

Review of the manuscript « Climate driven shifts in Southern Ocean primary producers and biogeochemistry in CMIP6 models».

This is the third time authors submit this paper. I again acknowledge that most of the introduction stays. Results and discussion section has been further simplified, which is something I acknowledge as well. I criticized the old versions mostly because I think that original results presented in the text were diluted within a review, instead of highlighting the relevance of the present work. I am happy to say that this version further improves this issue.

I still think that Table 1 can be presented as supplementary material.

I acknowledge as well the inclusion of some discussion on the effect of zooplankton grazing.

I think the paper has benefited from the changes made in the previous two iterations.

Specific comments about the text and figures:

Line 46-47: “As the ocean's buffering capacity increases, atmospheric CO<sub>2</sub> concentrations weaken (Jiang et al., 2019), and the role of pelagic ecosystems is expected to become more important in the Southern Ocean's carbon uptake. (Henley et al., 2020)”.

Line 58: I will remove “across the Southern Ocean”, as it is not the only place where phyto fuels ecosystems.

Line 60: I would say something like: “which is the scenario that comes closest to representing the current climate trajectory”.

Line 96-100: I think this can be removed.

Line 125: we?

Line 184: May acronyms be defined for these parameters.

Line 275: “...where iron-manganese...” can be removed, and “yet only iron...” joined with precedent sentence.

Line 334: I think authors mean that changes in phytoplankton growth don't result from acidification in CMIP6 results, as they explain after how these effects have impacts.

Line 390: Why these numbers have been corrected?