Reply to reviewer 2:

This manuscript is a nice exploration of how the plankton ecosystem in the Southern Ocean will change under a high-emissions climate change scenario. Xue et al., evaluate a set of CMIP6 models for phytoplankton, zooplankton, and mixed layer characteristics, arguing that the shift from bottom-up to top-down control under climate change results in roughly unchanged phytoplankton biomass in the Southern Ocean. Additionally, they find an emergent constraint between models' representation of the seasonal sensitivity of surface chlorophyll with the shoaling of the mixed layer to a long-term sensitivity of chlorophyll and mixed layer depth. With this emergent constraint they are able to reduce the uncertainty in model projections of chlorophyll change under climate change.

This is overall a nice, well-written manuscript. I found the emergent constraint portion of the manuscript to be the most interesting, however, it seemed at times disconnected from the rest of the manuscript (even though the shoaling of the ML leading to phytoplankton blooms is a key bottom-up mechanism driving phytoplankton biomass) – as if it were somehow tacked on to the manuscript at the end of the writing process rather than fully integrated from the beginning. This is evident from the fact that no mention of the emergent constraint is in the abstract or introduction, even though it makes for quite a key result.

While the EC result is quite interesting, I also found it to be insufficiently explored both in its own sake and also within the context of "shifting balance of bottom-up and top-down control." A few points for the authors to consider for their revision:

We sincerely thank you for dedicating your time to reviewing our manuscript. Your feedback is highly valued, and we are genuinely appreciative of both your positive comments and the insightful points you have raised. It's encouraging to learn that you found the manuscript to be well-written and particularly found the emergent constraint aspect of the results intriguing. We consider your four main points in detail below:

It's nice that we are able to now use this new model analysis framework of the emergent constraint to reduce uncertainty in future projections, but what does this analysis REALLY tell you about bottom-up vs. top-down control in the Southern Ocean, in terms of drivers and mechanisms? In the conclusions, you state "we further employ the approach of the emergent constraint to increase our confidence in the increasing trend of phytoplankton concentration … which is the underlying mechanism that contributes to the intensified top-down processes under climate change." (lines 297-298) but first, phytoplankton concentration (biomass) does not really increase under climate change, and second, chlorophyll as a proxy appears to be a mix of biomass and productivity. All that the EC analysis appears to really be used for is to reduce uncertainty in future projections of chlorophyll (not phytoplankton biomass). The reader is left wondering what the actual connection really is between the EC and the mechanisms of top-down vs. bottom-up control.

R: Thank you for pointing out the disconnect between "top-down control - phytoplankton biomass concentration" and "emergent constraint - chlorophyll concentration".

Chlorophyll and phytoplankton biomass concentration correlate very well (Fig. A3). We have a figure in the appendix that supports the correlation and that we will reference in the main text not only in the methods section (as previously done) but also in the results, section 3.3.1, to more clearly link chlorophyll and biomass. Further, we will include the new Fig. R2.1 in the results section to explicitly show the change in phytoplankton biomass concentration in the 21st century. This addition will help bridge the conceptual gap and provide a more cohesive presentation of our findings.

