Reviewer 2

The manuscript “Trophic amplification of Southern Ocean plankton emerges from changing seasonality” by Tianfei Xue et al. investigates plankton trophic amplification in the Southern Ocean until the end of this century under SSP5-8.5. Trophic amplification is prevalent in the subtropical zone and the sea-ice zone, but with opposite patterns. This study highlights the importance of seasonal research, as some phenomena are concentrated in specific periods. However, some analyses and interpretations of the mechanisms of Southern Ocean trophic amplification in this manuscript lack scientific logic and evidence. I will list some points that I am concerned about, which will enhance confidence in this manuscript if you can address most of them.

We would like to thank you for reviewing our manuscript and your valuable feedback and constructive comments. Below we address each comment individually. Reviewer comments are in black, our responses in blue, and planned changes to the manuscript in *italics*.

Major comments:

The manuscript uses the multi-model mean throughout to compare changes in phytoplankton and zooplankton biomass, as well as variables used to interpret relative mechanisms such as growth rate and grazing, in long-term time series or seasonality. This might be inaccurate when using only themulti-model mean. Assuming that the amplification rate is 200% (i.e., phytoplankton increase 10% and zooplankton increase 20%) for the multi-model mean, one or two models may behave with extremely strong trophic amplification, e.g. 500%, which counteracts the amplification rate of other models. It is practical to discover how each model behave I suggest comparing trophic amplification across the twelve models, with the figures or tables presented in the appendix. If most models (at least 10) show strong, clear trophic amplification, that will give confidence to support this. Otherwise, if only about half of the models show this phenomenon, it means trophic amplification appears only in some models through specific processes in the simulation. You then might need to analyse the processes and mechanisms among models.

Thank you for raising this important point. We have examined the ratio of relative zooplankton change to relative phytoplankton change by the end of the century for each of the 12 models individually (Fig. R2,1). In the STZ, 10 out of 12 models show trophic amplification, indicating that the multimodel mean result is robust and not driven by a few outliers. In the SPZ, the intermodel spread is larger and the median amplification ratio is close to 1, consistent with the weaker and more uncertain signal reported in the manuscript. In the SIZ, 7 out of 12 models show clear trophic amplification. Among the remaining five, three show no substantial change in either phytoplankton or zooplankton biomass, while two show changes of similar magnitude in both groups. The weaker intermodel agreement in the SIZ likely reflects the greater uncertainty in sea ice projections (Roach et al., 2020) and the structural limitations in zooplankton representation discussed above. We will add below figure in the appendix showing the model-by-model amplification ratios for each zone to support these findings.

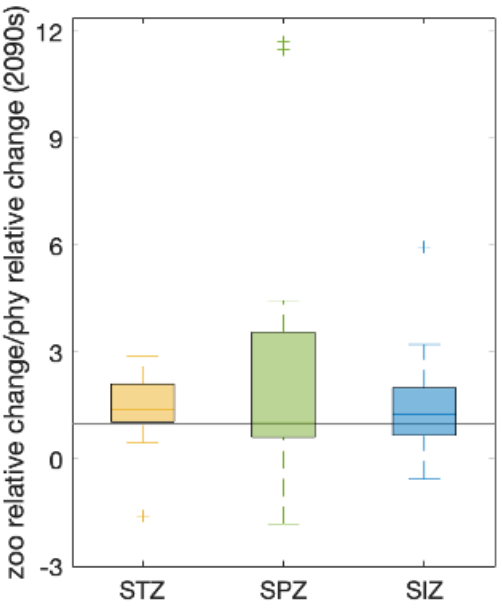


Figure R2.1. Ratio of relative zooplankton biomass change to relative phytoplankton biomass change by the end of the century (2090s) for each zone. Lines and boxes indicate the median and the 25th and 75th percentiles, respectively. Whiskers extend to the most extreme data points not considered outliers, and outliers are plotted as '+'. The grey horizontal line at 1 indicates equal relative change in zooplankton and phytoplankton; values above 1 indicate trophic amplification.

