General comments and overview

Marrot et al. provide an interesting study examining the impact of accounting for spatial autocorrelation on our interpretation of ecological variables that affect lifetime reproductive success in a wild clownfish population. Overall, I think the set of the study is interesting and a worthwhile pursuit, but I think the authors overemphasize the impact of their results on the biological interpretation. In the writing of the results and the discussion sections I find there is a disconnect between the results provided and the authors interpretation of those numbers. I think a stronger manuscript would emphasize both the similarities and the differences found between the two approaches. Or the authors need to provide stronger evidence that the spatially explicit model drastically changes the results.

The introduction could be improved to better guide the reader to understanding environmental drivers of LRS. Currently, there is a lot of emphasis on adaptive potential, and I'm not sure this manuscript really address this concept.

I think the underemphasized aspect of the study is the difference in variance explained between the adjusted R2 for both models. It seems to suggest that there is missing information that results in spatial autocorrelation and explains some variation in LRS. What are the hypotheses for this? Is it just many hard to measure environmental variables that are shared in proximity? How do we begin to understand it?

Specific comments

Please consider my comments below that range from small and specific to broader suggestions for manuscript.

Line 58-67: This section is not convincing. Many of the definitions are unclear. What does it mean to "respond positively by mean of phenotype or molecular changes to the environmental demand? What is the environmental demand? I'm don't think you've outlined what you are defining as adaptive potential. Please consider being more specific in your definitions.

Further, many of these citations show responses to climate change, I don't know if the demonstrate that populations with adaptive potential are coping with climate change? Again, it is unclear to me what you mean by "cope" and "adaptive potential" in this circumstance.

Line 68-69: I think this is a second definition of adaptive potential, but it might be better to be more specific and clearer in your 1st definition (Line 58) and instead here note how adaptive potential is/can be measured? I also think there might be some important nuance missed here, but I could be wrong. I think here you are referring to the additive genetic variance of fitness, but I think this measures the rate of adaptive genetic change and not necessarily the adaptive potential.

Line 69-71: Does environmental variation always reduce the response? I think it would not always reduce the response.

Line 78-82: It seems pretty important to reference Bonnet et al. (2022) in this paragraph.

Bonnet, T, Morrissey, MB, de Villemereuil, P, Alberts, SC, Arcese, P, Bailey, LD, Boutin, S, Brekke, P, Brent, LJN, Camenisch, G, Charmantier, A, Clutton-Brock, TH, Cockburn, A, Coltman, DW, Courtiol, A, Davidian, E, Evans, SR, Ewen, JG, Festa-Bianchet, M, de Franceschi, C, Gustafsson, L, Höner, OP, Houslay, TM, Keller, LF, Manser, M, McAdam, MG, McLean, E, Nietlisbach, P, Osmond, HL, Pemberton, JM, Postma, E, Reid, JM, Rutschmann, A, Santure, AW, Sheldon, BC, Slate, J, Teplitsky, C, Visser, ME, Wachter, B & LEB Kruuk. (2022) Genetic variance in fitness indicates rapid contemporary adaptive evolution in wild animals. Science 376(6596): 1012-1016.

Line 38- 39: To date, only **a** few studies have quantified ~~the~~ LRS across multiple generations in **wild** marine species.

Line 40-42: **Because** of ~~a~~ long-term sampling effort, such information is available for a population of the …

Line 42: What does PNG refer to? It is not clear to me.

Line 42: -43: Is this from previous work or from this study? I think, it should be clear from the summary alone.

> Please consider rewording as below.
>
> Previous work on the wild orange clownfish near Kimbe Island suggests that there is little adaptive potential and that variation in LRS is mainly driven by a breeder's habitat.

Line 48-49: Our ~~state of the art~~ spatially explicit analysis disentangled the role of these factors.

Line 52-64: Our findings imply that this clownfish population is susceptible to human-induced or natural changes in the spatial distribution and local assembly of anemone species. (Please correct to clownfish if it is the spatial distribution of clownfish that matters…but it is not completely clear to me from the current summary as written).

Line 57: Wild populations resilience to ~~worldwide~~ anthropogenic changes is….

Line 89-91: What possibility? It isn't clear from this sentence what possibility you are referring to? Please consider clarifying.

Line 91-93: Again, it is unclear what "it" is referring to in this sentence. Measuring fitness? Collecting pedigree data? Measuring additive genetic variance of fitness? Please clarify.

What about this work is convincing us that we are accurately measuring adaptivie potential/fitness/additive genetic variance of fitness? Is it credible intervals? I'm not following the logic of what was accomplished?

Line 93-95: What also showed? Previous work? Please be specific in this sentence.

Line 99-100: Again, important to consider the Bonnet et al. (2022) reference above and perhaps this reference below.

