Recommendation by the Subject Editor (anonymous):  
This paper was read by two reviewers, both of whom saw value in the paper for the Ecography audience. However, both had concerns about the presentation and accessibility of the manuscript as currently written, which will take some hard work to address. A number of the statistical details need to be presented more clearly (and some potential statistical issues related to estimability and AIC addressed), and the overall framing presented more clearly to non-fisheries audiences. Ecography's readership works in a wide range of ecosystems, and this paper should be accessible to that range.  
  
Reviewer(s)' Comments to Author:  
Reviewer: 1  
  
Comments to the Author  
I was excited to read this manuscript as it presents a new way to detect shift in species distributions, which is clearly an important topic. Overall, I like the approach presented and think it will be a good contribution. I also think it fits well with Ecography’s goals and scope. As you see below, however, I do have some reservations with the approach. In addition, I think many of the details are missing, which makes it hard to fully assess the validity of the method and the quality of the results.  
  
Major comments:  
  
1. In the methods section, most of the model details are lacking. I appreciate that part of the reason was potentially to keep the description general, but by doing so it makes it really hard for readers and reviewers to assess the validity of the method and nearly impossible to reproduce the method on their on data. Thus, I think the manuscript needs to present the full models (with all of the equations).  I think it would make sense to present the full model for the fish data in details in the main text, thus including the observation equation, the link function, the distribution used for the random fields, the correlation structure (Mattern?), etc.  I would also write down the full model for the simulation, but this could be placed in the supplementary material. I realize that the code is available, but this is not sufficient, the model must be described. Describing the model is essential to understand things like the parameters stated in Table S1 and the results presented in Fig. S1.

**We appreciate the desire of the reviewer and other readers to have these details readily available. We have revised the methods in the main text to include all relevant details and full equations. We have also added details regarding the simulations to the appendix as requested.**  
  
2. I think these kinds of spatial random fields model suffer from Identifiability issues even without observation error and looking at the equation definitely make me fear that this is the case. The simulation studies should explore changes in the magnitude of all variance parameters (sigma\_0, sigma\_{0,trend} in addition to sigma\_E and phi).

**There is an existing literature exploring these issues in models with the same essential structure, but without the spatial trend component. We modified the text to point the reader to these studies. Furthermore, to address the question in the context of this specific model, we performed additional simulations as recommended to determine how the magnitude of the spatial variance (sigma\_O) and spatial trend variance (sigma\_0\_trend) influence model performance. These results were in line with our intuition and described in a new appendix figure along with additional text in the results of the main manuscript.**  
  
3. I’m not a 100% sure what the theta is in the simulation results (e.g. in Fig 3). My understanding is that Z\_s will have multiple values for a simulation (one for each point s), so is theta the mean Z\_s or are you taking each Z\_s value of a simulation to be independent (then why not just call theta Z\_s and \hat{Z}\_s?). I don’t feel that either is great nor terrible, but knowing what it is definitely essential.     
  
4. My understanding is that AIC can be pretty poor at selecting between random effects due to parameter boundary problems. It would be worth using the simulation to see whether you can select between the model with and without the spatial trend. This could be done by fitting the model with and without the trends to your simulations and see whether you can recover the good model. You may need to add a model version without a spatial trend.

**We generated data with and without a spatial trend and compared models using AIC. We demonstrated that we were able to recover the correct model and summarized the results in a new figure.**

5. Have you fitted the model with SST as a covariate and compared it with the trend model? It looks like some of the patterns are likely driven by changes temperature and it would help when understanding the patterns and support some of the statements in the discussion.  
  
6. L217-225: it’s only after reading the results and really digging into the figures that I understood what you are comparing. My understanding now is that what is done by others is calculate the COG either coastwise/for the whole range of species (which you call coarse-scale), or by sub-regions (which we could call med-scale?) - in your study there are 3 sub-regions divided by Point Conception and Cape Mendocino (you could specific these in a map and refer to it, see comment below for Fig 2). You are proposing that looking at the spatial trend (Z\_s, which you call fine-scale) is much better. Could you please clarify this.  
  
7. Similarly here, it took me a while to understand the clustering algorithm and its goal. I think it needs to be clarified here and likely needs its own paragraph (separated from the COG). I might be wrong, but my understanding is this is a non-spatial clustering technique that is used, and that the only spatial component is the latitude. Wouldn’t you want to use a spatial clustering algorithm? Or something that allows to identify hotspots and coldspots? My intuition is that this would allow you to better identify the shifts inland that are discussed in the discussion. Here the cluster are really restricted by latitude and the patterns of the clusters are not particularly striking. I wonder (not sure if it makes sense), whether it would be worth plotting the change in COG for the clusters?  
  