In the phytoplankton and zooplankton tendencies, you describe the phytoplankton grazing loss as being the same as the zooplankton grazing gain. However, this is wrong and highly biased. The “graz” variable in CMIP6 output represents the grazing flux and therefore describes phytoplankton grazing loss. However, zooplankton grazing gain is often the assimilation part of the total grazing flux, where the assimilation rate ε is a parameter in biogeochemical models. Equation 6 should therefore change to Tzoo = dzoo/dt = εGRAZ − Please review the model description document for all 12 models, identify their relative descriptions and equations, and update the zooplankton gain/loss processes and fluxes. This will help improve the accuracy of the zooplankton gain/loss processes and fluxes.

Thank you for this correction. Only a fraction of the grazed prey is assimilated by zooplankton; the remainder is egested as fecal material. Following the reviewer's suggestion, we reviewed the model descriptions and source code of all models used in the budget analysis. The zooplankton assimilation efficiency is 0.8 in CanOE (Christian et al., 2022) and HAMOCC6 (Ilyina et al., 2013), 0.75 in MOPS (Kriest and Oschlies, 2015; Chien et al., 2022), 0.7 in PISCES-v2 (Aumont et al., 2015) and COBALTv2 (Stock et al., 2014, 2020), and 0.51 for carbon in MEDUSA-2 once messy feeding is accounted for (Yool et al., 2013). In OPEM, assimilation efficiency varies with ingestion (Pahlow et al., 2020). We have revised Eq. 6 to Tzoo = ε·GRAZ − Ozoo and applied each model's ε before computing the multimodel mean (new Table Sx). For OPEM we use the simulated actual assimilation efficiency. Ozoo now comprises respiration, excretion, mortality and predation, and is still diagnosed as the residual, so the budget remains closed. Because ε is constant in all models except OPEM, the relative seasonal cycle of zooplankton grazing gain is essentially unchanged. The revision mainly reduces the absolute magnitude of both gain and loss terms. We have updated Figs. 5 and 6 and the related text accordingly.

Some factors, such as nutrient limitation and light exposure, are mentioned to explain changes in phytoplankton biomass (e.g., L197, L264, and L280). However, no evidence (e.g. figures or tables) is provided in this manuscript to prove their relationship. You should download nutrient outputs (e.g., nitrate, silicon for diatoms, and dissolved iron) or limitation terms, as well as the light field, from ESGF. The data include surface and in-depth fields, which can be compared with surface plankton biomass and integrated biomass, respectively. This will help interpret the contribution of environmental variables to changes in plankton biomass under future climate change.

Thank you for this suggestion. We looked into the CMIP6 limitation diagnostics but found two issues that limit their usefulness here. First, only three models in our ensemble (CESM2-WACCM, IPSL-CM6A-HR and UKESM1) output the full set of limitation terms across all experiments used in our study. Second, the light limitation term (limirr) carries a hidden temperature dependence in most models: its computation involves the temperature-scaled maximum growth rate in the denominator (e.g., in MARBL and MEDUSA-2), so warming reduces limirr even when light conditions improve. We will therefore take a complementary approach, adding an analysis of the direct environmental fields that are consistently available across all 12 models: sea surface temperature (tos), mixed layer depth (mlotst), surface shortwave radiation (rsntds), surface nitrate (no3os), dissolved iron (dfe) and sea ice concentration (siconc). We will also revise the relevant text to align our findings.

Minor comments:

Figure 1: It would be good to have a colour bar with the three different shadings. It is hard to distinguish chlorophyll concentration among zones. Label the three zones with three colours in panel (b), for example, beside the panel.

Will be adjusted as suggested

L61: There is an appropriate reference to be cited here: Cheng, M., Maher, N., and Ellwood, M. J.: Evaluating the performance of CMIP6 models in simulating Southern Ocean biogeochemistry, Biogeosciences, 22, 7269–7291, https://doi.org/10.5194/bg-22-7269-2025, 2025.

