

Biogeosciences Discuss., author comment AC3  
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## Reply on RC3

Elliott L. Price et al.

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Author comment on "Sensitivity of plankton assemblages to hydroclimate variability in the Barents Sea" by Elliott L. Price et al., Biogeosciences Discuss.,  
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Thank you very much for this in-depth review and we appreciate the time taken to analyse our manuscript to this extent. Below you will find our responses to each of your comments.

**In response to "I agree with Reviewer 1 that the region of study is not "the Arctic"..."**. We concede that the language we have used throughout is misleading and that the study region is not primarily an Arctic region. We do, however, believe that, due to its exposure to the polar night and the subsequent impact this has on the base of the food web, this area could be defined as sub-arctic. As we responded to RC1, we will revise the wording throughout our manuscript to reflect the region's definitions as sub-arctic.

**In response to "I found it strange to pool phyto- and zooplankton communities..."**. We grouped these phytoplankton and zooplankton communities together in the same analysis for the reason that you stated - that one group may influence the other. Because of the potential top-down control of phytoplankton by zooplankton, and the potential bottom-up control of zooplankton by phytoplankton, the pCCA, to our knowledge, should reveal the relationships between phytoplankton and zooplankton species by assigning them on opposite ends of each axis.

**In response to "After thoroughly reading the paper several times..."** We acknowledge your point that the findings of plankton being interannually vulnerable and related to water masses are not novel and therefore not the main finding, and we will revise the manuscript to provide a measure of variability based on the abundance of the plankton species, rather than, as you state, the ordination axes. We tried to definitively state which water mass properties were impacting the plankton communities, however we agree that this is perhaps not clear enough in our analysis so we will revise the analysis and the conclusions of our paper to make the environmental associations clearer. To accomplish this, we will carry out a PERMANOVA on the underlying distance matrices produced from the ordination analyses to understand whether the distance to the centroids are different between groups (between year and region).

**In response to “The presentation of the results is messy and confusing...”** We will move tables displaying model output from the main text and into supplementary as you correctly state the displayed information is already visualized in the ordination plots. We will also display all the pCCA axes in ordination plots regardless of whether there was apparent variability between groups for each axis. We acknowledge that the pCCA outputs are not scientific findings in themselves, and, as stated in the previous response paragraph, we will use PERMANOVA to assess differences in the ordination outputs between groups. Furthermore, we will clarify the questions we are asking and the hypotheses we are testing in the introduction to help sharpen the conclusions and improve the understanding of the results to the reader.

**In response to “The main (and really only) tool the author apply is a pCCA...”** We will amend our description, use and interpretation of the pCCA analysis throughout as to represent the technical aspects of the analysis more accurately.

**In response to “Moreover, the way the data was treated is not clear.”** We removed species that represented less than 5 % of the total zooplankton count and 5 % of the total phytoplankton count per sample, not the entire dataset. We will make this point clearer in the text.

**In response to “Line 156. “Species counts in both phytoplankton and zooplankton datasets...”** We took the highest abundance for each species and set it to 1, then adjusted the other counts for that species accordingly. So we standardised the data matrix by column (species) rather than row (sample), the latter method would, as you stated, conserve the ratio between species and be not be a relevant method. Whereas standardising by individual species should mitigate the problem that arises in the example that you provided with *Oithona* and *Calanus*. Thank you for shedding light on this uncertainty and we will alter our wording accordingly.

**In response to “You standardised your abundance values...”** The environmental matrix is automatically standardised when running the pCCA from the ‘cca’ function in the R package ‘ade4’. We will make this clear in the text.

**In response to “Line 111 onwards - you said you use SST separate into 3 regions based on water masses...”** The regions were separated based on both SST and Sea Surface Salinity values averaged over the time series which is stated on Line 113. These groupings are consistent with water masses identified from CTD depth profiles observed in that region – please see Tuerena et al., 2020 *Biogeosciences*.

**In response to “Line 132 We selected a suite of environmental variables that have previously...”** Thank you for your suggestion and we will include citations back up our selection of environmental variables.