8. I don’t find Fig 4 compelling. Almost none of the clusters matched the bottom line, and only a few matched the top line. What are you defining as near? I woonder if adding area boundaries to the second column of Fig 5 would better show your results? Or maybe the clusters are not really getting at these boundaries?  
  
Minor comments:  
  
The title is not really selling the paper. It feels like you are comparing metrics that are already available, but in fact you are proposing a new method.  
  
L147: I think it’s a bad idea to call it spatial trend. You are trying to get a temporal trend? I think generally spatial trend is understood to be a systematic (and generally smooth) change in the mean value of a variable over an area. Here it’s just a random field no (and from Fig 5 it not particularly smooth and systematic)? And the main interest is that it demonstrates how the other random field change through time.  
  
L153: I realize that the model presented is likely a state-space model, especially when we include the observation error, but nowhere is it referred as such (and actually the observation equation is never presented), so this connection is not clear.  Worth mentioning above that it is a state-space model. Also, would it be a state-space if there were no observation error?

**Without an observation model, it’s still a state space model because the random effects are estimated at the knot locations and predicted to the data locations. But I don’t really like the state space model jargon – it could also be labeled as a mixed effects model or random effects model**  
  
L170-182: Could you add 1-2 sentences describing the survey effort.  
  
I would number all of the equations. It helps to refer to them.  
  
Fig. 2 Please add boundaries around the 3 sub-regions and/or large lines at the two breaks, so the sub-regions are clear.  
  
Fig 3.  why is  sigma=1 (which according to table S1 was explored) is not presented?   
This is a really really small detail, but it would help quickly understand the results (without going back to the table S1), if you had 0.01 rather than 0.00 on the y-axis.  
  
Fig.5 Why the mean density value over all years? Why not the value at t 0 and final time, to show the trends. Even if it’s not striking, it might make the point that just comparing the predicted value at time 0 and last year is less powerful than looking at the trend estimated. The map projection is also strange.  
  
Reviewer: 2  
  
Comments to the Author  
Thank you for giving me the opportunity to review the manuscript “Comparing metrics of species distribution change across spatial scales with spatiotemporal models”.  The authors present a new modeling approach to account for spatial and temporal variability in species density and distribution.  I find the title somewhat misleading as I don’t see this as a comparison but rather the introduction of a new model.  Although I am familiar with species distribution models, I’m afraid I found the paper hard to follow and some of the figures completely indecipherable.  I fear the utility of the paper may be lost on someone unfamiliar with fisheries.   
As a terrestrial species biologist I found myself looking for terrestrial analogies and trying to understand the methods in that context.  The authors cite Yackulic et al 2013 who provide a review of MAXENT and presence only data.  I wonder if a better comparison would be to the extensive literature on spatially explicit mark recapture (SECR) that easily incorporates temporal and spatial variation in species density.  I’m obviously biased and perhaps this is an unfair criticism but I wonder if the paper could be made more broadly approachable by adding comparisons to spatial mark recapture and or terrestrial systems and surveys?  
If I understand correctly a major limitation in the model is that it requires a linear trend in density and distribution.  Given the introduction I assumed that the model would take into account species density rather than distribution to better track population trend (i.e. a shift in distribution may suggest a decline in population that is not supported if density is accounted for).  I’m left wondering what happens if a decline is not systematic and why this is better (as stated on line 170).  
  
Lines 34-37 – You provide examples of the ranges of things that might be have distributions we want to track, but you don’t anything about what might cause distribution shifts.  Might also be beneficial to add a sentence regarding what causes distribution shifts.  
Line 40 – “However, when reliable population density data are available, distribution shifts are better quantified by spatial predictions of population size” This statement might benefit from the support of a citation.  
Line 55 – I found this transition a little distracting.  I don’t find the choice of appropriate scale any more or less germane to this question than any other in ecology so am not sure this is necessary.  
Line 109 – This is the first use of SDM in the manuscript so the acronym needs to be defined.  
Line 149 – Figure 1, like all of the figures in the manuscript excluding figure 2, is a bit of a challenge.  Judging from the model is it necessary that the trend be linear from year to year?  I’m not sure what it is the authors are trying to convey here in the figure.  
  
Figure 4 – I find it almost impossible to decipher anything from this figure.  
Figure 5 – labeling the North American continent may make it more readily apparent that each tile represents the west coast.  What do the colors in column 2 represent?  There is no legend.