Thank you for the reference and we will add it as suggested.

Figure 3: It should be checked and stated here whether the error bars and boxes in panels (a, c, e) denote the errors calculated by the mean for each model or all data points for each model.

We will update the figure caption to clarify that the boxplots in all left panels (a, c, e) are calculated based on the regional mean values of the contemporary state (2000s) across the 12 models.

“Boxplots in panels (a, c, e) are based on regional mean values for the first decade of the 21st century (2000–2009) across the 12 models.”

L149-150: It's not appropriate to say “cannot fully close the budgets”as you have already introduced Ophy and Ozoo. Change "fully close" to "explicitly resolve all terms in".

It will be revised as suggested.

L184-185: See comments on Fig. 3. If the error bars and boxes in Fig. 3a, c & e are calculated among all data points for all models but not region means for all models, this sentence is not right. If the error bars are calculated from all data points across all models, these panels can better show larger spatial variation rather than MME spreads of plankton biomass.

As clarified in the above comment, the boxplots in panels (a, c, e) are calculated from the regional mean values across the 12 models, not from all grid points pooled together.

L196-197: Increasing temperature greatly enhances primary production and phytoplankton growthrate in STZ in CMIP6 models, counteracting a great part of the nutrient limitation effect by mixed layer shoaling (Cheng et al., 2026).

We will add Cheng et al., 2026 to the sentence.

L198-200: The stability of the grazing rate in half the models that include the temperature term f(T) in the zooplankton grazing function could be explained by the balance of higher temperature and decreasing phytoplankton biomass. However, the other half of the models do not include f(T) in their grazing function. If phytoplankton biomass decreases in these models, the grazing rate

may not remain stable; instead, it may decrease. This expression should be careful that "could be attributed to" is too strong because this statement can only be supported by half of the models. You shouldchange it to "may be partly attributed to" and carefully check the grazing rates of models without f(T) if they are decreasing.

We agree that the statement is too strong given that only a subset of models include a temperature-dependent term f(T) in the zooplankton grazing function (e.g., PISCESv2, COBALTv2, MARBL), while others treat grazing as temperature-independent (e.g., HAMOCC, MEDUSA, WOMBAT, CanOE; see Kearney et al., 2021, Table 5). We have softened the language from "could be attributed to" to "may be partly attributed to."

L200-203: The logic here is unclear. You first said “the decline in phytoplankton biomass is driven by other increased loss processes or cannot be fully understood”and then said “it is primarily driven by nutrient limitation”. You said “this decline differ across season” and then said “it's concentrated in spring and summer”. It is verbose and hard to understand. You should simplify these sentences and state like, "the decline in phytoplankton is primarily driven by nutrient limitation, especially in austral spring and summer, as revealed by the seasonal analysis in Sect. 3.2.1".

We have simplified the line to avoid preempting the findings of the seasonal analysis, while directing the reader to Sect. 3.2.1 where the mechanisms are examined in detail.

“Consequently, the decline in phytoplankton biomass cannot be fully understood from the annual mean budget alone and is further examined in the seasonal analysis in Sect. 3.2.1.”

L216-218: It is mentioned that there is no clear change of total phytoplankton in the SPZ. However, this stability (-1% change) is attributed to improved light conditions and stable iron conditions. No negative effect balances it. This could be caused by mixed-layer shoaling, which shoals the phytoplankton growth depth, leading to increased surface phytoplankton biomass (Fig. B3) and stable integrated biomass (Fig. 3). This should be interpreted here.

