Reply to reviewer 1:

I found this manuscript interesting and generally well written. The methods are quite clear and the figures are of good quality. I appreciate their emergent constraint approach. I think this paper should be published eventually because the approach is novel and their hypothesis about phytoplankton change in the Southern Ocean is intriguing. However, there are three main points that I would like to see addressed before publication.

Thank you for taking the time to review our manuscript. We appreciate your positive feedback and the valuable points you have raised. It is encouraging to hear that you found the manuscript interesting, well-written, and the methodology clear. We will address below the three major points you raised.

** I think the authors need to tone down their definitive language; as it is written now it comes off as hubris. The authors should use more speculative or conditional language to convey the points they are making. Attributing such specific mechanisms of change in phytoplankton using such a diverse set of models is problematic because the models could potentially have different processes that are controlling productivity/biomass in the Southern Ocean. Multi-model ensembles are useful in that they average out biases in individual models, but such specific attribution of mechanisms can really only be done with certainty by looking at the equations of individual models. Otherwise, it is just speculation about what's going to happen. The story that the authors describe is compelling but it does not necessarily mean that this is what's happening in every model. For example, the subantarctic region of the SO could become cloudier with climate change (see Fig 1f in Leung et al 2005), leading to an increase in Chl/C ratios of phytoplankton that could lead to increasing surface chlorophyll trend shown in Fig 6. I'm not suggesting that the authors are incorrect with their hypothesis, but they need to be more modest about how they attribute the drivers of change.*

* Thank you for pointing this out. We have modified the manuscript to use more speculative or conditional language to better align our statements with the inherent uncertainties associated with modelling a complex system. As an example, consider the modification made in Line 150. We revised the sentence to read, "The increasing top-down control and grazing pressure on phytoplankton may be a consequence of ...," opting for this phrasing instead of stating "is a consequence of" to tone down the certainty.

In regard to Fig 6, the effect of a change of cloud coverage on Chl:C and thereby chlorophyll is an interesting point that we will integrate into our manuscript and change Line 115 to: "Surface chlorophyll is used as a reliable proxy for surface phytoplankton concentration due to the strong correlation between modelled chlorophyll and phytoplankton biomass concentration (Fig. A3). This correlation holds despite the potential impact of changes in cloud coverage on the Chl:C ratio (Leung et al., 2015)."
Meanwhile, we will add Figure R1.2 below to Figure 6 to explicitly show the increase in phytoplankton biomass concentration under warming.

** Relating to the first point - the authors are very dismissive about the impacts of changing iron availability for phytoplankton (despite the well documented importance of iron in controlling production in the Southern Ocean; e.g., see section 3.5 and refs therein of Petrou et al., 2016). They also do not address the potentially big impact of increasing temperature.*

As phytoplankton growth rates, zooplankton grazing rates, and phytoplankton/zooplankton loss rates are highly sensitive to temperature in most models, I think this deserves some discussion and perhaps additional analysis. How do temperature and iron conditions change in the Southern Ocean upper mixed layer in this MME? How do these changes project onto the hypothesis that the authors present?

* We agree that iron and temperature are two important factors influencing the phytoplankton in the research area. While we did incorporate a discussion on the impact of iron availability on phytoplankton growth and the effect of temperature on zooplankton grazing rates, we understand that our mention of these critical factors may not have been sufficiently comprehensive given their importance. In response to your suggestions, we have thoroughly revised our manuscript, enriching the existing content with a deeper exploration of the roles of temperature and iron.

To address the multifaceted impact of temperature, we have incorporated its effects into Sections 3.1 and 3.2. These sections now cover the influence of temperature on phytoplankton growth and its potential to affect zooplankton grazing. For instance, we have included the following sentence in Line 160: "Higher temperatures are linked to increased rates of zooplankton grazing, leading to more pronounced top-down control of phytoplankton populations (Caron and Hutchins, 2013), though only a few CMIP6 models include temperature-dependent grazing (Rohr et al, 2023) " We have added Fig.R1.1 bottom panel to the manuscript appendix as Figure A5, to illustrate Southern Ocean surface warming by the end of the century as simulated by the Multi-Model Ensemble (MME).

