The importance of spatio-temporal dynamics on MPA's design

Sergio Estay based on reviews by Ana S. L. Rodrigues and 1 anonymous reviewer

A recommendation of:

Charlotte Lambert, Ghislain Dorémus, Vincent Ridoux. **The persistence in time of distributional patterns in marine megafauna impacts zonal conservation strategies** *(2020), bioRxiv, 790634, ver. 3 peer-reviewed by Peer Community in Ecology.* 10.1101/790634

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Sergio Estay (2020) The importance of spatio-temporal dynamics on MPA's design. *Peer Community in Ecology, 100048.* 10.24072/pci.ecology.100048

Marine protected areas (MPA) have arisen as the main approach for conservation of marine species. Fishes, marine mammals and birds can be conservation targets that justify the implementation of these areas. However, MPAs undergo many of the problems faced by their terrestrial equivalent. One of the major concerns is that these conservation areas are spatially constrained, by logistic reasons, and many times these constraints caused that key areas for the species (reproductive sites, refugees, migration) fall outside the limits, making conservation efforts even more difficult. Lambert et al. [1] evaluate at what point the Bay of Biscay MPA

contains key ecological areas for several emblematic species. The evaluation incorporated a spatio-temporal dimension. To evaluate these ideas, authors evaluate two population descriptors: aggregation and persistence of several species of cetaceans and seabirds. The authors determined that despite the MPA contains key areas for some species, for many others the key areas fall outside the MPA (aggregation sites) or observed aggregation sites are poorly persistent in time. They found that aggregation and persistence behave as two uncorrelated descriptors of the spatio-temporal distribution of populations. Variability of both characteristics was species-specific, but in all cases the message is clear: both features must be taken into account to evaluate the effectiveness of MPAs. Both conclusions pointed out to the difficulties that a strategy based on MPAs could face when the target are those species with low aggregation or those where key sites show low persistence in time. Conceptually, the manuscript and its conclusions are very interesting, specially its recommendation of including temporal variability of species abundances and aggregation in the design of MPAs. However, despite the clear biological importance of persistence and aggregation of the conservation targets for the design of a MPA, its implementation will still be an extremely complex task. A first constraint is that important areas for one species could not be relevant for others, making the design of the MPA difficult because the more target species we include the larger the area needed for the MPA. As a consequence, the management of the MPA turns difficult and expensive as the area increases. These increased costs could be a key point for accepting/rejecting the implementation of these MPAs for governments. Also larger areas could imply highest level of conflict with local communities or stakeholders. In many the inclusion inside MPAs of areas with traditional social or economic use will be a major source of conflict with the people. Despite these difficulties, the results of Lambert et al. [1] give us a key message for improving MPA's design. The best strategy for including their conclusions in the effective implementation of these areas will be the next target in conservation research.

References

[1] Lambert, C., Dorémus, G. and V. Ridoux (2020) The persistence in time of distributional patterns in marine megafauna impacts zonal conservation

strategies. bioRxiv, 790634, ver. 3 peer-reviewed and recommended by PCI Ecology. doi: 10.1101/790634

Revision round #1

2019-12-02

Marine protected areas (MPA) have arisen as the main approach for conservation of marine species. Fishes, marine mammals and birds can be conservation targets that justify the implementation of these areas. However, MPAs undergo many of the problems faced by their terrestrial equivalent. One of the major concerns is that these conservation areas are spatially constrained, by logistic reasons, an many times these constrains caused that key areas for the species (reproductive sites, refugees, migration) fall outside the limits, making conservation efforts even more difficult. Lambert et al. evaluate at what point the Bay of Biscay MPA contains key ecological areas for several emblematic species. The evaluation incorporated a spacio-temporal dimension. In particular, authors use aggregation as a measure for spatial identification of key areas, and persistence for identifying key areas in time. The authors determined that despite the MPA contains key areas for some species, for many others the key areas fall outside the MPA (aggregation sites) or observed aggregation sites are poorly persistent in time. Both conclusions pointed out to the difficulties that a strategy based on MPAs could faced when the target are those species with low aggregation or those where key sites show low persistence in time. Conceptually, the manuscript and its conclusions are very interesting, specially its recommendation of including temporal variability of species abundances and aggregation in the design of MPAs. However, some points in methods and results could open the door to question the validity of the particular results. A major concern is the low goodness of fit of the habitat models. Explained deviance for most species is below 40%. Because most of the results are sustained in the prediction of these models, the low quality of them could be a major confusion factor for evaluating the hypotheses the manuscript implicitly proposed. I think the inclusion of new relevant environmental variables could be an useful next step to guarantee that

conclusions are supported by good-quality models. These variables could be related to the general habitat or can be selected specially for each species, the important point is select variables with clear ecological meaning, and so improve the quality of the models. In terms of the formal aspects of the manuscript, some parts need improvements. In reference to methods, authors indicated that the habitat modeling step is already published, but the inclusions of major details of the process would help to the reader. Currently, many aspects of the modeling require the reader review the previous manuscript. Also many clarifications are needed. For example, frequency of PELGAS cruises, distribution of the times each cell was sampled, the justification of the selected environmental variables, etc., need a better description and explanation. Reviewers also point out to some methodological steps that need clarification like the meaning of aggregation, the transformation of abundances to proportions, and the selection of thresholds. In general, this is a valuable manuscript with some interesting and opportune conclusions for improvement the role of MPAs. I think that including the previous points and clarification current concerns could be a major improvement of it.