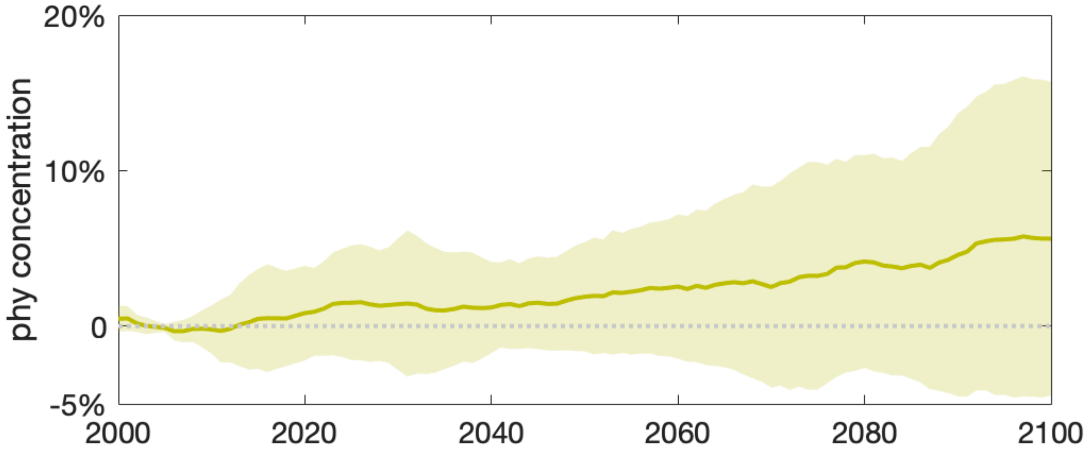


Fig. R2.1 An increase in surface phytoplankton biomass concentration during the 21st century. Multi-model ensemble (MME) projections of the relative changes of surface phytoplankton biomass concentration from 2000-2100 under the SSP5-8.5 scenario, with shading indicating one standard deviation, relative to the respective mean values of the first decade of the 21st century (2000-2009) in the Southern Ocean. The time series are filtered using a 10-year moving average.

I understand that the methodology of the EC utilizes a linear relationship between the models within the multi-model ensemble to constrain future projects. However, is there an extent to which the models (in the historical period) are sufficiently far away from the observations that they can be excluded? There are 2-3 models with a negative relationship between shoaling of the ML and chlorophyll concentration (S_seas > 0 or S_clim > 0) – what is causing this different relationship in those models? Should they be excluded, or the results interrogated further in some way?

R: Emergent constraints are fundamentally different from model selection or weighting approaches. The models are only used to determine a relationship between a predictor variable and a predicted variable. A model that has a predictor variable far away from observations is still assumed to be equally capable of representing the relationship between that predictor variable and the predicted variable. As such, no model should be excluded and outliers in the predictor variable are even more valuable to robustly detect the underlying relationship. Only after the model-based relationship is established based on all models, this relationship is exploited with observations.

The causes of the different relationships between the seasonal variations of mixed layer and chlorophyll in individual models could potentially arise from various aspects, such as model physics, simulation initial conditions, parameters of model equations. However, the biases in the predictor variable similarly influence the predicted variable, increasing confidence that the identified relationship is indeed robust.

In the revised manuscript, we have added the following sentences to L203 to avoid any misunderstandings:

"Though the sensitivities of chlorophyll to changes in MLD on a seasonal scale from individual models show some spread, it is important to note that the models deviating from the observed sensitivity are still considered to be capable of representing the relationship between chlorophyll sensitivity to MLD changes across seasonal and long-term scales."

Along these veins, I noticed that many of the models clustered around the S_seas=0 line (e, i, f, n, h) all have skillful representations of mesozooplankton (Petrik et al. 2022) – and likely have put work into their modelled zooplankton such that they are not "treated stemotherly as a mere closure term." The one exception is model (c – CanESM CanOE) which is far away from the S_seas = 0 line but not particularly skillful in its representation of mesozooplankton. Have the authors thought about why this might be and what may be driving this clustering of these particular models (CMCC, UKESM, CNRM, IPSL, GFDL)?

R: Thank you for bringing to our attention the connection to the findings in Petrik et al., 2022. In their study, Petrik et al. evaluated and used six models that simulated mesozooplankton, including CMCC, UKESM, CNRM, IPSL, GFDL, and CanESM-CanOE. Compared to the mesozooplankton observations, all models performed reasonably well, which is important for capturing the top-down process for phytoplankton. In the context of our study/the variables that we assess here (MLD, phyto seasonality), the additional eight models (include only one zooplankton group) that we include do not necessarily perform worse than the six models with multiple zooplankton groups. In fact, one of these models, the closest to observation in S_seas, k (MPI), has only one zooplankton group. To respond to your question, we include below figure R2.2 from another paper that is currently in review, using nearly identical model ensembles. This figure illustrates that biogeochemical model complexity does not systematically affect the projections on plankton change.

