General comments:

Talmy et al. Biogeosci notes

The authors use a simplified N-P-Z-D model to examine the influence of food web structure and mortality closure form on plankton community composition. They found that total phytoplankton biomass and plankton community composition were impacted by food web structure and the form of losses on the microzooplankton grazers. Results demonstrate the quadratic mortality is most consistent with the linear scaling of phytoplankton and microzooplankton biomasses seen in observations and suggest that parallel food webs may be more appropriate for representing the coexistence of small and larger phytoplankton. This simplified experimental design allows for clear understanding of model structure influences ecosystem dynamics and of how much model complexity is required to capture observed biogeochemical properties. The results are bolstered by comparing model results to observed patterns (e.g. the linear scaling of phytoplankton and microzooplankton) rather than individual biomasses alone. I think this would be valuable to the BG readership.

The paper addresses relevant scientific questions within the scope of BG. The concepts, ideas, and tools are not necessarily novel, but their implementation and the comparison with recent observational datasets are. The scientific methods and assumptions are valid and clearly outlined, the results sufficient to support the interpretations and conclusions, and substantial conclusions are reached. The description of experiments and calculations are not sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results). Additionally, the authors do not give enough credit to related work, though they do clearly indicate their own new contribution. I do not think that the title clearly reflect the contents of the paper, specifically the use of the term "top predator." The abstract provides a concise and complete summary, the overall presentation is well structured and clear, and the language is fluent and precise. All mathematical formulae, symbols, abbreviations, and units are correctly defined and used, though parameter values, which I think are necessary, are not given. As such, the amount and quality of supplementary material is inappropriate. However, some formulae in appendix can be abbreviated. Finally, the number and quality of references are appropriate.

We are grateful for this careful review and constructive criticism of our work. We are preparing a revised text, and in it we will provide an expanded supplemental information with appropriate sensitivity studies and model output, specific parameter values, and more detailed representation of our allometric size-rules. We will also change the title in line with the reviewer's suggestion. We provide more details of our planned revisions below.

Specific comments:

1. Title and terminology
I would argue this term is misleading because microzooplankton are rarely considered top predators, even if they are the end of the food chain as modeled in this paper and others. Most people think of a top predator as a carnivore with few or no predators themselves; e.g. L64-65: "higher predation on the top predator" is counterintuitive. "Highest predator," (L136) "highest predator modeled/represented," or "terminal predator" may be better.

We plan to change the title to: "*Killing the predator: impact of highest predator mortality on global ocean ecosystem structure*"

2. Take-away message

2a. L19: "importance of parameterizing" Density-dependent mortality on the terminal predator has been included in these models for decades. The closure term was always chosen

to get plankton dynamics "correct" (near those observed). I think that L41 may be the more relevant significance of this paper, i.e. understanding "how much model complexity is required to capture biogeochemically relevant properties." Specifically, the underlying drivers of linear scaling between microbial predators and prey are expounded.

This is an interesting point. We plan to rewrite the abstract to focus on the closure as an explanation of linear predator-prey scaling in models with minimal complexity.

2b. L44-46, L50-52: I really like the use of relationships/patterns found in observations to inform modeling or to assess models. I think this is an underutilized tool in the field. Such comparisons have been made by Luo et al. (2022) and Petrik et al. (2022), albeit chlorophyll instead of phytoplankton biomass, but they should be cited.

Thank you – we will be sure to add these important citations

3. Comparisons with ESMs

3a. L66-67: I think this statement is false. BGC model developers put a significant amount of effort into calibrating mortality equation structure and parameterization. Often they are trying to capture simple metrics like total NPP or the spatial patterns in the global distribution of chlorophyll, but others go beyond that by calibrating against plankton community composition, Z:P spatial patterns, seasonal plankton biomass patterns, z-ratio, e-ratio, etc. (see Aumont & Bopp 2006, Aumont et al. 2015, Stock & Dunne 2010, Stock et al. 2014, Yool et al. 2013, 2021). Following on this, the authors have shown how phytoplankton community composition can vary based on food web structure and zooplankton mortality form. Do these choices also influence other important BGC quantities like NPP, export production, and the amount of secondary production available for zooplankton consumers?

We will be sure to cite these important works in our revisions and explain how our work builds on the foundation they have set. We are also in the process of outputting additional BGC quantities. We have NPP, zooplankton biomass and carbon export. We are a little unclear what is meant by the 'amount of secondary production available for zooplankton consumers'. We have tentatively assumed that this refers to the quadratic closure (as this parameterizes losses from microzooplankton to higher trophic levels, e.g. mesozooplankton). However, we would welcome clarification as we prepare our revisions.