**In response to “Plankton are not stationary, they do not hang out in one spot for one year...”** We agree that the consideration of SST in previous summer and previous autumn were not valid predictors given the strong role of advection in the system. Whilst we could add in upstream data, we agree and believe that this would introduce too much uncertainty and increase the likelihood of generating false positives. We will therefore remove this variable from the dataset and re-run the analysis. However, we will leave in the SST from the spring as this provides information on the conditioning of the bloom at the start of the season and will have influenced the ecology at the time of CPR sampling in June.

**In response to “Line 161-166 What is the purpose of all these additional tests?”** On further reflection we see that examining the correlation between the species and hydroclimate axes is redundant given that multiple regressions are intrinsically used in the pCCA model itself to associate the hydroclimate variables with the plankton species. We will also replace the ANOVA’s to assess differences between years and regions with a PERMANOVA that is a more robust analysis when dealing with between group differences on the back of ordination techniques.

**In response to “I don’t understand this. What did you use a zero-inflated negative binomial regression for?”** It is apparent that our understanding of zero-inflated datasets was inaccurate and therefore the application of zero-inflated negative binomial regressions to assess seasonal abundance was unwarranted. As you state later in your review, this section seems misplaced and isolated - since the results of this section only inform a few minor discussion points, we will remove this analysis from the manuscript. We will, however, show the raw abundance data across the seasons in a supplementary figure to support the minor discussion points.

**In response to “For all the plethora of tables and figures and text, some basic information is missing...”** We will remove the tables displaying much of the pCCA output, and shift this into a data file in the supplementary information. This will be replaced by tables showing the inertia of the axes and the significance of these. We chose axis 1 and 3 for ST1 as these were the axes that described a community that were interannually variable. In our revision we will present the first three ordination plots for each pCCA carried out, remove much of the results texts that is describing the patterns that can be seen in the plots, and provide greater detail on the technical outputs of the pCCA that describe its performance.

**In response to “I don’t really understand how you infer “variability” of species from axes scores...”** We inferred variability of species by assessing which species most strongly contributed to the pCCA axes that showed interannual variability in the ANOVA. However, now we see that the pCCA doesn’t produce scientific findings in itself, we will assess variability of the species contributing highly to the pCCA axes by running ANOVA’s on the abundance of the species of interest identified from the pCCA. We will conclude levels variability based on the summary statistics produced from the ANOVA’s, this also has the added benefit of being able to relate the variability to actual abundances and not arbitrary axes scores so thank you for the suggestion.

**In response to Section 3.2 comments.** We will include ordination plots and the statistical outputs from the pCCA.

**In response to Section 3.3 comments.** Since the results of this section only inform a few minor discussion points, we will remove this analysis from the manuscript. We will, however, show the raw abundance data across the seasons in a supplementary figure to support the minor discussion points. We will also revise our use of the term phenology and use seasonal variation instead.

**In response to "I am missing here a discussion - and comparison"** We will provide a more in depth comparison with studies other plankton community composition studies from the Barents Sea region.

**In response to "Line 261. I would hardly qualify a "high interannual variation"** We will amend the standout finding of the paper once the changes to the analysis are complete and make sure that any quantifiable findings are reported in the main text. For example, showing the high level of variability in *C. finmarchicus* using boxplots and ANOVA's as you suggested in your review.

**In response to "Line 265-268. This is an overly simplistic view."** We will elaborate on the implications of *C. finmarchicus* replacing *C. glacialis* to reflect the complexity of mechanisms relating to lipids in marine food webs.

**In response to "Line 285- . SST the year before at this location would not be influencing the community..."** We agree and we will remove the SST of the previous seasons from the analysis.

**In response to "For all the talk of Atlantification in the introduction, one very important..."** We will revise the discussion of the manuscript to reflect the strength that advective processes play across the Barents Sea Opening, and were appropriate, try to avoid discussion points that treat our regions as self-contained boxes rather than interacting regions. Specifically, we will add a section addressing how advection could have impacted the results and the implications of this on the wider ecosystem.