ASSOCIATE EDITOR COMMENTS FOR THE AUTHORS  
The two reviews I have now received identified some major limitations to this study, with one reviewer expressing particularly serious doubts about the quality of the data and analyses. Given that the results are largely confirmatory, based on potentially flawed methods, and much of the data has been previously published, the authors should perhaps return to their conceptual framework and analyses with a view to thoroughly re-working the manuscript in light of the comments received. The main questions are interesting but more needs to be done to refine the methods and clarify the  message.  
  
  
REVIEWERS' COMMENTS TO THE AUTHORS  
Reviewer: 1  
This paper looks at the relationship between benthic variables, estimates of fishing pressure and UVC-based fish abundances from which ecosystem functions are estimated. The extensive author list includes many of the world’s leading coral reef researchers. I therefore anticipated a conceptual or analytical breakthrough. Unfortunately, there are major problems with this paper. At a conceptual level the manuscript largely repeats earlier work. It represents yet another paper where fish numbers are multiplied by bites to estimate rates of presumed functions. This was extensively applied 10 years ago. The main conclusions from the paper have been previously reported, especially for parrotfishes, by Russ and Hoey. In this respect the paper is at best confirmatory.  
  
However, the main problems are methodological. Most of the data collection methods are fundamentally flawed.  
1.      The paper considers benthic correlates. Unless all surveys were in the same reef habitat the results are simply demonstrating the effects of habitat variability not the role of specific benthic components.  
2.      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).  
3.      To claim that the size of a fish can be estimated to the nearest centimetre from 7m distance underwater is likewise incredulous. I do not accept that this is possible with any degree of accuracy.  
4.      An 8cm size cut-off is bizarre. Especially for this paper as I suspect that most scrapers are croppers at this size.  
5.      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.  
6.      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.  
7.      A Bayesian modelling approach was used to estimate grazing values for the 63 species not included in feeding observations (=39). This means that the bite rates of almost two thirds of the species in the analyses were based on observations from the remaining third. This is not acceptable.  
Given the fundamental problems with the basic data collection methods other problems with the analyses and subsequent interpretations do not need to be addressed. The results and subsequent interpretations are irrelevant.  
  
  
Reviewer: 2  
This is an interesting and well written manuscript. Here, Robinson et al. explore the top-down and bottom-up drivers of herbivory on coral reefs using “snapshot” data from numerous coral reefs throughout the Indo-Pacific region. Employing elegant models, they tease apart the relative importance of habitat and fishing effects on this key process. By doing so using a macroecological approach (at large spatial scales), this work notably advances a rich body of literature on this topic. I commend the authors for their creativity and execution. Well done.

Thank you for your positive comments about the importance of our study, and the appropriateness of our statistical analyses.  
  
That being said, I take issue with certain elements of the study, which I think should be dealt with before the manuscript is considered further for publication:  
(1) The feeding data for croppers are poorly resolved (i.e., were only collected for a few species), thereby requiring extensive inference when calculating cropping rates. I appreciate the challenges of collecting data for all observed species, and would not advocate for more data collection here – however, I’d like to see a direct acknowledgement of this study limitation (i.e., some text on the issue, citing the relevant supp. table to point out the limitation, etc.).  After all, it speaks to the trade-off between conducting small-scale studies of herbivory (where such data are finely resolved but limited in spatial scope) and large-scale studies of herbivory (where such data are inferred for many species, but are much greater in spatial scope). I think this inherent trade-off should be addressed in the Discussion, thus giving the study greater context.

## Thank you for the suggestion to better caveat the results for croppers. In the Discussion, we add:

## detail on trade-offs between small and large scale studies

## comment on data scarcity for cropping feeding behaviours as a limitation, linking to Table S1 and Fig. S3

## identify cropping feeding function as an important avenue for further research

(2) Common to the field of “macroecology” is the study of local and regional species richness and community composition. While the authors argue for taking a macroecological approach to studying reef herbivory, they ignore differences among sites and regions with respect to herbivore community richness and composition (alpha and beta diversity). Fishing likely impacts both metrics by way of selectively removing large bodied species (which will in turn influence biomass and thus impact bite rate). See Lefcheck et al. 2019 (Science Advances) as an example where herbivore community biomass, richness, and composition collectively account for much of the observed variance in turf grazing rates on Caribbean coral reefs (that is, after statistically controlling for bottom-up habitat influences on herbivore bite rate). At a minimum, I think the potential role of herbivore community richness/composition should be briefly addressed in the Discussion (a sentence or two) as another potential source of unexplained variance in grazing rate in this study (see technical comment below).