While mixed layer shoaling may contribute to the increase in surface phytoplankton concentrations (Fig. B3), we believe the near-stability of integrated phytoplankton biomass in the SPZ is primarily a result of the dynamic balance between bottom-up and top-down processes under projected climate change (Fig. B1). To avoid confusion, we have moved this explanation in the end of the paragraph to the first sentence of the paragraph:

“*Within the SPZ, total phytoplankton biomass remains relatively stable over the century (−1 ± 2%, Fig. 3d, green line) as a result of a dynamic balance between bottom-up and top-down processes under projected climate change.”*

L218-219: Although NPP is projected to increase in SPZ over the century which is proved by a few studies (Cheng et al., 2026; Fisher et al., 2025; Tagliabue et al., 2025), many studies revealed that NPP has been declining over the past few decades, both by satellite and BGC-Argo product (Cheng et al., 2026; Ryan-Keogh et al., 2023; Tagliabue et al., 2025). Much of the literature presents the opposite view. This sentence should be rewritten, and the results need further interpretation. Liniger et al. (2025)report annual net community production increases across most of the Southern Ocean, which differs from the phytoplankton production observed here. You should either remove "which agrees ... 2025)" and compare it to the observational studies (satellite and BGC-Argo) I mentioned above, which provide observed NPP trend analyses.

We will remove the comparison with Liniger et al. (2025), as that study reports net community production rather than net primary production. Regarding observed NPP trends, Tagliabue et al. (2025) highlight that satellite-derived NPP trends in the Southern Ocean remain highly uncertain, with two out of six algorithms (VGPM-based) showing increasing NPP and four (CbPM and absorption-based) showing declining NPP, largely due to differences in chlorophyll retrieval

methods. Given this lack of observational consensus, we have revised the text to acknowledge this uncertainty and the tendency towards decline.

"Overall, phytoplankton production is projected to increase by 6% over the century (Fig. B1a), yet remote sensing and BGC-Argo trends remain largely uncertain, with a tendency towards declining regional NPP (Tagliabue et al., 2025)."

L220-221: The wording here is too strong, e.g., "Leadingto", "and hence". You should either provide evidence to prove this relationship (shoaling mixed layer -> increased surface phytoplankton concentration -> intensified zooplankton grazing), or use precise vocabulary. Increased surface phytoplankton concentration may be mainly driven by higher sea surface temperature in the SPZ (Cheng et al., 2026).

We will soften the wording and acknowledge that warming may also play a role in enhancing zooplankton grazing in models where grazing is temperature-dependent.

"However, the shoaling mixed layer could also increase surface phytoplankton concentration (5%, Fig. B3a), potentially reinforced by warming in models with temperature-dependent grazing, contributing to higher zooplankton grazing (7%, Fig. B1b)."

L229-230: The causality in this sentence is too strong. Strong shoaling in MLD could increase light exposure and improve growth conditions, but it can also reduce nutrient supply and restrain phytoplankton growth. I would suggest deleting ",partially due to ... (Fig. 3b&d)" and using a more precise relationship.

We will add analysis on nutrient, PAR, sea ice change into the picture.

L231-233: "the shoaling mixed layer along with sea ice retreat" should be changed to "enhanced light conditions caused by the shoaling mixed layer along with sea ice retreat". Moreover, increasing temperature also greatly enhances the production here (Cheng et al., 2026).

We will revise the sentence as suggested and acknowledge the contribution of rising temperatures to enhanced production in this region.

"Consequently, enhanced light conditions due to the shoaling mixed layer along with sea ice retreat (more than 50% over the century, Fig. B5), together with rising temperatures (Cheng et al., 2026), increase both phytoplankton production (16%, Fig. B1a) and biomass, despite a concomitant increase in zooplankton grazing (30%, Fig. B1b)."

L254-255: "during austral spring" should be changed to "during late austral spring".

Corrected.

L259-261: Assuming the reduction in primary production is during austral summer here, then grazing trend is negative during this period (Fig. 4b). You should remove the statement of "a lower phytoplankton biomass as a result of increasing grazing mortality". Instead, this lower phytoplankton biomass may mainly result from deteriorating growth conditions. You should rephrase this sentence.

