Review of Fisher et al. (2023)
“Biogeochemistry of climate driven shifts in Southern Ocean primary producers”

General comments:

This paper synthesizes our current knowledge on projections of Southern Ocean productivity and biogeochemistry, complemented by some analyses of existing CMIP6 model projections for the high-emission scenario SSP5-8.5.

While the topic is generally within the scope of Biogeosciences, I have some concerns regarding the overall scientific advancements and the presentation of the manuscript in its current form. Therefore, I recommend major revisions before this article can be considered for publication for the reasons outlined in more detail in the following.

After reading the paper, I was left wondering what the specific aim of this paper is and what its scientific advances are, e.g., relative to two reviews published in the last years on changing Southern Ocean phytoplankton dynamics (Deppler & Davidson, 2017, doi: 10.3389/fmars.2017.00040) and biogeochemistry (Henley et al., 2020, doi: 10.3389/fmars.2020.00581). Unfortunately, as it is, the knowledge gain from this manuscript seems rather small.

Something that sets the manuscript by Fisher et al. apart from the aforementioned reviews (and the reason why this paper ultimately should be published) is the inclusion of analyses of CMIP6 data. This is generally valuable and useful, but unfortunately, in its current form, the analyses are not presented (enough) as a central part of the manuscript. The results from these analyses are mixed up with reviews of existing literature, with knowledge gaps resulting from the literature review not being tied closely enough to the performed analyses and the shortcomings of the assessed models. To that aim, in my view, a revised version of the manuscript would benefit from a more “traditional” manuscript structure, with a clear knowledge gap/goal of the paper identified in the introduction, a method section with details on which models are assessed, and dedicated results and discussion sections.

I appreciate the extensive work that went into the assessment of existing literature (and I am not arguing to leave any of that out). I think the authors should just work on linking the model analyses more clearly to it, which requires diving deeper into the processes represented (or not represented) in the CMIP6 models in the main text of the paper. In the abstract, the authors set out to “identify key processes where greater observational data coverage can help to improve future model performance”. While I agree with the authors that this is a key step forward, it is not entirely clear to me how the presented description and assessment of the CMIP6 models in the current version of the manuscript contribute to that goal. In the revision process, the authors should therefore focus on making it much clearer to the reader what this paper adds to existing literature.

Specific comments:

L. 24: I don’t understand what such an evidence-based sampling approach would be, given that there is substantial disagreement between the models. Unless I have missed it, you don’t mention more about this elsewhere in the manuscript – can you elaborate?

L. 27: A useful reference to add in this section and throughout the paper is Deppeler and Davidson 2017, as these authors have synthesized our understanding of climate change impacts on Southern Ocean phytoplankton in a lot of detail.

L. 28: It is unclear to me why you say that biological carbon uptake is increasingly important (see also a few lines further down where you it plays a minor role in oceanic CO2 uptake). I suggest to simply say “an important process”.

L. 30-32: I think the inserted “with the Southern Ocean […]” breaks the flow of this sentence. Combining this piece with the information in the sentence before and then switching the order of the sentences (first global, then Southern Ocean) might result in a better flow.

L. 46: I suggest deleting “especially”. I don’t disagree per se, but from what you write it is not clear (yet) why this part of carbon cycling should be “especially” vulnerable.

L. 49: Given that this NPP estimate is based on the years 2002-2007, I am wondering here why you’re not reporting a more up-to-date estimate. Also, what definition of the Southern Ocean are you using?

L. 50: I suggest adding Laufkötter et al. (2015) to the discussion, as the authors describe projected changes in primary productivity in CMIP5 models.

L. 53: Please rephrase “sea ice duration” and “increasing ocean surface area”. In addition, maybe rather say “light availability” than “light supply”?

L. 54: I find this statement a little misleading. Given that the increase in upwelling is predominantly found in open ocean waters, the link to sedimentary iron supply is not obvious. From what I know, sedimentary iron supply does not play a huge role in deep, open ocean regions, as the upwelling does not necessarily bring waters from the seafloor to the upper ocean. Can you clarify this?