Regarding the role of iron, we have broadened our discussion to highlight the complexity of the iron cycle and the caveat of the model simulation in this aspect:
“Given the complex interplay between iron and its biological responses, along with the multitude of external processes affecting its availability, projecting changes in iron availability and subsequent phytoplankton responses under future climate conditions is challenging (Petrou et al., 2016). Current model projections do not show a significant change in iron availability (Fig.A6). As a result, models with or without iron limitation do not show distinct differences in their projections of phytoplankton responses to climate change (Fig.A7), though models with iron representation show a much larger spread than those without iron. ”
We have added the Fig.R1.1 top panel to the appendix—Figures A6—depicting the projected changes in dissolved iron as simulated by the MME.

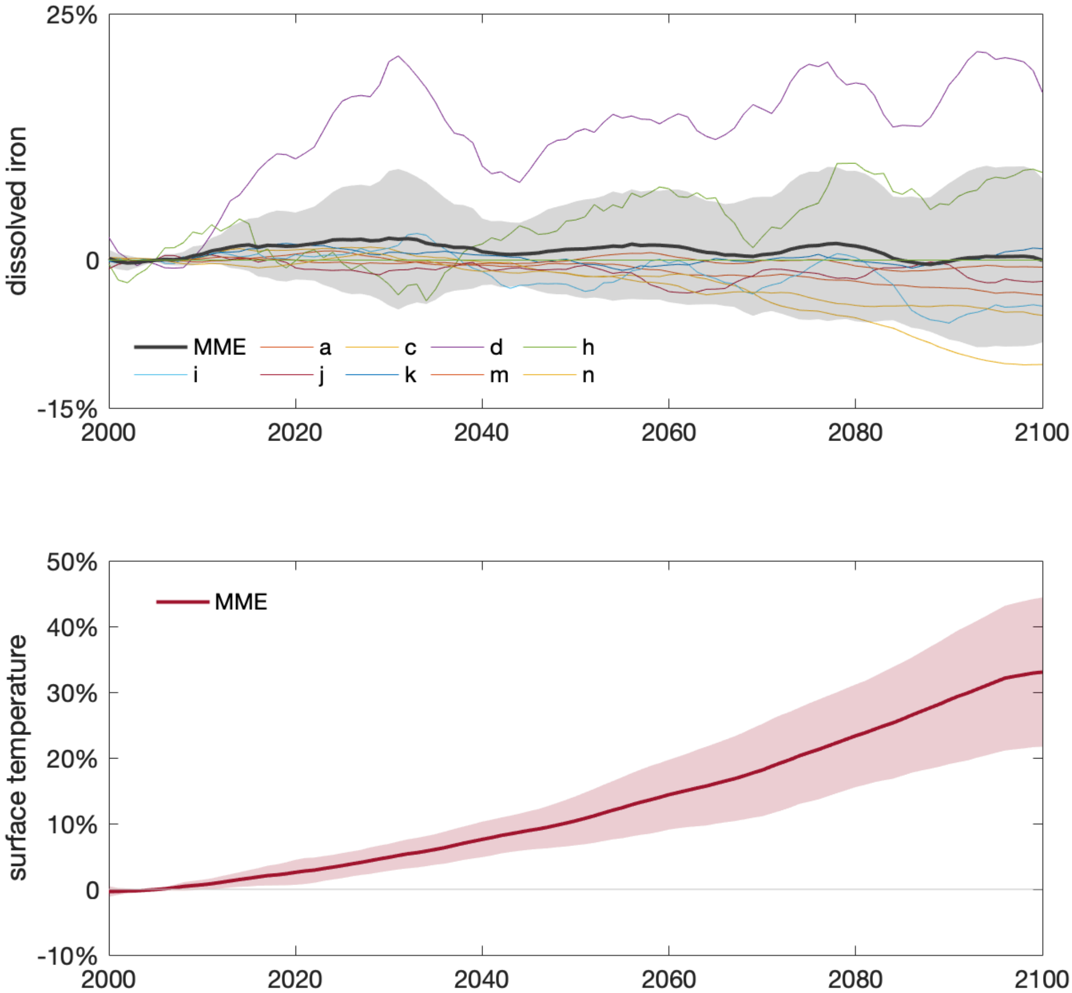

Fig. R1.1 **Varying future dissolved iron trend and clearly increasing sea surface temperature under climate change**. Multi-model ensemble (MME) projections of the relative changes of dissolved iron (top) and sea surface temperature (bottom) from 2000-2100 under the SSP5-8.5 scenario, with shading indicating one standard deviation, relative to the respective mean values of the first decade of the 21 century (2000-2009) in the Southern Ocean. Dissolved iron data is only available from a limited set of models. The letter in the legend indicates the respective model listed in Table 1. The time series are filtered using a 10-year moving average.

