

Overall thoughts: Overall, I found the manuscript to be clear and well-thought out in terms of their methodologies. One concern was the amount of information provided in the Results section which was not adequately elaborated on in the Discussion. There were a lot of figures and tables which I don't think are all necessary in contributing to the story the researchers are trying to tell. I also thought that the Introduction could be improved through talking more about expected results and specific hypotheses. Finally, I have a concern about the Pacific "control" used in this experiment, in which replicate samples were not taken, and the authors didn't discuss potential behavioral differences of the zooplankton in the mesocosm enclosures vs. at the Pacific "control" site.

Abstract and Introduction, general comments:

- In the Abstract, they presented a lot of results and not much interpretation of those results. I would have liked to see more interpretation / key takeaways from their results. Their concluding sentence in the Abstract is about how further studies are needed to answer their research questions, but what are their main conclusion(s) that they are able to draw from their results?
- Overall, I thought the Introduction made it clear what the researchers were seeking to study.
- However, I would have like to see some hypotheses laid out about what they expected to happen over the course of the experiment. A lot of their results were presented in terms of changes from Day 0 to Day 48 of the experiment, but I don't think the Introduction did enough to set up hypothesis on what they thought would happen across this time frame. Similarly, I would have liked to see more concrete predictions about anticipated differences in results for the moderate OMZ vs. extreme OMZ vs. Pacific "control."

Abstract and Introduction, specific comments:

- Line 5: Remove the word "how" or "on"
- Line 17: The wording of the sentence starting with "Possibly" makes it seem like this is their guess about what the major diet of copepods was - did results show this, or is this just a guess?
- Lines 61-62: Move the citation for Garcia-Reyes et al. 2015 to the end of the sentence
- Line 80: I think they should set up in the Introduction what the "two different types of OMZ waters" are. They just mention that they do two types with "different OMZ signatures," but I think they should elaborate on this and say explicitly that they are simulating upwelling with differing inorganic N:P ratios (extreme and moderate OMZ signature).

Methods, general comments:

- I would have liked to see more information provided about the Pacific "control" site where they also collected samples from - they should give more details on the location of this site and possibly other characteristics, such as its depth, distance from the mesocosms, etc. Its location should be added to the map in Figure 1. It seems like at this Pacific "control" site, no replicate samples were taken (indicated by no error bars for the data from Pacific), which I am hesitant about. Can this really be considered a "control," since it wasn't under the same conditions as the mesocosms (enclosing the zooplankton) - would they expect to see behavioral differences in zooplankton that are enclosed vs. not enclosed?
- Were zooplankton communities evaluated in the water that was added and removed from the mesocosms? I think this would be helpful to know when considering community changes over time after water addition and removal.

Methods, specific comments:

- Lines 111-115: Were these trends that determined the 3 “phases” consistent across all eight mesocosms and the Pacific “control” site? I would have liked elaboration on whether all of these showed the same trends which determined these “phase” definitions.
- Lines 123-124: They sampled zooplankton in the afternoon only, so I would have liked to see them discuss somewhere whether DVM of the zooplankton could have influenced their results - perhaps a lot of the zooplankton were near the bottom of the mesocosms during that time that they sampled.
- Lines 133-135: I would have liked to see an explanation for why they changed the depths they sampled microzooplankton from Day 20 onwards. Why did they stop sampling the 17-10 m depth interval?
- Figure 2: Legend should say what % CIs are shown. (I assume 95%, since that’s what the rest of the figures use, but should be stated explicitly.)
- Line 168 and Figure 2: Why were only female *Paracalanus* sp. used for this analysis? Why not males and females? I didn’t think this was explained well enough. Same comment for Line 210 about fatty acid analysis.
- Line 187: This is a pretty high variance in sample size (between 8 and 52 individuals) - did they find any correlation between sample size used and their results on gut clearance rate?
- Line 198: Remove period after *Paracalanus*
- Line 214: What was the minimum number of individuals? They state 80 as the maximum, but what minimum did they use?
- Line 295: Add a citation for the R Core Team