Thank you for bringing this study to our attention. We agree that our approach overlooks potential biodiversity influences. We now discuss the potential roles of richness and community composition in driving functional rates at our study sites.  
  
(3) The authors mention ecosystem “thresholds” of herbivory in the Introduction, but do not touch on this concept again in the manuscript. It seems they possess one of the better datasets for identifying/proposing such thresholds, so I came away wanting more in this area.

Analyses of grazing thresholds typically test the hypothesis that benthic cover is predicted by herbivore biomass (i.e top-down grazing on benthic communities is the focus), and are best suited to time-series datasets where temporal changes in coral cover can be ascribed to temporal changes in grazing biomass (e.g. Graham et al. 2015, Nature). However in our study, we focus on the drivers of herbivore function, including both bottom-up benthic and top-down fishing influences. To clarify these distinctions, we now note that temporal analyses might be used to identify grazing function thresholds (LX), and emphasize that our analysis is focused on relative roles of bottom-up and top-down drivers of grazing functions (LX).  
  
(4) Lastly, the statistics need clarification (see below). I feel confident in the authors execution, but some additional information is needed to ensure that correct decisions were made.

Thank you for your suggestions to improve communication of the statistical models. We now demonstrate that model covariates were not collinear, and justify the use of fishable biomass as a metric of exploitation pressure that is not strongly dependent on herbivore biomass.  
  
Overall, I am very supportive of this manuscript – I think it’s important.  After some modest revision, I would advocate for its publication in Functional Ecology.  
  
Technical comments:  
line 65: Perhaps replace “restricts” with “limits”.

Corrected  
  
line 114: I think this statement is a bit misleading – it’s probably true with respect to cropping bite rate, but not true regarding the depth to which the fish community can scrape the reef. Due to morphometrics, it’s hard to imagine a case in which many small fish can scrape/excavate the reef to the same depth as a few large fish. In other words, gouging the reef is important to coral recruitment, and I just don't see how small fish would be physically capable of carrying out this function with the same capacity. Please clarify.

RESPONSE  
  
line 173: By whom? Unclear.

Added detail that A Hoey and A Graba-Landry conducted feeding surveys.  
  
line 174: reference Table S1, as it indicates which fish you did (and did not) survey with regard to feeding rate/behaviour.

Done  
  
line 233-234: I suspect that some of these predictors are highly collinear (e.g., coral cover and available substrate; fishable biomass and protection status). Have you tested for multicollinearity? How have you dealt with this issue?

We tested for collinearity using Pearson correlations (r) and variance inflation factors (VIF) in global models. No two covariates were strongly correlated (r < 0.5), and all covariates had low VIF (< XX). We have added this detail at LX.  
  
line 260: I’m concerned about the circularity of using herbivore biomass as both a proxy for fishing intensity (a predictor) and also a component of grazing computation (the response). Please briefly explain why this is OK.

Fishable biomass is primarily comprised of large-bodied predatory fishes that are typically targeted in reef fisheries. Fishable biomass and cropper/scraper biomass were also only weakly correlated, which indicates that fishable biomass estimates capture additional information on the exploitation pressure at each reef. We have clarified these details.  
  
line 290: Figure 2 is cited in the manuscript before Figure 1. Please revise the text, or the order of the Figures, to as to comply with the general rule of citing in the order of presentation.

Thank you for noticing this error. Figures 1 and 2 have been switched.   
  
line 321: One would expect this if biomass is being used both as a predictor and to compute the response variable. See comment above.

RESPONSE  
  
line 326: change “grazing” to “cropping/scraping”, for the uninformed reader.

Corrected.  
  
line 404: And also species identity/richness (which places bounds on biomass, in turn placing bounds on grazing potential).

RESPONSE  
  
line 437-449: Differences in the richness and composition of the herbivore assemblage may also account for some of your unexplained variance. See note above and example from Lefcheck et al. 2019.

RESPONSE