L. 62: Please check here and throughout the text: Species names are usually given in *italic*.

L. 75: globally?

L. 76: I suggest adding Nissen et al. (2021) here, as the authors describe the impact of changed Southern Ocean nutrient utilization on nutrient fluxes to lower latitudes.

L. 78: What do you mean by “regional nutrient supplies” here? Please clarify.

L. 82/83: Please note that ocean-only (hindcast) models are used in the Global Carbon Budget, not Earth System Models. I suggest adding a reference to the latest IPCC report here, as Earth System Models are used there.

L. 84: Please specify in what exactly the spread of models has increased. In all variables?

L. 88: How do you know if a model projection is *deficient*? Maybe “differences” is more appropriate here?

L. 90: In addition to the factors you list here, I think the representation of phytoplankton loss processes, and zooplankton and particle dynamics also impact differences in model projections of biogeochemistry (as discussed in Henson et al. 2022).

L. 96: I am not sure that Laufkötter et al. (2018) is the most suitable reference here. Please double-check.

L. 99: I am left somewhat confused after this first section. I expected a knowledge gap being highlighted that will be filled in this paper. Is the purpose of your paper to synthesize current knowledge or to provide new insights based on new assessment of CMIP6 output? I think making this very clear early in the paper will help the reader. I think a better management of the reader's expectation will improve the paper.

L. 105: Can you clarify what you mean by "microscale" here?

L .108: Based on the title of the section, I was not expecting the discussion of any biology here. I suggest rethinking the title or reorganize the content of this section.

L. 110: What CMIP6 models are shown here? How many models are you considering in this ensemble? I suggest to not expect the reader to go to the supplement for this information and to instead include this in the main paper.

L. 112: Since the importance of the limitation factors over time is not shown in the Figure (only the annual mean is given), from what do you infer the "before"? This is unclear from what you write and show.

L. 116/117: It is unclear what changes to the forcing affect iron supply. Can you clarify?

L. 123: Please check the Figure reference – are you referring to Fig. 2a?

L. 123-148: In my view, this part does currently not fit very well into the narrative of the paper. Don't get me wrong, it is a nice summary of the physical changes. However, there is currently too few ties to what the paper is about according to its title: the impact of changes in phytoplankton productivity and community structure on Southern Ocean biogeochemistry. I suggest revising this whole section to fit the aim of the paper.

L. 167: I think "grazer community" should be added to this list, as these will significantly impact the particle composition, particle size distributions, and therefore sinking speeds and the depth of remineralization.

L. 191: As you're showing chlorophyll concentrations in Figure 3 and not carbon biomass, I am left wondering if there are changes in the Chl:C ratio. Is the picture the same for phytoplankton carbon biomass (I am sure this is part of the standard output of CMIP models)?

L. 203: I don't understand this sentence. Can you rephrase this?

L. 204: Please be careful with your wording here: changes in chlorophyll do not necessarily translate into changes in growth. If I understand correctly, what you're finding is that where Si* is high in the present-day, the projected increase in chlorophyll is largest. Given that you don't mention projected changes in Si*, it is difficult to make that link for the reader, especially because Si is not the limiting factor in that region. I suggest expanding here to explain this in more detail.

L. 212: This is another good location to refer to Nissen et al. (2021), in which the authors have performed sensitivity experiments of the importance of dissolution/remineralization depth of nutrient resupply to the surface and nutrient distributions across the Southern Ocean.

L. 216: Unclear to the reader why “especially in low sea ice years”.

L. 220: “surface” pH. Add “under the high-emission scenario SSP5-8.5”.

L. 225: Start a new sentence before “for example…”.

L. 227: The wording is unclear to me here for “Petrou et al. (2019)’s modelled OA conditions”.

L. 247: While I agree with you that there is lots of uncertainty surrounding the implementation of CO2 dependencies into marine ecosystem models, I disagree with them that their implementation is not feasible. Acknowledging that there are still gaps in our understanding, there have, in fact, been several attempts to implement these dependencies into both ocean-only and climate models (e.g., Dutkiewicz et al., 2015; Krumhardt et al., 2019; Seifert et al., 2022 and references therein). I suggest rephrasing this part to reflect the state of the art more accurately.