We will revise the sentence to distinguish between the austral summer core (December to April), where both grazing and growth rate trends are negative (Fig. 4b) and the decline in biomass is primarily driven by deteriorating growth conditions, and the shoulder months (November, May, June), where both growth rate and grazing mortality increase and the lower phytoplankton biomass is mostly attributable to enhanced grazing pressure.

"The reduction in primary production is mainly driven by deteriorating growth conditions from December to April, while in the shoulder months (November, May, June) it results from lower phytoplankton biomass due to increasing grazing mortality (g, Fig. 4b)."

L263-264: Fig. 4b cannot support this sentence that mixed layer shoaling can result in nutrient limitation. You should make some figures to support the nutrient limitation, see the third major comment.

We have downloaded the full set of light-, N- and Fe-limitation terms from CESM2-WACCM, IPSL-CM6A-HR and UKESM under the experiment simulation that has been used in our study (PiControl, historical, SSP585). We will add the analysis of the nutrient limitation variables here.

L264-266: Δg/g2000s - Δu/u2000s > 0 (enhanced grazing mortality outweighs the enhanced phytoplankton growth rate) does not exist during the rest of the year, but only in a few months. You should rephrase it.

We agree that this pattern does not persist throughout the rest of the year. We have revised the text to clarify that enhanced grazing mortality contributes to the continued decline in phytoplankton biomass during certain months, rather than implying that it consistently outweighs enhanced growth rate for the remainder of the year.

Figure 4: (a) As I can see, there are two dotted lines in panels a, c, d and two lines overlap in panels a and c. However, you only describe one horizontal line in the caption. Please check whether the description is missing or if there is a plotting error. (b) Moreover, the horizontal lines in the panels seem to denote the relative change at the end of the century rather than the annual mean. Please check both the plots and the caption and ensure they match. (c) Some month abbreviations in panel b are duplicated, such as M, J, and A. You should use a more unique way to denote each month. (d) You should replot panel b to make it clearer to compare Δg/g2000s and Δu/u2000s: use the same scale of x and y axis; plot an auxiliary line (y=x). (e) These comments are also appropriate for Figs. 5 and 6.

(a) Sorry for the missing information. We will update the caption for both horizontal lines: the thick dotted lines indicate the projected end-of-century annual mean change from models with grazing flux output (dagger sign in Table 1), while the light dotted lines indicate the projected end-of-century annual mean change from all models in Table 1, corresponding to the values shown in Figure 3; (b). The horizontal lines show the projected end-of-century change calculated from annual mean values; (c) We will replace the letters to numbers for months to avoid confusion; (d) we will add a 1:1 line to panel (b); (e) These changes will be applied consistently to Figures 5 and 6.

L285-286: The cited figure should be changed from fig. 5e to fig. 5c.

Corrected.

L305-306: There is no evidence provided here to support the idea that MME reproduces the seasonality well. You should either provide relevant analysis or rephrase/remove the relevant description.

We will add the reference to Figure 2f, i, which show the MME seasonal cycle of phyto- and zooplankton together with the observations.

L307-308: Similarly, "strong light-driven seasonal cycle" is unsupported. You cannot state the strength of the light-driven seasonal cycle among zones without evidence. Please provide relevant analysis and rephrase the description accordingly.

We will add an analysis on surface radiation and light limitation from models to support this.

L351: You should provide a detailed definition of ZPC, which you use throughout the discussion, to help readers better understand what you are discussing.

We will add a brief definition of ZPC (zooplankton-phytoplankton coupling, GRAZ/PP) at its first mention in the discussion to help readers follow the subsequent discussion.

L353-356: The mechanism interpreted here is hard to follow. It's unclear why phytoplankton concentration, rather than integrated biomass, can control the zooplankton response. Moreover, physical forcing is raised here to drive ZPC, which is also unclear. The authors should interpret these mechanisms in more detail, rather than making such a leap.