3b. L95-96, L105, L264: These statements suggest that both parallel and diamond food web models are very prominent in the OBCG component of ESMs. How common is either of these? And is one more common than the other? It would be nice to have more quantitative idea of this. How many global ESMs only have microzooplankton (not one zooplankton that represents both)? And how many of them have parallel vs. diamond feeding? You could focus on just those that participated in CMIP5 or CMIP6. For example in Rohr et al. (2023): 3 with only 1P, 1Z (HAMOCC, CMOC, WOMBAT); 2 diamond (OECO, MARBL); 2 parallel (CanOE, COBALT); 3 hybrid (MEDUSA, PICES, BFM). Or Kearney et al. (2021): 1 with 0P, 0Z (BLING); 4 with 1P, 1Z (CMOC, WOMBAT, MRI, HAMOCC); 3 diamond (OECO, MARBL, NOBM); 2 parallel (CanOE, COBALT); 2 hybrid (MEDUSA, PICES). I don't expect the authors to list all these details in the paper, but suggest at their prevalence (e.g. ~30% of CMIP5 ESMs).

This is a helpful suggestion. We plan to discuss these differences in food-web structure between Earth System Models in our revisions.

4. Missing information on experiment design and parameters.
4a. L109-110: Are the growth and grazing parameters different by size? Where can I find these?

Yes, both growth and grazing rates are allometric. The rules used are specified in Ward et al. (2012), and we will provide a table with them in our revisions – apologies for this omission.

4b. L114: What is the sensitivity of the results to these assumed sizes? I would argue that when ESMs use only one zooplankton type, it is supposed to represent both micro and mesozoo. Similarly, when there are two zoo types, one is micro and one is meso, and the meso preys on the micro, which is missing from the parallel food web here. Also, the "large phytoplankton" here is barely the size of the diatoms in ESMs (10-100 um, "microplankton"), while both the small and large microzooplankton are also at the low end of the microzooplankton (10-200 um in ESMs, 2- 200 um in Sieburth et al. 1978), and when there is only one zooplankton group in ESMs it tends to encompass everything 10-2,000 um. I do not mean to suggest that this study is without value for that reason, but the comparison to global ESMs used for climate change studies is less direct.

Interestingly, there seems to be a great deal of variation regarding what is being described in ESMs. From Rohr et al., 2023: "*it is concerning that some models imply something statistically similar to an ocean filled entirely with very slow-grazing meroplankton larvae (MEDUSA2.1, OECOv2) and others an ocean filled entirely with very rapidly-grazing ciliates (MARBL and CMOC).*"

We are currently conducting sensitivities to assumed phytoplankton and zooplankton size classes, that we plan to include in our revisions. We will be sure to interpret our output in the context of the existing range of organism sizes represented in ESMs.

4c. L127-128: Is the linear mortality term included in both versions? Or is it either linear or quadratic mortality? It is rare for ESMs to have either linear or non-linear mortality terms on their zooplankton. The linear loss term in these models usually accounts for metabolic losses while the non-linear term accounts for higher predator mortality (see Kearney et al. 2021, Petrik et al. 2022). This is alluded to on L276-279.

Great question, we did have the linear turned on with the quadratic but have since conducted a sensitivity where we switch it off. Turning off the linear in the quadratic case appears to have negligible impact on the properties we report, indicating that quadratic closure may have an outsized impact on ecosystem properties.

4d. Table 1: Did $\delta_Z$ and $\delta_{ZZ}$ have the same values in the parallel and diamond food webs? Or all the parameters for that matter?

Yes, we will clarify this in our revisions.

4e. Figure 2: Are the arrows meant to line up with (phyto→PON) and (zoop→DOP) or is that just a coincidence?

This is a coincidence – we will clarify this in our revisions.

4f. Appendix Equation A34: What are $g_{max}$ and $K_{1/2}$ for the two microzoo types? Are all parameters held constant between the model version?

Yes, all parameters are held constant. We will report all parameter values in our revised manuscript

4g. Table A3: The parameter values pertaining to zooplankton mortality and grazing should be given here since that is the focus of the paper. Also, I could not find these parameters on the website listed. Please give the filename and folder/directory with these values.

We will add these values to the manuscript and also to our GitHub repository.

4h. Table A3. "The large phytoplankton has a faster maximal growth rate and higher nutrient half-saturation constants than the small phytoplankton, representative of differences in growth rate between a eukaryotic algae and a cyanobacteria, respectively (Litchman et al. 2007; Ward et al. 2012)." Is this true of the microzooplankton when there are two size classes?

Yes, the larger zooplankton have slower maximal growth rates consistent with allometric scaling. We will report this in our revisions.

5. Other

5a. L102-104: How do the model results presented in this paper compare to observations in the Pacific (e.g. Follett et al. 2022)? This seems relevant as much of the early literature cited on linear vs. quadratic mortality were trying to capture the difference in the seasonal cycle between the N Pacific and N Atlantic.

This is an interesting question. We are currently trying to identify a dataset in the Pacific with microzooplankton and phytoplankton carbon density. The Hawaii Ocean Time Series appears to only have mesozooplankton (>200 micron). Although some datasets include microzooplankton carbon (e.g. https://doi.pangaea.de/10.1594/PANGAEA.779970), it is unclear to us if these have coincident phytoplankton carbon density associated with them. We will continue to search for appropriate data in the Pacific.

5b. Figure 3 and results: Why just surface biomass? Aren't plankton distributed throughout the euphotic layer? Subsurface biomass could show different patterns. I suggest analyzing and depicting depth-integrated biomass.