Results, general comments:

- This study has a lot of results and a lot of figures. I feel like overall, the text could be reduced. I noted a few chunks of text that I felt contained too much listing of data without any elaboration: Lines 330-337, Lines 369-383, among others. If these results aren’t explicitly discussed further in the Discussion, I don’t think it’s necessary to list them if they are already contained in a figure.
- Another general comment on the Results is that I didn’t see many statistical test results (p-values, F-values, etc.) presented in the Results section to back up their claims. For example, statements like those on Line 309 could be backed up with a relevant statistical test result.
- Significant issue with Figure 9 that needs to be fixed: Either the color scheme or the legend needs to be fixed on this figure - on all other figures, red signifies extreme OMZ mesocosms (M1, M4, M5, M8) and blue signifies moderate OMZ mesocosms (M2, M3, M6, M7). On Figure 9, the legend is different and either the points are colored wrong or the legend is wrong, since it’s implying that M1-M4 are one treatment and M5-M8 are another treatment, which is not the case.

Results, specific comments:

- Figure 4b: In my opinion, the color used for Echinodermata and the color used for Others were difficult to tell apart on the figure, which made it difficult to interpret.
- Figure 4b: I think they could add a label for the “moderate” column and the “extreme column” above each column, and then just have “M2”, “M1”, etc. in each panel, to make this easier to interpret.
- Line 322: Why were fish eggs added to the mesocosms on Day 31? This should be explained further and discussed in terms of whether it could impact any of their results.
- Lines 342 and 345: In my opinion, the phrases “of some importance” and “of minor importance” indicate some level of influence of those communities. If the authors meant to indicate just

abundance or biomass contributions, they should say that instead of stating things in terms of “importance.”

- Figure 5 caption: “genera” instead of “genus” in the second to last sentence
- Figure 6: The different colors here aren’t really necessary since the panels are broken up by genus. If they wanted to, they could just have a black (adults) and grey (copepodids) line for each panel.
- Line 390: Confused by what they mean by “M2 and M3 reached biomass maxima for both copepods and polychaetes” - what maxima?
- Figure 7: The caption says there are vertical lines on the figure indicating the three phases, but these lines are not on the figure.
- Figure 7: Why not do averages and confidence intervals like previous figures? Why have a different line for each mesocosm? Do we really need to see variation between single mesocosms, or would it convey the same information if the mesocosms in each treatment were averaged like in Figures 3, 4, and 5, with error bars?
- Figure 8: Same comment as Figure 7 - can the replicates in each treatment be averaged?
- Line 423-424: Add citation(s) for the sentence “This is a common problem in small copepods with rather low total lipid mass close to the detection limit” to back this up.
- Line 425-426: What proportion of samples hit this purity cutoff?
- Table 2: Maybe this could go in the supplemental information
- Section 3.4.2. Fatty acid and fatty alcohol composition: Lots of information in this section that isn’t set up adequately in the Introduction or elaborated on adequately in the Discussion
- Lines 483-494: Were these Pearson correlations calculated with abundance or biomass values?
- Figure 9: Since these results are discussed in terms of the 3 phases, I think the 3 phases should be indicated on this figure with vertical lines.

Discussion, general comments:

- To me, the Discussion felt slightly disjointed. I think it could be improved by strengthening the first and last sentences of each paragraph, to connect the flow of ideas and provide the reader an indication of what each paragraph will be about from just reading the first sentence of each paragraph.

Discussion, specific comments:

- Line 524: Elaborate on “lower than expected” - what values for N concentrations were initially expected, and why?
- Table 3: Pretty low sample sizes - I would have liked to see more discussion of this and justification for these results.
- Lines 604-632: Very long paragraph - try to cut down or break up into multiple paragraphs
- Line 686: The information in the parentheses here seems disjointed and confusing
- Line 690-691: I would take out the “but implementation....more time and manpower” part of this sentence. In my opinion, ending with this statement diminishes the results that they found in this study. I would rather have them end the Conclusions section with a final take-home message illustrated by their results. But this is just my opinion - up to the authors.