L. 265/Table 1: It is unclear to me why you only report SI* from WOA and not from the models. Do the models agree with this spatial distribution suggested by WOA data? It would then be great to also report Si* from the projections. Further, I suggest providing the historical average for pH and temperature to put the reported changes into perspective. Is the reported standard deviation across models, across time, or across space? Please clarify. Please also consider adding Freeman et al. (2018) to the respective paragraph in the text for completeness, who have looked at the projected changes in Si* in the Southern Ocean in CESM.

L. 274: It might just be me, but in my mind, “POC” is most commonly used in the literature for sinking particulate organic carbon and not for organic matter in phytoplankton. I would appreciate if you could clarify the terminology here to be more consistent with existing literature. Since you’re referring to primary productivity here, why not use (N)PP?

L. 275: Maybe “quantities” rather than “parameters”? (see also title)

L. 275: Do all considered CMIP models parametrize Chl:C variability in the same/a similar way?

L. 280: This addition of remote sensing might confuse the reader in this paragraph about the models.

L. 287: I think you’re introducing the different phytoplankton types way too late. You’ve already been mentioning diatoms and other types throughout the text. Please build your paper in a logical way, so that things are properly introduced when first mentioned.

L. 291: Nissen et al. (2018) is not a good reference for this point. I suggest citing Luo et al. (2012) instead.

L. 293: The citation of Nissen et al. (2018) should be moved here.

L. 293: I don’t understand what you mean by “90 distinct models”.

L. 295: If you’re referring to all CMIP models that include 2 phytoplankton function types, isn’t the second type typically a mixed phytoplankton, i.e., combining different size classes and not only picophytoplankton? Please double-check.

L. 296ff: If these two models are so different in their future projections, I am wondering which one of the two simulates a more realistic community composition for the present-day. Have you looked into that? Additionally, it is unclear, based on what you write alone, why you decided to show these two models in particular.

L. 301: closing parenthesis missing

L. 301ff: I am not sure I follow your argument here. I assume that the two models you’re showing here are already different in their present-day community composition. If one is more realistic than the other, there might not be a *fundamental* problem regarding the parametrization of phytoplankton functional types across models, but instead only in some of the models. Additionally, models account for phytoplankton-zooplankton already. While I agree with you that more work needs to be done on that end, a more nuanced statement might be necessary here to better reflect where the shortcomings of current models are.

L. 314: delete “ocean”

L. 329: This sentence about seasonal progression appears disconnected from the topic sentence of this paragraph. Please revise.

L. 348: Reading through the paper, this section title confused me, as you have referred to CMIP6 projections throughout the paper already. I suggest revising the section structure. Additionally, please check for redundancies throughout the manuscript. I suggest including Laufkötter et al. (2015) in this section.

L. 384: How much do you think this statement would change if CMIP models included more diversity for the high-latitude phytoplankton community? I am not convinced that this is a robust statement, given all the uncertainties surrounding CMIP-type projections in high-latitude waters.

L. 396ff: Please list some of the processes you’re referring to here.

L. 404: Isn’t the lack of eddies to a first degree a result of the rather coarse model resolution?

L. 408: Please rephrase “diversity in fate of phytoplankton species”. It sounds odd to me.

L. 409: Replace “required for improving” by “which needs to be filled to improve”

L. 414: It very unusual to have a method section as the last section of a manuscript. As you could see from my detailed comments, there was a few locations where I was looking for information on the methods, suggesting that the current placement of this section might not be the best (especially since this section is not referenced anywhere in the text, implying that the reader does not even know it exists). I suggest moving the method section to a more “traditional” location in the revision process. Further, please include information on how many and which models were included in the main text.

L. 419: “quantity” instead of “parameter”

L. 421: I don’t understand what these “compatibility issues” are. Can you clarify? As it is, I don’t find this very convincing, as data structures can be homogenized in postprocessing, and I am left wondering why some models were (seemingly randomly) excluded from the analysis.

