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 revisions were to expand our discussion of (1) multi-species systems, (2) invasion theory, and (3) MPA spacing and heterogeneous harvest pressure, which we discuss first in the description of our changes. 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 model and why both single- and multi-species are needed (lines 464-476).
* Reviewer 1 suggested we expand our discussion of invasion theory. We thank the reviewer for pointing us to this literature. We have expanded our discussion of the similarities between an invading population at initially low abundance, as usually modeled in invasion theory, and the population tracking a shifting climate, as in our model (lines 381-394). We have also incorporated a reference to the literature on invasion into a patchy environment and a discussion of a similarity between that model and ours (lines 413-421).
* 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 sentence about how whether a fishery is optimally or over harvested affects the impacts of MPA spacing and more discussion of the distribution of harvesting pressure. We incorporated a number of new references that showed the diversity of existing opinions about what type of MPAs are better and added a discussion of how our results compared to previous models (lines 432-449). We also added a discussion of how increased harvesting pressure at the boundary of protected and unprotected areas would affect our results (lines 479-488).
* 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 83-91).
* Reviewer 1 suggested we clarify the distinction between our analytical methods and simulation methods and the dispersal kernels we used for each. The section previously entitled “Simulations”, we have renamed “Management strategies” to emphasize that simulations allowed us to implement management strategies that are analytically tractable. We have added a few lines of clarification there to explain more fully the purposes for the two types of analysis (lines 211-215). 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 277-281).
* Reviewer 1 suggested we clarify our language about interactions between the two stressors throughout the paper. We recognize that our language with respect to interaction was confusing. To help clarify these points, 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 211-215) 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, but have not been able to do in the time allotted.
* 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 we used was primarily to ensure consist numerical results. We have now added some text to the Model section to clarify this issue (lines 248-250). 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 and we choose a representative set of parameters for our figures" (lines 281-283).
* 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