Dear EcoSphere Editorial Board,

We are re-submitting our article (MS#ES14-00533) based on the invitation to re-submit with revisions. We are grateful for this opportunity to re-submit an improved manuscript.

We have made revisions in response to the concerns of the reviewers, which we discuss in detail below. The three most substantial suggestions were to expand our discussion of (1) multi-species systems, (2) invasion theory, and (3) MPA spacing and heterogeneous harvesting pressure. We discuss our revisions with respect to these suggestions first. Any significant revision is highlighted in blue in the manuscript.

* Reviewer 1 suggested we broaden our discussion by incorporating commentary on multi-species systems. We agree and expanded our discussion of multispecies systems by incorporating the reference the reviewer suggested, as well as a broader discussions of the merits of multispecies models and why both single- and multispecies are needed (lines 448-459).
* Reviewer 1 suggested we expand our discussion of invasion theory. We have expanded our discussion of the similarities between an invading population at initially low abundance, as usually modeled in invasion theory, and a population tracking a shifting climate, as in our model (lines 376-386). We have also added a reference to studies of invasion into a patchy environment and a discussion of the similarity between that model and ours (lines 403-407).
* Reviewer 1 brought to our attention references on MPA spacing that related to our comparison of many small or fewer large MPAs. Reviewer 1 also suggested we add a discussion of how the distribution of harvesting pressure affects this comparison. We incorporated a number of new references that showed the diversity of existing opinions about what type of MPAs are optimal and added a discussion of how our results compared to previous models (lines 415-433). We also added a discussion of how increased harvesting pressure at the boundary of protected and unprotected areas would affect our results (lines 462-470).
* Reviewer 1 felt that our presentation of the results in the Introduction, Results, and Discussion was redundant. Reviewer 2 also felt that describing our results in the Introduction was redundant. We have removed the details of the results from the Introduction accordingly (lines 82-91).
* Reviewer 1 suggested we clarify the distinction between our analytical methods and simulation methods and the dispersal kernels we used for each. We have renamed the section previously entitled “Simulations” so that it is now entitled “Management strategies” to emphasize that simulations allow us to implement management strategies that are analytically intractable. We have added a few lines of clarification there to explain more fully the purposes of the two types of analysis (lines 206-209). We also added a sentence emphasizing that we chose to analyze a different dispersal kernel with each method to ensure that our results were robust to this choice (lines 271-275).
* Reviewer 1 suggested we clarify our language about interactions between the two stressors throughout the paper. To help clarify our discussion of interactions, we have made a distinction between an interaction between the critical rates of the two stressors and the interaction between their effects on population biomass (lines 173-177) and we have tried to be more explicit about which type of interaction we are referring to throughout the paper.
* Reviewer 2 suggested we include a case study to show how the model could be applied to an empirical system. We acknowledge that adding a case study would help to validate our results and show how they could be applied to empirical systems. However, a case study is beyond the scope of our paper. Further, we discuss in the manuscript that, even without an empirical study, the merit of our theoretical model is that it helps to build intuition for how these two stressors might interact and it sets a baseline prediction to which empirical results can be compared.
* Reviewer 2 suggested we discuss why we used as many iterations in our simulations as we did and the robustness of our results to this choice. We did not mean to suggest that we were modeling a population dealing with harvesting and climate change for 6000 years. The population reaches equilibrium after a short number of iterations, but we used a large number of iterations to ensure consistent numerical results. We have now added some text to the Model section to clarify this issue (lines 141-145 and 242-244). Reviewer 2 also asked us to mention our parameter choices in the main text. To address this point, we added a sentence in the text stating that our results are qualitatively robust to the choice of parameters (lines 275-277).
* Reviewer 2 suggested we clarify our language about plants. We thank the reviewer for noticing the ambiguity in our language here and fixed the sentences to remedy the issue. Reviewer 2 also identified typos in our references. We thank the reviewer for noticing these mistakes and have remedied them.

We hope that our revisions have addressed the concerns the reviewers made. We appreciate your time and look forward to your response.

Sincerely,

Emma Fuller

Eleanor Brush

Malin Pinsky