** Their argument appears to be somewhat circular – The authors say that a shallower MLD leads to more concentrated phytoplankton at the surface (this is not shown) and that leads to more grazing efficiency which reduces the phytoplankton concentration (which would, in turn, reduce the grazing efficiency). So, I suggest they add more plots to show that phytoplankton biomass really is more concentrated nearer to the surface. The integrated plots that are shown in Figure 5 for example should be broken down by depth to support the hypothesis they are making. They show surface chlorophyll trends in Figure 6, but with most models having variable Chl/C ratios, this is not definitely showing what they claim. The* *authors repeatedly say that phytoplankton concentrations in a shallower MLD increase so this needs to be demonstrated.*

* Thank you for pointing out that we missed showing the increasing surface phytoplankton concentration in the 21st century. We did include Fig A3 in the original manuscript, showing chlorophyll and phytoplankton biomass concentration correlate very well in the model simulations. We will stress it more clearly in the method section, as mentioned above. In response to your suggestion, we will include an additional figure that explicitly illustrates the increase in phytoplankton biomass concentration over the 21st century (Fig.R1.2), supporting that it is not merely chlorophyll that increases but that phytoplankton biomass concentration does so, too.

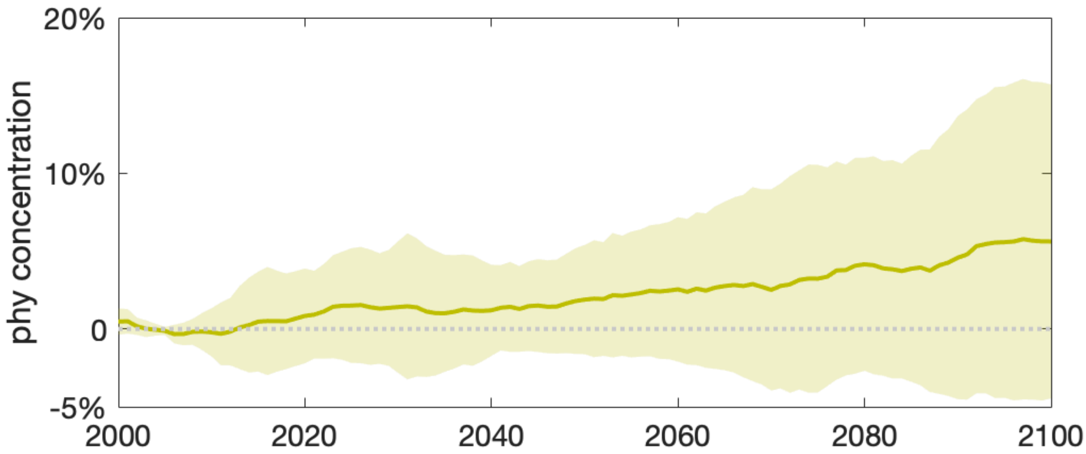

Fig. R1.2 An increase in surface phytoplankton biomass concentration during the 21st century. Multi-model ensemble (MME) projections of the relative changes of surface phytoplankton biomass concentration from 2000-2100 under the SSP5-8.5 scenario, with shading indicating one standard deviation, relative 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.

Detailed point-by-point responses to your detailed comments are listed below:

L22: Rather than "poorly simulated", perhaps say "simplistic"
R: Adapted.

L32/33: This sentence is awkward in that the words "climate change" are used twice. Reword to something like this: " These factors are all projected to change with climate change so phytoplankton will likely be impacted from changing bottom up processes".
R: Thanks for pointing it out. We will rephrase the sentence as follows:
"Given that these environmental factors are projected to change under climate change, it is expected that phytoplankton will also be affected."

L40:light conditions in high latitude regions may also improve due to decreasing sea ice cover.
R: We will add the impact of light conditions due to sea ice cover change and rephrase the sentence to:

"An opposite bottom-up response can be found in high-latitude regions where improved light conditions due to increased stratification and sea ice retreat are projected to lead to phytoplankton increases (Sarmiento et al., 2004b; Deppeler and Davidson, 2017)."

L52: perhaps remove the word "current" since Tagliabue et al (2016) is about CMIP5 models.
R: Thanks for pointing this out. Done.

