Re: Estimating species occupancy across multiple sampling seasons with temporal autologistic occupancy models via the autoOcc R package.  Fidino, Mason.  
  
Thank you for submitting your manuscript to Journal of Animal Ecology.  
  
I have now received reviewers' reports and the Associate Editor's comments on your manuscript and looked at it myself. As you can see, both reviewers and our AE found the manuscript to be well-written, clear, and to provide valuable guidance on using the autoocc R package, supported by practical simulations and application examples that enhance accessibility for users. However, several aspects require major attention. At the moment, the framing lacks clarity regarding the model’s position within the broader literature, particularly by using the term “temporal autologistic” instead of the more standard “first-order Markov” or acknowledging its equivalence to a constrained form of the MacKenzie et al. (2003) dynamic occupancy model. The authors should better articulate this connection, clarify assumptions about model structure (e.g., covariate effects on colonization vs. extinction), and improve the manuscript’s practical utility by including comparative schematics, clearer terminology, and additional context on limitations and flexibility of the approach.  
  
  
As a result, I cannot accept this current version of the manuscript but I am giving you the option to submit a revised version of the manuscript for consideration.  
  
Although we are giving you the option to complete a major revision on this manuscript, please note that acceptance and publication is not guaranteed. Your revised manuscript is due on 12-Oct-2025. If you foresee any problems with meeting this deadline, please contact the editorial office at: admin@journalofanimalecology.org  
  
It is important to respond to all comments in this decision letter, showing how you have amended your manuscript. Please remember that we expect your manuscript to be carefully re-worked with a detailed response as to how you have addressed the comments on a point by point basis.  
  
When revising your manuscript, please refer to our author guidelines for revisions: https://besjournals.onlinelibrary.wiley.com/hub/journal/13652656/author-guidelines-revisions  
Please note your resubmitted manuscript must contain a Data Availability Statement, stating where you intend to archive your data associated with the manuscript. For details on Journal of Animal Ecology's data archiving policy please see: https://besjournals.onlinelibrary.wiley.com/hub/data\_archiving\_policy  
  
We are also offering language editing with Writefull, free of charge, which may be of interest to you. Writefull screens your text for correctness of grammar, spelling, vocabulary, punctuation, style, word order, phrasing, and more. You can use this service by following this link: https://revise-c7962b5ef21ca52c4163c44e983d5b45e2.writefull.ai  
  
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The new manuscript will be sent to the original Associate Editor who may or may not choose to send it out for Peer Review and may or may not ask the original referees.  
  
I will then make a decision based on these reviews and my own reading of the manuscript.  
  
This journal offers a number of license options for published papers; information about this is available here: https://authorservices.wiley.com/author-resources/Journal-Authors/licensing/index.html. The submitting author has confirmed that all co-authors have the necessary rights to grant in the submission, including in light of each co-author’s funder policies. If any author’s funder has a policy that restricts which kinds of license they can sign, for example if the funder is a member of Coalition S, please make sure the submitting author is aware.  
  
Yours sincerely  
Rob Salguero-Gómez  
Editor, Journal of Animal Ecology  
  
  
Associate Editor  
Associate Editor Comments for Authors:  
This manuscript has been reviewed by two experts who had differing assessments. Reviewer 1 had a largely positive assessment of this manuscript, whereas reviewer 2 questioned the novelty and technical framing of the model. Both reviewers made important points that would need to be satisfactorily addressed prior to publication.  
  
REFEREES'COMMENTS TO AUTHORS  
  
(nb. If there is no comment from a Reviewer listed below, this probably means that they have uploaded a separate 'file for author' to the ScholarOne Manuscripts Site. You can see these comments in your Author's Centre by clicking ‘manuscripts with decisions’ and then using the 'files attached' link at the bottom of the decision letter)  
  
Reviewer: 1  
  
CONFIDENTIAL COMMENTS TO AUTHORS  
This is a Research Methods Guide for running autologistic occupancy models with the R package ‘autoocc’. I found the manuscript to be very clear, well written, with an interesting choice of two different application examples as well as a useful simulation. I am strongly in support of the publication of this manuscript and only provide minimal line comments.  
  
Line comments:  
  
38. I know that McKann et al. 2012 is already published, but this requirement (120 sites) doesn’t make sense, as the power of a dynamic occupancy model also depends on the number of seasons, the number of visits per season, the average probability of detectability, etc. If you have many seasons (e.g., 40) and moderate detectability and no temporal trend in colonization, you certainly need many fewer than 120 sites for adequate power.  
  
52. I don’t believe that it \*is\* guaranteed that temporal dependence is “absorbed by site-level random effect”? The random effect will treat all annual draws of occurrence as random and uncorrelated, essentially a non-Markovian process. Where does obvious Markovian temporal dependence go? It could be partly absorbed by the random effect, it could just remain in error, or it could (unfortunately) be mistakenly applied to other fit parameters. Imagine if you put in a seasonally-varying covariate, that covariate might become over-fit or biased due to remnant temporal dependence in the data. My view is that the mistaken assumption of stacked models that there is no temporal dependence is partly why no one should use them.  
  
90. Would it be possible to add in a schematic table that shows a single multi-season sampling scheme with the different models (dynamic, stacked, autologistic) and what parameters they inform?  
  