The quantitative genetics of fitness in a wild seabird M Moiron, A Charmantier, S Bouwhuis Evolution 76 (7), 1443-1452

Line 102-105: In what case? In the case of the Hendry et al. (2018) manuscript?

Line 108: ~~As mentioned earlier,~~ A previous study outlined that the habitat of the breeders - definted as the combination of their host anemone species and the lagoon where they live – contributes most to the variation in LRS…

Consider rewriting as, "In this system most of the variation in LRS is explained by a a breeders host anemone and the lagoon where they love."

Line 120: Is it spatial autocorrelation that generates similarity or is it shared environmental features that are measured by spatial autocorrelation?

Line 128-130: A meta-analysis conducted on 24 studies using linear regressions found that on **average model coefficients were biased by ~25%** when spatial …..

Line 135: Please consider the rewritten paragraph below to improve clarity.
There are two main aims of this study: (i) to build a spatially explicit model estimating the effect of environmental features on the LRS of clow fish, and (ii) to compare this spatially explicit model to a spatially naïve model to quantify the potential biased induced by spatial dependency. To accomplish these aims we used a geostatistical method that takes into account spatial autocorrelation at multiple spatial scales to disentangle the relative contribution of these effects independently from their spatial structure. Overall, we expected **these effects** to be overestimated when spatial autocorrelation is not taken into account.

"these effects" should be replaced by exactly what you mean when you say these effects. I think you mean the effects of anemone species, local density, and depth.

Line 163: "fishes" usually is the plural for multiple species, while fish is used for the plural of a single species. I think you are counting multiple individuals of clown fish here and not surveying other fish species.

Line 167-169: Is there any error rate is this size measuring method for sex and reproductive status? Is it always the relatively largest fish that is female?

Line 227: Because no ecological variable~~s~~ was to expected predominantly…

Line 235: Colon should be used to introduce a list.

Please consider the following rewrite to correct.

The model-averaging method is based on three steps: (i) the generation of all possible sub-

models from the set of predictors of interest, (ii) the selection of the AIC-based 95% best models (leading to 106 spatially explicit models and 13 non-spatial models, see geostatistics below), and (iii) the averaging of estimates **from** predictors among all selected models weighted by Akaike weight of each model that include**s** the corresponding predictor. Here our zero-inflated model included ten variables (the effect of our five variables on the probability that fish will not produce a self-recruit and the count number of recruits produced by breeders), leading to 1032 models generated.

Figure 2: Could you label the intervals with their associated depths?

Line 257-261: You mention the choice of this method is based on the literature, but what about this approach is favourable? Can you be specific about what aspect of performance you are referring to?

Line 265: This colon should introduce a list separated by commas as above.

Line 293: Examining table one the point estimates differ, but almost all confidence intervals overlap and the estimates seem to be in the same direction. I think you should avoid using the word affected…especially in the results section because there isn't much difference in the interpretation or the estimates themselves.

Line 344: I don't really see any evidence for the strong wording in this manuscript for this statement? Almost all of your estimates are similar (overlapping confidence intervals) between the two approaches. Please consider being more specific in exactly the biological interpretation that would differ between the two approaches (e.g. the effect of S. gigantea). I think this would be a more credible interpretation of the results.

It might be more convincing to evaluate whether the spatially-explicit model outperforms prediction of LRS.

Line 376: In our analysis, only the effect of life span….
I don't see any evidence for this in your results? How are you concluding that only life-span was unaffected by spatial auto-correlation? Please define how you are defining differences between the approaches in your methods.

Line 399: Please consider rephrasing to "Only a few examples from wild plant and animal populations indicate that spatial…

I think it would be worthwhile including in the discussion a paragraph on what exactly spatial autocorrelation is biologically. Sure, there are not many studies that use your approach, but there are certainly many studies that have show that different environmental conditions might affect selection or genetic variation. I think this is an interesting aspect of the study that could be brough to the forefront of the manuscript. You are finding that some of the variation is explained by spatial autocorrelation, and it seems we just don't have a grasp on this environmental features.

Line 419-431: Your exclusion of depth is not the most convincing evidence that depth plays no role in the number of recruits. RI is still high and the confidence intervals for the estimates for each model overlaps.

Line 438-440: Please consider the following rewording. However, anemone bleaching cannot explain our results because **evidence suggests that bleaching affects** all anemone species with the same magnitude (Hobbs et al. 2013) and no anemone bleaching event was reported over the duration of the survey.

Conclusion: Overall, I think the wording just needs to be toned down. I don't think the results provide as strong a contrast as your interpretation suggests. Please provide stronger evidence for the dramatic differences if you disagree.

Line 485: space between self and recruitment.

In number of recruits the intercept is not bolded as all other estimates with confidence intervals that do not overlap zero.

Consider increasing the dpi of the figures for higher quality figures.