The key point is that MLD shoaling concentrates phytoplankton into a shallower surface layer, increasing encounter rates between zooplankton and their prey even when column-integrated phytoplankton biomass changes only modestly. This concentration effect enhances grazing pressure disproportionately relative to changes in total primary production, driving the trophic amplification. We will revise the text to make this mechanism more clear.

"The disproportionate zooplankton response in both zones is ultimately linked to changes in phytoplankton surface concentration. For example, as mixed layers shoal, phytoplankton biomass is compressed in a shallower layer, increasing local concentration even when column-integrated biomass changes are small (Fig. B3a). Because zooplankton grazing depends on local prey concentration, this increased concentration amplifies grazing pressure and strengthens zooplankton-phytoplankton coupling, providing a mechanism through which physical forcing modulates trophic transfer (Behrenfeld, 2010; Xue et al., 2022a, 2024a)."

L359: "If heterotrophic processes are more temperature-sensitive": There is an appropriate reference to be cited here:Aumont, O., Ethé, C., Tagliabue, A., Bopp, L., & Gehlen, M. (2015). PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies. Geoscientific Model Development, 8(8), 2465–2513. https://doi.org/10.5194/gmd-8-2465-2015. PISCES-v2 uses different temperature-sensitivity parameters for phytoplankton and zooplankton, with f(T) of 1.066^T and 1.079^T, respectively.

We have looked into the temperature dependence terms for heterotrophic processes across our model ensemble using the Q10 values reported in Kearney et al. (2021, Table 5). Most models in our ensemble either treat zooplankton grazing as temperature-independent or apply the same Q10 to all biological rates. We have revised the text accordingly and added the reference to Kearney et al. (2021), which provides a comprehensive summary of temperature dependency across the multimodel ensemble.

"*Yet, most models in our ensemble treat zooplankton grazing as temperature-independent or apply the same Q10 to all biological rates, with only PISCESv2 (Aumont et al. 2015) applying a stronger temperature sensitivity for grazing than for phytoplankton growth (Kearney et al., 2021).*"

L370: Change "equalling" to "often exceeding". Zooplankton biomass is higher than phytoplankton in the Southern Ocean summer (Yang et al., 2022).

Yang et al. (2022) reported zooplankton biomass often exceeding phytoplankton biomass in the Southern Ocean during summer. However, their phytoplankton estimates were derived from satellite chlorophyll (ESA OC-CCI) converted to carbon using a fixed C:Chl ratio, which may underestimate phytoplankton biomass compared to the BGC-Argo-based estimates used in our observational comparison.

L376-377: Statistical analysis should be done to prove "relative changes in phytoplankton biomass are not influenced by the structural complexity of ecosystem models" (i.e. $p>0.05$), see examples in Fig. 15 of Cheng et al. (2025).

We agree that a statistical test would strengthen this statement. We will add a correlation analysis between model structural complexity (e.g., the number of plankton functional types) and the projected relative changes in plankton biomass across the three zones, and report the statistical significance. We will revise the text to reflect the outcome of this test, following the approach in Cheng et al. (2025, Fig. 15).

L377-379: This expression makes it confusing. Assuming trophic amplification patterns are not influenced by model structural complexity, can they be simulated by simple processes and

underestimated by specific parameterisation, or do they require more complex models to constrain them well? The expression of "robust to differences in model complexity" is inaccurate and exaggerated. You should reconsider this and rephrase it with a more qualified statement.

The intended meaning is that trophic amplification emerges across models regardless of their structural complexity, not that model complexity has no influence on the results. We have rephrased to convey this more carefully.

"This means that trophic amplification emerges consistently across models with varying structural complexity, although the absolute magnitudes remain uncertain and may be sensitive to specific parameterisations."

Reference

Kearney, K. A., Bograd, S. J., Drenkard, E., Gomez, F. A., Haltuch, M., Hermann, A. J., ... & Woodworth-Jefcoats, P. A. (2021). Using global-scale Earth system models for regional fisheries applications. Frontiers in Marine Science, 8, 622206.