133. Andy Royle and others, I think, have referred to this phenomenon (Theta having no effect when z[t-1]=0) as the autologistic model providing persistence correlation but not colonization correlation. In other words, a high theta can keep occupancy at {1, 1, 1, 1, …}, but it has no effect on a no-occupancy site such as {0, 0, 0, 0, …}. Mathematically, it’s why the autologistic parameterization has been referred to as a ‘half-dynamic’ model, I believe.  
  
210. Worth parenthetically mentioning that missing surveys would be just ‘NA’s?  
  
  
Reviewer: 2  
  
CONFIDENTIAL COMMENTS TO AUTHORS  
While I think there are things to like in this paper, it suffers from some serious flaws in terms of framing and understanding its place in the broader literature, which I detail below:  
The use of the term "temporal autologistic" models is, in my opinion, confusing. I know these authors are not the first ones to use this term, but the perpetuate this term. The term autologistic has been used for a much longer time to refer to situations in which the probability that a patch is occupied is a function of whether neighborhood patches are occupied. Augustin (1996) is the first use of the term in the ecological literature that I am aware of and they referred to modelling of static patterns. Wikle (2003) and Hooten et al. (2007) applied autologistic modelling in a dynamic context, while Bled et al. (2011), and Yackulic et al. (2012) applied it specifically in a dynamic occupancy context using the status of neighbors in time t to model the likelihood of colonization and extinction in time t+1. More recently, the use of the term “temporal autologistic” has been used to describe situations that the broader statistical literature and the narrower occupancy literature often refers to simply as a 1st order Markov model (i.e., a model in which the probability of being in a given state at time t+1 depends on the state at time t)– there is, in my opinion, no need to introduce the temporal autologistic term when a 1st order Markov model is a more generally understood term. Failure to recognize the broader term, in my opinion, has led folks who use the term to not understand that there model is really a special case of the general dynamic occupancy model introduced by MacKenzie et al. (2003) – which is the bigger issue.  
In the Mackenzie et al. (2003) model, colonization (γ\_(i,t)) and extinction (ε\_(i,t)) at site i, in interval t, are modelled separately:  
logit(γ\_(i,t))=α\_0+βX\_(i,t)  
logit(ε\_(i,t))=δ\_0+ηY\_(i,t)  
Where α\_0 and δ\_0 represent intercepts, β and η represent vectors of estimated slopes, and X and Y are arrays of covariates (with dimensions given by the number of sites, the number of intervals, and the number of covariates, where X\_(i,t) yields a row vector of the covariates values for site i, in interval t). Using latent variable notation as was used in the paper , the probability of occupancy in time t+1 is given by:  
ψ\_(i,t+1)=〖(1-ε\_(i,t) )\*z〗\_(i,t-1)+〖γ\_(i,t)\*(1-z〗\_(i,t-1))  
It is fairly common, particularly in Bayesian applications of dynamic occupancy models to model persistence (r\_(i,t)) instead of extinction, where persistence is defined as (1-ε\_(i,t)) or alternatively as logit(r\_(i,t) )=〖-δ〗\_0-ηY\_(i,t) if we maintain equation a above. Switching to persistence is useful here because it illustrates how the “temporal autologistic” model is merely a special case of the Mackenzie et al. (2003) model. Using persistence, occupancy is defined as:  
ψ\_(i,t+1)=〖r\_(i,t)\*z〗\_(i,t-1)+〖γ\_(i,t)\*(1-z〗\_(i,t-1))  
Now, if we define a term, θ, as  θ=〖-δ〗\_0-α\_0, slightly change our notation of β and X and make two assumptions we can reach the model presented in this paper on equation 2. Specifically, we must assume assume β=-η and use the same set of covariates (i.e., set X=Y). Then in terms of notation, we add α\_0 to the vector  β and add a matrix of 1’s to the array X and voila we have the temporal autologistic model.  
Summarizing the math from 2, the model presented here is simply a special form of the Mackenzie model in which the slope of covariate effects of extinction are assumed to be the exact inverse of the slopes of covariate effects on colonization (or alternatively where the slopes are assumed to be same on colonization and persistence) and where instead of estimating independent intercepts, we estimate the colonization and intercept and the difference that yields the extinction intercept – this is the exact same as switching between a means and difference parameterization in standard regression. The means vs. difference parameterization may be useful in some contexts (depending on the object of inference) but yields the same AIC in a model and both have been used in the broader dynamic occupancy literature. The assumptions of inverse (or equal in the case of persistence) slopes has also been used in the literature, can be easily implemented in programs like unmarked, MARK or Presence, and may make sense in some situations, however it includes a specific assumption (that covariates don’t affect colonization and extinction/persistence in different ways). The simulations don’t address this assumption.  
Minor comments:  
Line 104-105 This is not strictly true. While in a first-order Markov process state transitions only depend on the state in the previous time step, the states themselves may be autocorrelated over much larger lags with the memory of the Markov chain depending on the turnover rate (i.e., with high colonization and high extinction rates turnover is higher and memory lower, with low colonization and low extinction rates autocorrelation (memory) is higher and turnover lower).  
Line 150 you can also do it via Bayesian inference (see Yackulic et al., 2020).  
Line 219 This is the unconditional (i.e., not conditioned on the actual observations at a particular site) steady-state approximation (steady-state because it doesn’t account for expected occupancy in prior intervals (either conditional or unconditional) and is not a good estimate of occupancy under many circumstances.