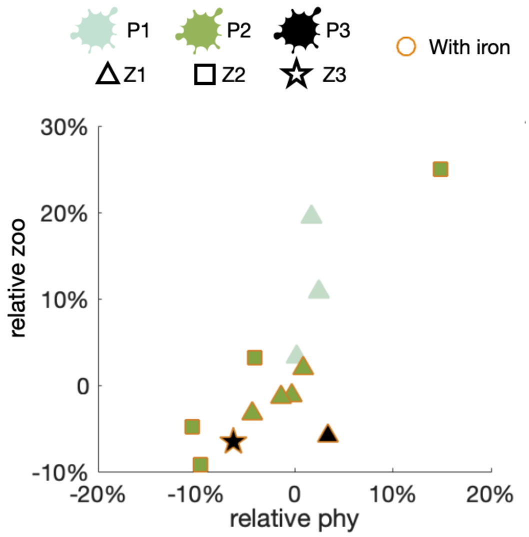


Fig. R2.2 Individual model projections of the relative changes of integrated phytoplankton (phy, horizontal axis) and zooplankton biomass (zoo, vertical axis) over the course of the 21st century, illustrating that there is no systematic effect of biogeochemical model complexity on the projections. The color of the markers indicates the number of phytoplankton groups included in the model, with light green, green, and black representing one, two, and three phytoplankton groups, respectively. The shape of the markers indicates the number of zooplankton groups included in the model, with triangle, square, and star representing one, two, and three zooplankton groups, respectively. Markers with orange edges indicate that the model explicitly includes iron limitation.

Regarding the observational constraint – it was quite striking to me that the uncertainty around the observed chlorophyll values were so much lower than observed MLD values (Fig 9). When constructing your observed S_seas values (with uncertainties) are you comparing like to like in the MLD and surface chlorophyll fields? E.g., would it be a better comparison if you were to resample the Globcolour chlorophyll field for the 1-degree grids where Argo MLD data are available?

R: Thank you for pointing it out. Indeed, as depicted in Figure 9, the uncertainty (standard deviation) associated with the observed MLD is notably higher compared to that of the observed chlorophyll values. We agree that this disparity is likely attributed in part to the limited availability of ARGO MLD data compared to satellite data, so that space/time are not well sampled. Additionally, another factor that might contribute to the relatively smaller uncertainty in observed chlorophyll values is the normalization process. Consider also that we here show relative changes throughout the seasonality, such normalization can effectively diminish the interannual variability of seasonality, thereby potentially reducing the uncertainty of chlorophyll to a certain extent. To this end, we will add the below sentence to state the potential source of uncertainty of the observed MLD in Line 187:

“Compared to observed chlorophyll with extended data coverage (satellite-based estimates), MLD (based on sparse in situ float data) reveals higher uncertainty, which could be largely due to the scarcity of data.”

Detailed point-by-point responses to minor comments are listed below:

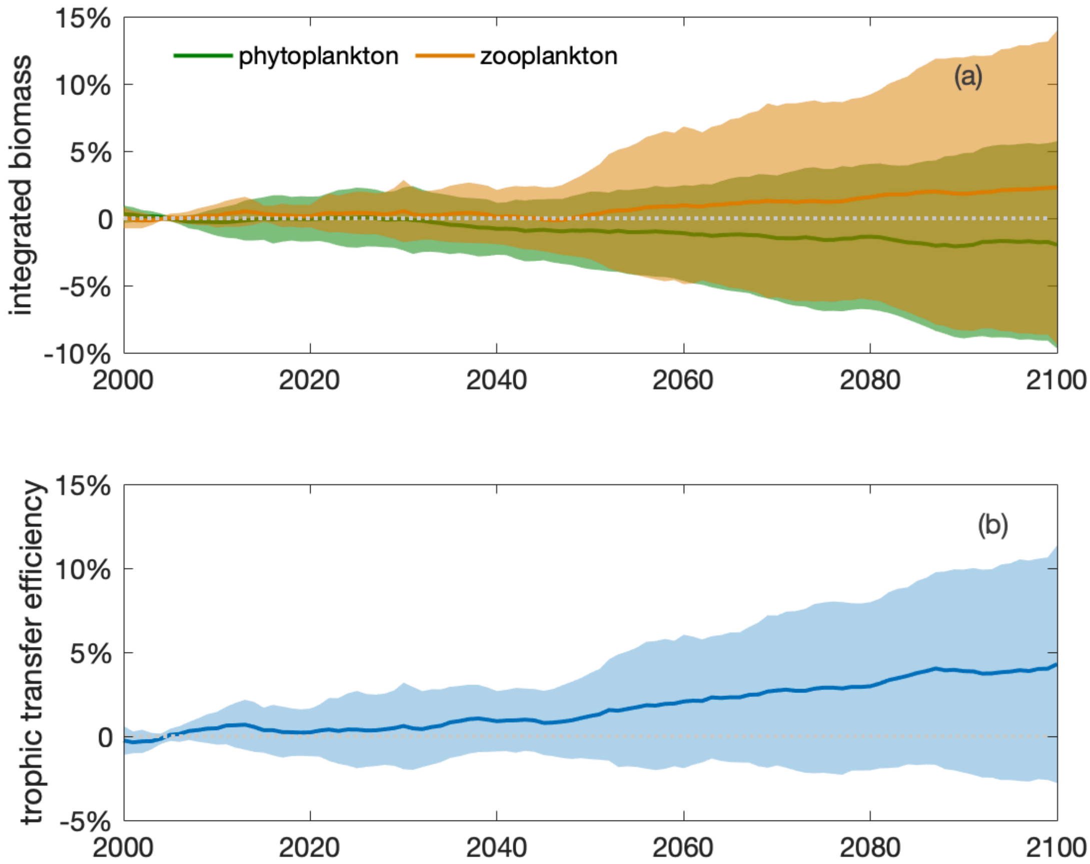
I believe CMCC-ESM2 phytoplankton and zooplankton biomass fields are provided on the CMIP6 ESGF archive. I did a quick search today (Jan 17) and found phydiat, phymisc, zmeso, and zmicro on the archive with monthly outputs. (I did not check for ACCESS-ESM1-5).

