ASSOCIATE EDITOR'S COMMENTS TO THE AUTHORS

The authors have revised their manuscript in line with the previous round of comments and this has made improvements to the paper. In some cases, however, they have not gone far enough. In particular, they have addressed the very critical points made by reviewer #1 mainly in the responses document, without this carrying through clearly enough into the ms.

I'd like to see the authors return to their manuscript a little more carefully. They may not agree with the criticisms of reviewer #1 but they need to do a better job acknowledging the proposed limitation or technical weakness and providing a better justification in the main ms supported by published evidence or examples of similar approaches. Future readers will not have the benefit of the responses file, so the main text has to be reinforced with the same arguments, supported by citations.

Some further clarification below in context of specific comments.

Thank you for the helpful suggestions to clarify concerns raised by reviewer #1. We have now added details and citations, which we originally outlined in our first response to reviewer file:

* Cited five large-scale studies as justification for combining survey methods, with three of those combining the datasets we analysed (McClanahan et al. 2011, MacNeil et al. 2015, Cinner et al. 2016, Graham et al. 2017, Darling et al. 2017), and explain how random effects were used to account for potential method biases
* Added citations on typical methods for measuring reef fish feeding behaviours, and detail that diver effects on fish behaviour were minimal (Choat & Clements 1993, Pratchett 2005, Feary et al. 2018)
* Added detail on matching the spatial scale of fish and benthic surveys
* Ensured that all reviewer #2 comments were addressed with revisions to the main text

Please see below for the new detailed text revisions and additional citations.

Rev 1 “The claim that point censuses yield the same data as belt transects is incredulous. The authors have themselves published work on the extent of diver effects. This misleading statement sweeps aside a vast literature that effectively and conclusively demonstrates the extent of diver effects when counting fishes. The UVC counting methods are a mixture of barely adequate (belt) and fundamentally flawed (point).”

AE: Response to this is inadequate. The authors have provided arguments, but made limited changes to the ms. They need to acknowledge some of these potential criticisms more clearly in the main ms, at least in passing, and justify their methods based on clear rationale and supporting citations.

We now incorporate our response to this critique in the main text:

* We cite studies which have combined survey methods, including three which use the datasets we analyse, and one which demonstrates that survey methods produce comparable fish biomass estimates: “*Because estimates of fish biomass using point counts and belt transects are comparable (Samoilys and Carlos 2000), these survey methods can be combined to infer large-scale correlative patterns for coral reefs (McClanahan et al. 2011, MacNeil et al. 2015). The datasets we analyse have also been combined in previous studies (Cinner et al. 2016, Graham et al. 2017, Darling et al. 2017).*” (L141-144)
* We explain that the random effect term estimates grazing rate mean and variance separately for each dataset (i.e. separating point counts from belt transects): *“Random intercept terms were used to account for different means and variance estimates for each dataset, and thus account for potential survey method effects (i.e. point counts in Seychelles vs. belt transects in the three other regions) (MacNeil et al. 2015)*.” (L253-255)

Rev 1 “The different methods for counting fishes and quantifying the benthos are put into the analyses under the assumption that they are effectively the same type of data. I do not accept this as a valid assumption.”

AE: The authors have argued against this point but without providing any supporting evidence in the form of citations. The criticism of the assumption needs to be acknowledged in the main ms, and addressed with support from 2-3 citations.

We have added supporting evidence to explain that benthic and fish data were collected at equivalent spatial scales, which is a typical approach for understanding benthic influences on herbivore assemblages, and also caution that a spatial scale mismatch could affect the strength of fish ~ benthic relationships:

*“Though the spatial scale at which fish and benthic metrics are collected may affect the strength of correlations (Wismer et al 2019), here benthic surveys were conducted adjacent to fish surveys and thus provided information on habitat composition at spatial scales which structure herbivorous fish assemblages (Russ et al. 2015, Nash et al. 2016b)”* (L223-228)

Rev 1 “For feeding behaviour fishes are allowed to acclimatise for 30s before following them for 3 minutes. This is not a credible means of quantifying fish feeding behaviour. Fishes either need no acclimation time (they are not scared) or no amount of time will suffice (they simply swim off). This is not rigorous best practice but an ad-hoc method with no quantifiable justification.”

AE: The authors' respond to this comment in the response letter but make no mention of this in the paper. They need to re-address the reviewer comment ensuring that in most cases, unless the comment is entirely whacky, they acknowledge the issue in the main text and justify their approach with supporting citations. At least some of the citations used in the response to this point need to be added to the manuscript to provide evidence that previous studies have used the same approach.

In the Methods, we now explain that a short acclimation period is widely accepted for behavioural studies (citing three reef studies) whichensures that any potential effects of diver presence on feeding are minimised, and note that less than 5% of fishes followed in the present study showed any negative response to the presence of a diver:

*“A short acclimation period is typical for reef fish behavioural studies (Choat & Clements 1993, Pratchett 2005, Feary et al. 2018), and here ensured that potential diver effects were minimized (<5% of fishes responded negatively to diver presence).”* (L182-185).

Please also check that all responses to Rev #2 have been dealt with in the main ms where necessary, rather than just in the responses file.

We have ensured that all reviewer #2 comments were addressed in the revision, with each comment resulting in one or more edits to the main text:

* Caveats for cropping feeding behaviour, data limitations and small vs. large-scale studies (L440-453)
* Unmeasured biodiversity influences (L475-484)
* Herbivory thresholds (L486)
* Statistical modelling details (L258)
* Methodological details, justification of explanatory covariates (L177, 210-219)