

This manuscript presents a fairly comprehensive examination of the feeding of a non-symbiont bearing cold water coral sampled from relatively shallow depths. The authors present hydrographic data and environmental samples for baseline ecosystem values, a feeding study (long term, and multiple values from diets), and a smaller and more brief study on starvation effects from reduced feeding intervals. I enjoyed reading this work and have a few comments. The MS could be more concise in places and the authors should consider when they are using vocabulary that is unnecessarily complex as there will be non-native speakers reading this work (eg. LN 437, evinced, when indicated will work fine, you forced a native speaker to have to look it up, also adroitly on LN 672, portend and heeded in the abstract). Please edit for clarity of language throughout.

The authors are quite thorough in their presentation of the *Artemia* diet that the corals were raised on, but they do mention that nauplii are hatched at monthly intervals. It would be useful to present the measured values of the *Artemia* across the timeline of the experiment as a supplemental figure, just to clearly present the lack of variability in the underlying food items being fed to the corals. I do not anticipate much variability here given the relatively clear asymptotic relationships presented in Figure 3, but I do think it is important to clearly present long term variability in the underlying food source. Alternatively to the supplement, you could just add values as small points along the indicated average values for the food items in Figure 3, but please ignore if it makes the figure harder to interpret.

Fig 3 and others as appropriate: The y axis range could use a few more indicators of value here, could easily go from 5 to 20 with the same number of interval ticks in between and make it easier on the eye when comparing against multiple diet and consumer differences. Multiple figures with odd range choices, so have a look through and see where interpretation could be made easier for the reader.

Regarding the starvation study, could you define the feeding regime as a percentage of your normal feeding rate so that you provide a more quantitative estimate of just how much less these animals were fed than their counterparts? It is fairly common in the literature to see conversations about % protein deficiency, and some effort in standardizing this feeding regime will make its inclusion in future meta analyses easier and also make it more likely to be compared against and included in future work. As is, it will be hard for future workers to feel confident assessing how much less they were fed, and it would behoove the authors to provide their assessment up front of just how much less was fed in the study.

I am surprised that I do not find a measurement of the DON d15N value included in this study. The authors have quite comprehensively measured values to account for most other potential sources, but have not provided a measurement to conclusively eliminate DON as a potential source (that I can find). Since one of the goals is to remove as many potential confounding variables from this accounting of feeding sources for non-symbiont CWC's, this measure may help to further eliminate or confound the author's conclusion, which would be interesting either way the measurement value turned out. Coastal waters in an upwelling setting, so it is my understanding that this should be possible from environmental samples.

Fig 5: I would like to see the local d15NO<sub>3</sub> values added to this figure, to conclusively display the 6-9 per mil differences that have inspired such a comprehensive effort. Even if the baseline was only measured in Aug 2021, showing the difference between the baseline values and the skeleton and tissue values will help to highlight the extent of the potential offset problem that you are highlighting in your conclusion. You could also include an offset in panel B, highlighting both the variability and how much larger the offset is.

Detailed comments:

LN 14: export is vague here, how about OM, or exported OM?

LN 15: Unusual to unusually

LN 18: probably worthwhile to specify that the coral is non-symbiont bearing up front, save readers having to look and specify the time period instead of long-term

LN 21: Is this turnover time correct? You state further on that none of the corals came to equilibrium with their diets across 400 days, so what portion of turnover does 291 represent then? Also is not apt is quite vague, I think you mean does not provide sufficient resolution to track seasonality.

LN 26: replace latter with phytoplankton

LN 28: lose portend and heeded, goal should be easy to read and interpret clearly for all readers, including the non-native speakers. Indicate would suffice and is clear for everyone. Further on, is it really a sensitivity, or just an unaccounted for feeding strategy that is fairly similar in other sites?

Intro

LN 41: belongs in methods, not the intro

LN 44: not :, should be . to end sentence

LN 58: intercalated...

LN 65: you use a semi colon, but I do not find the two parts of the sentence to be particularly clearly related, please edit for clarity

LN 70: ambiguous they, CWCs

LN 87: avoid excessive referencing, no need for 9 in the first and 7 in the second batch here to make this point. With this sort of coverage in the literature, it seems that these points are well contended, so pick the most relevant and go with those.

LN 93: I would specify zooplankton food web structure here, or lower trophic level structure.

LN 98: passed on instead of communicated

LN 105: fully to further

LN 106: Specify the non-symbiont bearing nature of the spp here.

LN 108-118: Edit for brevity, combine your experimentation with each question that was asked and clearly and concisely indicate why you performed two feeding studies, a size fractionation of zoops, and underlying measurements of d15N and hydrographic conditions from the environment.

Methods:

LN 205: Here is where I would specify just how deficient the diet was in the starvation feeding study.

LN 238: Go with intermediate instead of undefined

LN 262: I would be wary of reporting an SD of 0 for analytical replicates. You are probably better off here reporting the sample precision across your runs. Either way, please clarify your analytical precision for d13C and d15N of bulk materials.

LN 266: The denitrifier method is pretty standard these days, check the spelling on LN 266, and I think you can lose most of the detail here 266-274. Keep the reference standards used and the precision details, but a lot of this can go unless your aim is to establish the method for future reference here... At the very least, edit for brevity here.

Results:

LN 306: This model equation should be presented in methods and not in the results. I am somewhat skeptical of model output based on the fact that you have pretty clear visual evidence that animals did not completely come into equilibrium with their diets. Some discussion as to the assumptions within the model that lead to the unexpectedly short turnover time estimate and divergence from your model output from the well-graphed results is probably appropriate and currently missing from either LN 410-419 or LN 484-492.

Discussion:

LN 407: make this statement more clear, incorporate the data supporting your point into actual sentences and not a jumble of somewhat disjointed text in sub brackets. It is one of your main points, so presenting it clearly here serves both the authors and the readers best interests.

LN 419-425: The authors confirmed that the diets were of equal quality, so take this background information and find a place in the intro for it where you discuss diet quality effects, it is quite unnecessary here as it is not directly relevant to the conditions in your study.

LN 428: some discussion and some clear indications as to why your model indicates turnover in about half the time that your feeding study data does would be appropriate. IMO, the model results are the weakest part of your feeding study here, as at 490 days your CWCs are 3, 3, 2, and 1 per mil greater than their corresponding diets... This model output needs to be revisited, IMO.

LN 526-532: This number of references are not required to demonstrate this point, IMO.

LN 537: might define relative pore size for GFFs that define the cutoff of SPOM.

LN 544-547: Seems redundant from the introduction where you set up the background that was necessary for this study, please edit for brevity.

LN 566: This is lengthy enough again to feel like more broad review of literature that feels more appropriate in the introduction than at length in the discussion. A comparison against two to three studies of similar spp is adequate, IMO. LN 573-575 would suffice. Please edit for brevity.

LN 586: Your study spp was sampled shallowly, I do not find the the extensive discussion of circulation in cold water reefs here as particularly relevant to this study. You have nicely demonstrated size dependent usage of zoops in this setting, but I would stick to a tighter discussion of AA evidence of zoop use and not stray into deep circulation on cold water reefs, the lengthy speculation distracts from the author's solid results.

LN 613 to 628: edit for brevity.

