

Dear recommender and reviewers,

We are deeply thankful to both the recommender and reviewers for their constructive comments that improved the paper. I will screen through major comments arisen during the review process and detail modifications made to the manuscript (a version of the manuscript with changes tracked in blue is available).

First, following the suggestion of Ana Rodrigues, we modified the title to make it clearer, changing to: "The persistence in time of distributional patterns in marine megafauna impacts zonal conservation strategies".

One of the main points of concern was the habitat model quality, as deviances lower than 40% might appear as of medium quality. The habitat modelling procedure described here builds from an in-depth exploration of methodological aspects to maximize habitat quality in terms of explained deviances and prediction adequacy. The first approach was a classical one, using environmental covariates directly to model the distribution of species (with a backward selection procedure to select best models). However, this method did not provide satisfying results and we instead used the PCA dimensions as covariates. The PCA being a good summary of the habitats available in the study area (see Lambert *et al.* 2018) and this method permitting to reduce the number of covariates, the habitat modelling results in good predictions of species distributions. The obtained explained deviances are satisfying regarding the standard of marine megafauna habitat modelling and predictions adequately fit the original sightings.

However, we agree with recommender and reviewers that the quality of the great skua model was poor, with a rather small deviance and predictions of medium adequacy to sightings. Yet, it was clearly the best model we were able to obtain using either of the two tested methods. We argue this arises from the particular biology of the species, which feeds mainly from kleptoparasitism and thus might not select habitat based on environmental conditions. As a result, the modelling procedure was not appropriate for this particular species. For this reason, we decided to remove the great skua from the pool of species considered in the present paper.

As was recommended by reviewers, we added some discussion in text regarding the quality of the models used for the analysis, but also on their dedicated results (habitat preferences more or less stable across years depending on species). In addition, we also added details in the method section on the best model selection based on AIC under request of the second reviewer, as it was not clear why we chose the global model as best one (we parsimoniously chose the simplest model when the difference in AIC between models was negligible).

Reviewers also asked for making clearer which parts of the presented analysis was already published in Lambert *et al.* 2018 and which was not. We clearly stated in the "Data source" section that all the habitat modelling procedure builds from Lambert *et al.* 2018, providing a brief summary of the method in the main text, but detailing the complete procedure in the Appendix A (also adding maps and number of sightings as well as the PCA, as requested by reviewers).

All the clarifications requested in the methods were made: the frequency of PELGAS cruise, the methods for calculating aggregation of distribution, defining core areas and their persistence. Figures were also clarified as requested.

The second reviewer pointed out that we used the 75% threshold in method section but never introduced it before nor used it after: thank you for having spotted this; it was a mistake and all

reference to the “50% threshold” elsewhere in the text should in fact be “75% threshold”. This was fixed throughout the text and figures.

More precise definitions of aggregation and persistence were moved from the results section to the relevant method section for better clarity, as requested by reviewer 2. We also better defined the aggregation level in the introduction, clarifying the definition and expected relationship.

Finally, we added some words in the discussion on the seasonal variability of distributional patterns of studied species in the study area.

Suggestions of improvement from Ana Rodrigues for the start of the introduction were incorporated, and the first paragraphs a bit rearranged. We also incorporated her minor in-text suggestions.

Following suggestion of the second reviewer, we also made clearer in the abstract and discussion that one main point was the absence of covariation between aggregation and persistence.

Thanks again

Charlotte Lambert, on behalf of the co-authors.