Fig. 1: I really appreciate that you included a sketch! Just three minor comments: Please add “phytoplankton” before “size class” (otherwise it seems like it’s referring to bloom size classes, which is not what you mean I think). Further, the arrows related to upwelling should not (all) come from the seafloor. In my view, the way it is currently drawn is not an accurate representation of what is happening in the open ocean. Note also that the cited paper for the bottom warming only refers to warming of bottom waters on Antarctic continental shelves (Purich & England 2021).

Fig. 1 and Supplement: Note that ideally, there should be no references in the supplementary material that are not also cited in the main text. At the moment, Purich & England (2021) and McNeil et al. (2008) are only cited in the supplement. I suggest adding the references of the numbers given in Fig. 1 to the Figure caption in the main text.

Fig. 4: Why is iron limitation only shown for the GFDL model?

Fig. 5: I find it a bit difficult to identify the individual models for the different shown quantities. Maybe by showing the model spread as a shading around each multi-model mean this figure would be easier to read.

Cited references (not included in manuscript by Fisher et al.):

Deppeler, S. L., & Davidson, A. T. (2017). Southern Ocean Phytoplankton in a Changing Climate. *Frontiers in Marine Science*, *4*(February). https://doi.org/10.3389/fmars.2017.00040

Dutkiewicz, S., Morris, J. J., Follows, M. J., Scott, J., Levitan, O., Dyhrman, S. T., & Berman-Frank, I. (2015). Impact of ocean acidification on the structure of future phytoplankton communities. *Nature Climate Change*, *5*(11), 1002–1006. https://doi.org/10.1038/nclimate2722

Freeman, N. M., Lovenduski, N. S., Munro, D. R., Krumhardt, K. M., Lindsay, K., Long, M. C., & Maclennan, M. (2018). The Variable and Changing Southern Ocean Silicate Front: Insights From the CESM Large Ensemble. *Global Biogeochemical Cycles*, *32*(5), 752–768. https://doi.org/10.1029/2017GB005816

Krumhardt, K. M., Lovenduski, N. S., Long, M. C., Levy, M., Lindsay, K., Moore, J. K., & Nissen, C. (2019). Coccolithophore Growth and Calcification in an Acidified Ocean: Insights From Community Earth System Model Simulations. *Journal of Advances in Modeling Earth Systems*, *11*, 2018MS001483. https://doi.org/10.1029/2018MS001483

Laufkötter, C., Vogt, M., Gruber, N., Aita-Noguchi, M., Aumont, O., Bopp, L., Buitenhuis, E., Doney, S. C., Dunne, J., Hashioka, T., Hauck, J., Hirata, T., John, J., Le Quéré, C., Lima, I. D., Nakano, H., Seferian, R., Totterdell, I., Vichi, M., & Völker, C. (2015). Drivers and uncertainties of future global marine primary production in marine ecosystem models. *Biogeosciences*, *12*(23), 6955–6984. https://doi.org/10.5194/bg-12-6955-2015

Luo, Y.-W., Doney, S. C., Anderson, L. A., Benavides, M., Berman-Frank, I., Bode, A., Bonnet, S., Boström, K. H., Böttjer, D., Capone, D. G., Carpenter, E. J., Chen, Y. L., Church, M. J., Dore, J. E., Falcón, L. I., Fernández, A., Foster, R. A., Furuya, K., Gómez, F., … Zehr, J. P. (2012). Database of diazotrophs in global ocean: abundance, biomass and nitrogen fixation rates. *Earth System Science Data*, *4*(1), 47–73. https://doi.org/10.5194/essd-4-47-2012

Nissen, C., Gruber, N., Münnich, M., & Vogt, M. (2021). Southern Ocean Phytoplankton Community Structure as a Gatekeeper for Global Nutrient Biogeochemistry. *Global Biogeochemical Cycles*, *35*(8), 1–23. https://doi.org/10.1029/2021GB006991

Seifert, M., Nissen, C., Rost, B., & Hauck, J. (2022). Cascading effects augment the direct impact of CO2 on phytoplankton growth in a biogeochemical model. *Elementa: Science of the Anthropocene*, *10*(1). https://doi.org/10.1525/elementa.2021.00104