Figure 2 caption and throughout the Figures/captions: rather than "relative variation" could you say "relative change" or "normalized changes"… Variation implies you're looking at the variability and I find it confusing.
R: Thanks for pointing this out. The instances of "variation" have been replaced with "relative changes".

Figure 3 caption. Boxes denoted by dashed lines mark the focus area (reword that sentence of caption)
R: The corresponding sentence will be replaced by the suggested sentence.

Figure 3: are these maps means of Figures 1 and 2? If so, could you explicitly state that in the caption?
R: Yes, indeed. We will add "Maps (a) and (b) are the means of Figures 1 and 2, respectively." for further clarification.

L97/99: I don't think you should call out figures before you have introduced them in the Results section.
R: We will remove the early reference to figure 9 and figure 10b.

L150-160: this is where the logic sounds very circular – you need to show that phytoplankton concentrations actually increases in the mixed layer, or reword.
R: Thank you for pointing out that the logic sounds circular. We will add figure R.1.2 to explicitly show that the surface phytoplankton biomass concentration projected by the MME increases by 5 ± 10% from the 2000s to the 2090s.

L158/159: Figure 7 is not very convincing towards this point. Much of the negative relationship results from one model (CanESM).. or am I missing something?
R: In this context, our emphasis is on the increase in zooplankton grazing as the mixed-layer depth (MLD) shoals. To support this point, the relative variation of zooplankton grazing (y-axis in Fig. 7) should exhibit an inverse relationship with the relative variation of MLD (x-axis in Fig. 7). Indeed, CanESM-CanOE demonstrates the highest sensitivity, characterized by the steepest slope. However, most other models, while displaying lower sensitivity, also clearly indicate an inverse correlation between MLD and zooplankton grazing change, namely CNRM, FOCI, GFDL and UKESM.

L187: end this sentence after Le Quéré reference. Then start a new sentence with "It is thought to be mainly caused by…."
R: We will rephrase the sentence as suggested to:
"Such an opposite relationship between surface chlorophyll and MLD on a seasonal scale has been previously shown in observations (Uchida et al., 2019; Arteaga et al.,2020) and

model simulations (Song et al., 2018; Arteaga et al., 2020; Le Quéré et al., 2016). It is thought to be mainly caused by seasonal dilution of phytoplankton and by growth limitation along with zooplankton grazing."

Figure 8. I think you should be careful about using TTE in this context, as TTE depends on community composition of the plankton. For example if much of the zooplankton are microzooplankton, the energy stays more in the microbial loop, whereas if they are mesozooplankton they can be consumed by higher trophic levels. See, for example, Krumhardt et al., 2022

R: Thank you for bringing Krumhardt et al., 2022 to our attention, which nicely shows that beyond changes in biomass, global warming is projected to influence plankton composition. A shift in plankton composition is expected to have complex repercussions on ecosystem transfer efficiency through intricate food-web interactions. Unfortunately, due to the simplicity of plankton dynamics in some models (with one phytoplankton and/or one zooplankton group) and a lack of model output (which supports our point that zooplankton-related variables should be regularly saved as standard model output), we are unable to conduct a comprehensive analysis to detect changes in plankton composition and their subsequent impact on energy transfer to higher trophic levels. At this stage, we can only define trophic transfer efficiency (TTE) in its most basic form as the ratio of depth-integrated zooplankton biomass to depth-integrated phytoplankton biomass (Eq. 5). We included this caveat in the manuscript by adding the below sentence to line 120:

“Given model simplicity and limitations of model output, the trophic transfer efficiency (TTE, Eq.5) is simply defined as the ratio of depth-integrated biomass of zooplankton to depth-integrated biomass of phytoplankton following Barnes et al., (2010)”

We will further incorporate studies about the potential change in plankton composition and its consequences through trophic interactions into our discussions (Line 228):

“Beyond changes in biomass, it has been suggested that ecosystem transfer efficiency might be affected by phytoplankton composition under global warming (Petrou et al., 2016; Krumhardt et al., 2022), an effect that we expect not to be well resolved by MME foodwebs that are simplified compared to the real ocean.”

L188-190: this is exactly why you can’t definitively attribute mechanisms in the models.

R: We will use more speculative or conditional language in the revised manuscript.

L192-195: Have you verified that this is what is happening in each model? Otherwise please be more speculative in this statement.

R: Thank you for pointing this out. We will tone down our language.

L198: add ‘s’ to ‘exist’.

