one would have gotten from the full model without penalization! Wang and Zhou (2013) similarly proved that model-averaging CIs derived as suggested by Hjort and Claeskens (2003) are asymptotically (i.e. for arbitrarily large data sets) equivalent to the CIs from the full model. Analytical and simulation studies (Turek and Fletcher 2012; Fletcher and Turek 2012; Turek 2013, 2015; Kabaila, Welsh, and Abeysekera 2016; Dormann et al. 2018) have shown that a variety of alternative methods for constructing CIs are overoptimistic, i.e. that they generate too-narrow confidence intervals with coverage lower than the nominal level. Simulations from several of the studies above show that MMA confidence intervals constructed according to the best known procedures typically include the true parameter values only about 80% or 90% of the time. In particular, Kabaila, Welsh, and Abeysekera (2016) say that constructing CIs that take advantage of shrinkage but still achieve correct coverage will be very difficult to achieve using model averaged confidence intervals. (The only examples I have been able to find of MMA confidence intervals with close to nominal coverage are from Chapter 5 of Burnham and Anderson (2002).) In short, it seems difficult to find model-averaged confidence intervals that compete successfully with the standard confidence interval based on the full model.

Free lunches do not exist in statistics, any more than anywhere else. We can use penalized approaches to improve prediction accuracy without having to sacrifice any input variables (by trading bias for variance), but the only known way to gain statistical power for testing hypotheses, or narrowing our uncertainty about our predictions, is to limit the scope of our models *a priori* (Harrell 2001), to add information from pre-specified Bayesian priors

(or equivalent regularization procedures), or to collect more data.

If we have good experimental designs and sensible scientific questions, muddling through with existing techniques will often give reasonable results (Murtaugh 2009). But ecologists should at the very least be aware that the roundabout statistical methods they currently rely on to understand multifactorial systems were designed for prediction rather than inference. When prediction is the primary goal, penalized methods can work better (faster and with better-understood statistical properties) than multimodel averaging. When estimating the magnitude of effects or judging variable importance, penalized methods may be appropriate — or we may have to go back to the difficult choice of focusing on a restricted number of variables for which we have enough to data to fit and interpreting the full model.

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