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Reviewed by Ana S. L. Rodrigues, 2019-11-07 20:48

I commend the authors for an interesting and well-written manuscript. I have many comments, but they are mostly minor – see comments in the attached PDF file. They mainly cover suggestions for: - Improving wording clarity and consistency - Improving clarity in the presentation of the methods (including by clarifying better what is new in relation to previous work) - Additional points of discussion: o In some cases single species are modelled, in others multiple ones – why? And does it affect potentially the results? o For some species the "global model" was used, for others the "interaction model" - if I am understanding correctly, if the interaction model is better it indicates that (for that given species) there is significant variation in habitat preferences across years, which adds another layer of "dynamism" (over and above geographical variation across the years) o Variation across years is much discussed, but seasonal variation is never mentioned… o Model quality is never discussed, yet it is quite variable across species (implications for the results?)

Reviewed by anonymous reviewer, 2019-11-08 11:24

The manuscript is clearly written and has done choices in the presentation of material and methods and results which allow to go quite directly to the important points. It is also interesting to have a positive feedback on the MPA conservation approach in BoB. I think the discussion could go a bit further in discussing the possibilities for non-static conservation approaches, considering how this is introduced from the beginning of the paper.

Introduction.

Similarly to your definition of core areas of distribution, It could be useful to include a more precise definition of what you call aggregation level. I appreciate that you clearly write steps of research and briefly introduce the methodological approach. Could you also introduce the associated hypothesis in the introduction, rather than in the results part, where they are currently located ?

Material and methods

What is the frequency (over one year) of the PELGAS cruises ? You introduction the importance of the study in regard to the possible annual variability linked, among others, to breeding : is it possible to account for this with the PELGAS cruises ? What do those cruises represent from this variability ? In Figure 1.(a), the isobath – 4000m is not clearly visible (really thin).

Please add the SST abbreviation just after the first time you use "sea surface temperature".

I understand the modeling procedure has already been presented in a previous work. However, it seems valuable to include the results of the PCA in the supplementary.

Model selection by AIC shows that for storms petrels delta AIC was only 2 : how do you justify the choice of the global model rather than the interaction one ?

The description of the transformation from abundance maps to proportion maps could be reformulated to make it clearer. (First explain the prediction within the PELGAS stratum, them explain how it is used as a basis to compute the proportion ?).

The introduction defines the core areas of distribution as the ones containing 50% of the population. Here, you introduce the 75% threshold : you may explain why (did it appeared interesting during the statistical analysis ?). Why do you make this distinction here while you do not use it again in the rest of the paper ?

Results

3.1 –

3.2 Move the hypothesis presented here to the introduction. And maybe just remind them here. Maybe reformulate the hypothesis : "proportions of population covered" : we may not know at that point covered by what.

3.3 You propose a precise definition for persistent area at the end of point 3.4. : it would be more valuable to put it at the beginning of 3.3.

3.4 "We saw above that black-legged kittiwake core areas spatial extent was reduced, but their localisation quite variable" : please reformulate.

Discussion

"The relationship between the proportion of population and surface clearly showed that we had several species ..." : please reformulated to avoid "we had".

"As such, we confirm the potential of species with more aggregated distribution for zonal conservation, but species to be good candidate, these areas ..." : please reformulate, this is a bit unclear.

You point out that non-zonal conservation approaches might be more useful for some of the species. Could you develop this point ? What do we need to know for this ? Which temporal depth would we need to manage such conservation approaches ?

What do you think also about intra-annual variations ? Could we think about temporal evolution over the years and how could conservation through MPA approaches account for this ? (see small-sized shearwaters distribution predictions). Or is it already discussed in the Lambert et al. 2018 and just need a reminder ?

It might also be interesting to discuss the great skua results : r² of the GAM is the lowest and only one environmental covariate has a significant effect. What did you capture of the variations for this species and what are the consequences for the conclusion on its conservation ?

In the conclusion, you point out that an important result is that persistence and level of aggregation do not necessarily covary. This was not made so clear in the discussion and it seems valuable to make it clearer.

Your cover letter mentions that the methods would be easily transferable to other species. Why don't you mention and comment it in the paper too ?

Author's reply:

Download author's reply (PDF file)