We have already prepared depth integrated figures and will include these in our revisions

5c. Figure 3 and results: Could you please also show the microzooplankton biomass across the different models? I am curious how much it showed similar patterns to the phytoplankton, which would be expected for the quadratic closure, but not for the linear. This would help understanding the spatial differences that can't be seen in Fig 5.

We have zooplankton biomasses that we will provide. Note that earlier versions of the manuscript had the figure below, showing how carbon accumulates in the different groups. There are interesting differences in the way carbon builds up, with linear closure leading to far tighter constraint on the phytoplankton, and greater accumulation in the zooplankton (top row). We omitted this figure as some readers find it confusing. The maps we are preparing hopefully will depict this in a more intuitive way.

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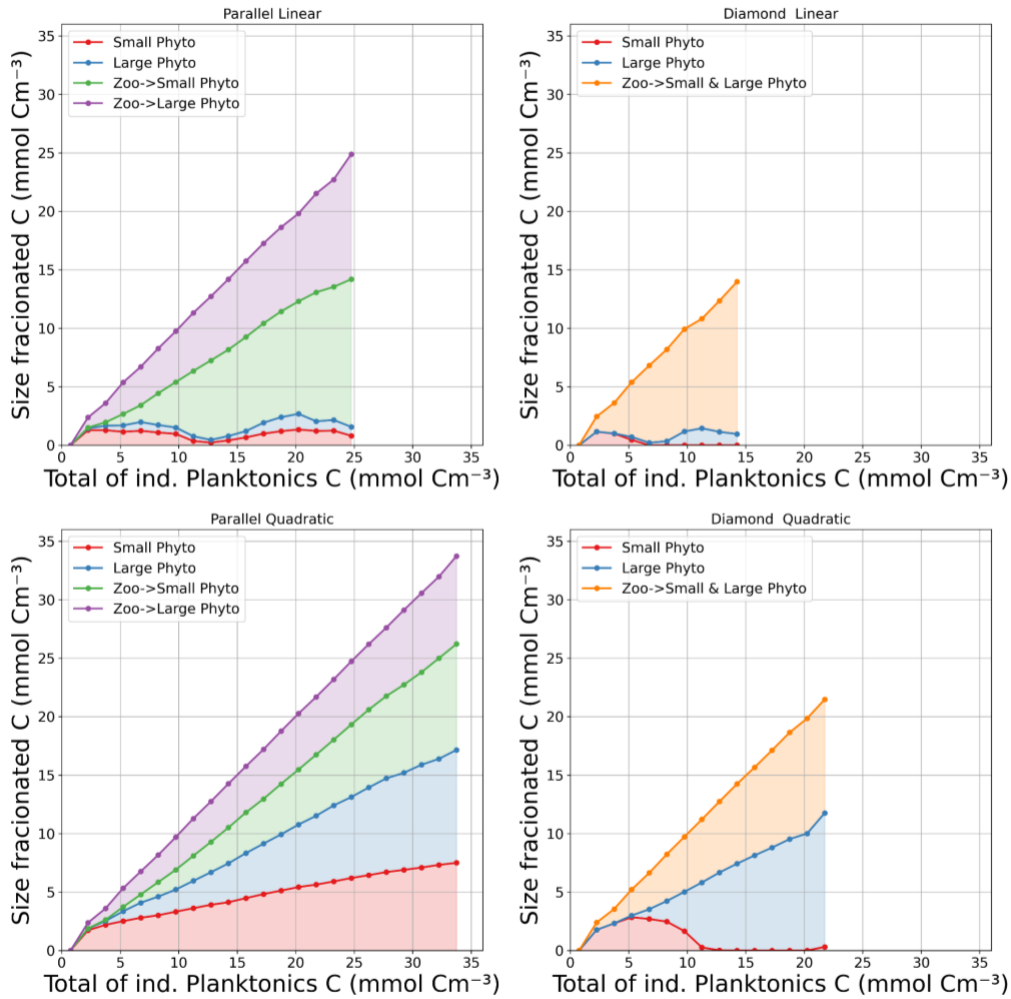
5d. L230: Linear mortality resulting in oscillations is known behavior. Please mention in the discussion and refer to the relevant literature (e.g. Steele & Henderson 1992, Fasham 1995, Edwards & Brindley 1999, Edwards & Yool 2000).

We will add these citations.

5e. Figure 6 and results: The statement that "Linear losses on the microzooplankton predict cyclic behavior in the predator-prey relationship that are inconsistent with observations" is not accurate. The slope of (a) and (b) are inconsistent with the global pattern, which are snapshots in time, but it is possible that a similar seasonal pattern exists in the observations. You would need to show those. Given the wealth of data in the English Channel from the CPR, I assume this is possible.

We will show the seasonal pattern at L4 – it indicates that phyto and zoo biomass is very highly correlated.

Technical corrections:

Appendix Equations A1-A10: These are all the same form and don't all need to be shown. You could simply use an "X" or something to denote the nutrient on DOM. Also, A1 has NO3 in the equation, but should be NH4 throughout.

Appendix Equations A11-A14: Again, could just use one example of POM.

We'll be sure to fix these.

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