Dear Mr. Berigan,  
  
Thank you for submitting your manuscript to Biological Conservation. We regret to inform you that your paper is not acceptable for publication. We have completed the review of your manuscript and a summary is appended below. The reviewers have advised against publication of your manuscript and we must therefore reject it at this time. For your information and guidance, any specific comments explaining why we have reached this decision and those received from reviewers, if available, are listed at the end of this letter.

You have the option of resubmitting a substantially revised version of your paper, which would be considered as a new submission. If you decide to do this, you should refer to the reference number of the current paper and include a cover letter which explains in detail how the paper has been changed or not, in reply to the Editor and Reviewer comments.  
  
Thank you for giving us the opportunity to consider your work.

Kind regards,  
Biological Conservation  
  
  
Comments from the editors and reviewers:

Editor: We apologize for the time it has taken you to get a recommendation on this submission, and thank you for submitting your manuscript to Biological Conservation. As you will find, both reviewers appreciate the topics of your study, while the authors should extensively revise the manuscript by improving the clarity of the writing and providing the details of the analyses for reproducibility. Note that reviewer #2 provided the comments in a separate PDF file.

Reviewer #1: The manuscript "Multi-seasonal species distribution models better facilitate habitat conservation for a migratory bird" used a spatial decision support system (SDSS) to highlight areas of conservation importance for the American woodcock in Pennsylvania state relying on two separated seasonal models of the species (breeding and migratory). The general finding was that habitat preference differs between seasons, a result that should be considered for effective conservation decisions. The framework used here could inform management plans for the species and can be applied to other migratory species, as was nicely discussed in the manuscript. However, some concerns should be addressed before publication, mainly regarding terminology and methods.  
  
1. Using a "multi-season model" to describe this framework could be misleading. The first time I read the term, I thought about building a unique species distribution model incorporating several seasons instead of building separated single-season distribution models to be used as inputs for the SDSS. For example, multi-season occupancy models, also known as dynamic occupancy models, can incorporate in the model building per se occupancy changes over time. Your framework does not include any temporal variable in the model that allows you to estimate trends in species presence. Additionally, there are already examples of SDMs incorporating time in the modeling framework (e.g., Dynamic or time-specific SDMs), which could be an alternative to estimate the species' realized niche throughout the year. This does not mean that the methodology proposed here is invalid, but it should focus more on using SDSS with several single-season models, which is what you are doing here.  
  
2. Along the manuscript, you framed spatiotemporal transferability between seasons. Still, you never performed analyses that compared your model performance between single-season models (e.g., use the breeding season model to estimate the migratory season and vice versa). To check that your method solves this issue, compare the single-season model predictions. The fact that you find a low correlation between the geographic model predictions (as affirmed in Lines 295-296) does not mean poor transferability between them.  
  
3. There is evidence in the literature of the inconvenience of using just AUC values as the only evaluation metric for model performance, especially when there is no real absence to calculate commission error. Please complement the evaluation of your models with other metrics such as omission rate, Continuous Boyce index, partial ROC, etc. Incorporating more metrics will also let you know if the higher AUC values of Random Forest are not due to model overfitting.  
  
  
4. The normalization of the model outputs (Lines 195-196) should be accompanied by their interpretation with the occurrence records. The fact that you have 50% of your normalization does not inform you about where to find the species. For example, the minimum training presence could show how low a suitability value can be to be considered part of the species' potential habitat. Additionally, the fact that your seasonal models use different variable sets makes comparing them harder. For example, the minimum suitability value for a presence could differ between seasons.  
  
5. An important outcome of your study is a Shiny app. Unfortunately, it was unavailable at the moment of this review (your site was down).  
  
  
Minor comments:  
  
Line 156 & 159: Space between units.  
  
Figure 2. Please clarify what you mean by prioritization metrics.  
  
Figure 3. This figure is hard to understand; please consider using a response curve.  
  
  
  
Reviewer #2: Your paper has a lot of promise and is certainly on an important topic! I think this paper would benefit from restructuring, more background and more references that directly explain the key underpinnings of this paper, far more methodological details (particularly related to SDMs), and improvements in writing quality. I believe the study has a great deal of potential and the topic is very interesting and important, but at its present state, it is simply not ready for publication. With some work and restructuring, I am hopeful that it can be published in a reputable journal!