R: We appreciate your efforts in conducting a quick search. Indeed, we missed these models as we considered the output variable zooc that combines all zoo groups.

We will update our results and include the plankton output of CMCC in our results. We will exclude ACCESS-ESM1-5 as we find that the output for the historical simulation is missing (even though piControl and ssp585 are available).

Figure 8a – there is no green shading to indicate the variability in phytoplankton biomass, only orange shading for the zooplankton. If you are intending for the reader to compare the orange shading in Fig. 8a with Fig. 5a then please indicate so. (Also, make your y-axis labels consistent between those two plots)

R: thank you so much for pointing it out. We will update Figure 8a and include the green shading.



It really was not clear to me what Fig. 7 was supposed to show. The text where Fig. 7 is referenced was not particularly informative – can you please expand on it (particularly for readers not familiar with Xue et al. 2022a), and if it's not essential to the main text, then perhaps remove it or place it in the supplemental?

R: We acknowledge that Figure 7 is not clear to the readers. To further clarify the purpose of Figure 7, we will include a topic sentence (in bold) for Figure 7:

"Mixed layer depth directly influences zooplankton grazing."

Additionally, we will modified L161 to help clarify the mechanism:
"A shoaling mixed layer enhances phytoplankton concentration and supports higher prey-predator encounters for zooplankton, which in turn results in a greater grazing pressure and thus a stronger top-down control on phytoplankton."

Again, there's no mention of the emergent constraint in the abstract or introduction. It would be great to introduce the concept of emergent constraint earlier than in the methods.

R: Thank you for pointing this out. Indeed, we did not give emergent constraint enough credit within the abstract but will do so in the updated manuscript, Line 11:

“To increase our confidence in these projections, we employ an emergent constraint approach using the observed relationship between seasonal variations in mixed layer depth and surface chlorophyll concentration, as a proxy for surface phytoplankton concentration. This emergent constraint further supports the intensified top-down control under climate change, driven by rising phytoplankton concentrations due to shoaling mixed layers. ”

Also, the first time that chlorophyll is mentioned as a proxy for phytoplankton biomass is in section 2.4 (methods). I think that if there is space, it should be mentioned in the introduction – but also with the caveat that given variations in chl:c ratios due to photoacclimation and phytoplankton type, it is quite an imperfect proxy for phytoplankton biomass. (Though I personally think that chlorophyll is instead a proxy for a combination of phytoplankton biomass and productivity.)

R: We acknowledge the concern regarding the varying chl:c ratio. We agree that chlorophyll is not a perfect proxy for phytoplankton biomass. However, it remains the most practical and effective option available for our study. Fig. A3 of the original shows linear relationships from different models, which indicate that chl and phytoplankton biomass are well correlated in model simulation and further support using chlorophyll as a proxy for phytoplankton biomass. In addition, we will include the new Fig.R2.1 (see response above) next to Fig. 6b (the latter shows rising chlorophyll under global warming), to explicitly show that not only chlorophyll but also phytoplankton biomass concentration is rising."

I hope that these comments are helpful and not burdensome to address. This is indeed a nicely written paper and interesting study.

Thank you again for your time and effort in reviewing our paper.