R: Adapted.

L205: rather than “mixed layer, average light” say “mixed layer with higher average light intensity”.

R: The text will be modified accordingly:

"when the MLD is shoaling and phytoplankton is being contained in a shallower surface ocean mixed layer with higher average light intensity and temperature, as a result, phytoplankton grow better and concentrations are expected to increase."

L215: say "Our results suggest, therefore, that there will be an increase in surface chlorophyll…"
R: We will modify the sentence accordingly:
"Our results suggest, therefore, that there will be an increase in surface chlorophyll in the subantarctic and subpolar Antarctic regions of the Southern Ocean and thus an increase in phytoplankton concentration by the end of the century."

L218: you have not shown an increase phytoplankton concentration, so be careful how you word this statement.
R: We will add figure R.1.2 to provide support for this statement.

L226: add "productivity" after "phytoplankton
R: Adapted.

Figure 10: could you make the colors more distinguishable? The dark purple and black are so close in tone so it took me a while to see the difference in color between the two dashed lines on panel b.
R: Thank you for pointing it out. We will make the colors more distinguishable.

L231: replace "all" with "some"
R: Adapted.

L236: actually models with iron representation show a much larger spread than those without iron, which is something that should be mentioned.
R: We will rephrase the comparison of model with and without iron to:
"Models with or without iron limitation do not reveal clear differences in the projection of phytoplankton response to climate change, though models with iron representation show a much larger spread than those without (Fig.A5)."

L244-247: this sentence is confusing and long. Please reword. Also, mention the potential influence of different types of predator prey relationships, Holling type II and Holling type III, both of which are used in ESMs.
R: Thank you for pointing out the long sentence. We will split it into two:
"Top-down grazing by zooplankton is related not just to the total available prey (phytoplankton biomass) but also to the efficiency with which zooplankton can feed on this prey. This efficiency is influenced by different factors including traits of both prey and predator, prey concentration, and also by the type of predator-prey relationship used in a model, such as the different Holling types."

L255: actually, the COPEPOD dataset has pretty good coverage globally, except for the subantarctic Southern Ocean (see Moriarty and O'Brian 2013). Perhaps mention this.
R: To acknowledge the COPEPOD dataset, we will add the below sentences after L256:
"The availability of the COPEPOD dataset (Coastal and Oceanic Plankton Ecology, Production, and Observation Database, https://www.st.nmfs.noaa.gov/copepod/, Moriarty and O'Brian 2013) has greatly enhanced access to global mesozooplankton data and provides significant support to advance relevant studies. However, the dataset does not

include microzooplankton, whose dynamics can have substantial effects on ecosystem processes, such as trophic transfer efficiency (TTE; Prowe et al. 2022)"

L310/311: I don't think this is true. Many studies have aimed to understand more about the zooplankton component of ESMs. See for example, Heneghan et al (2016 and 2020) & Negrete-Garcia et al (2022).

R: The biogeochemical models we are referencing here are typically employed in climate research, specifically the Earth System Models (ESMs) included in CMIP6, as listed in Table 1. These models tend to use relatively straightforward descriptions of plankton dynamics, in contrast to more sophisticated size-spectrum models as seen in works like Heneghan et al. (2016 and 2020) and Negrete-Garcia et al. (2022), or end-to-end models capable of simulating complex trophic interactions and feedback from higher trophic levels, including fish.

We will replace "in biogeochemical models" with "in biogeochemical models for climate research, such as CMIP models."

Reference:

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Caron, D. A. and Hutchins, D. A.: The effects of changing climate on microzooplankton grazing and community structure: drivers, predictions and knowledge gaps, Journal of Plankton Research, 35, 235–252, 2013.

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Krumhardt, Kristen M., Matthew C. Long, Zephyr T. Sylvester, and Colleen M. Petrik. "Climate drivers of Southern Ocean phytoplankton community composition and potential impacts on higher trophic levels." Frontiers in Marine Science 9 (2022): 916140.

Moriarty, R., and T. D. O'brien. "Distribution of mesozooplankton biomass in the global ocean." Earth System Science Data 5, no. 1 (2013): 45-55.

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Negrete-García, Gabriela, Jessica Y. Luo, Matthew C. Long, Keith Lindsay, Michael Levy, and Andrew D. Barton. "Plankton energy flows using a global size-structured and trait-based model." Progress in Oceanography 209 (2022): 102898.