We thank the Reviewer for their constructive comments, which have helped improve the quality of the manuscript. Please find detailed responses to the comments raised below along with excerpts from the revised manuscript given in boxes.
We have worked in particular on the introduction and discussion sections to emphasize the differences between changes expected in the future and the changes we analyze here based on our Last Interglacial simulation.

Reviewer 1:
General comment:
This paper analyses the Southern Ocean carbon cycle from simulations of the Australian Community Climate and Earth System Simulator Earth System Model, ACCESS-ESM1.5, which has been applied for the PMIP4 scenario lig127k on the Last Interglacial (LIG). The argument is made that since this was the warmest interglacial of the last 1 million year it might serve as an analog for changes which might be expected in the future due to anthropogenically caused glabal warming.
The methods and results are well written, and the figure are very informative. I suggest some improvement in the introduction during the framing of the research question (see details below).
However, my major point is that I have some difficulties with this suggested analogy of the Last Interglacial with future warming. The analysis presented here shows that during the simulated LIG the westerly winds have been shifted equatorwards resulting in weakenend winds south of 50°S. This process is then responsible for different upwelling pattern and is important for quite a bit of changes in the Southern Ocean carbon cycle. For the future warming, it is now anticipated that westerly winds will shift polewards and get stronger, thus the opposite of what has been found for the LIG. I therefore strongly suggest to reframe the article in a way that its interpretation is largely restricted to the LIG. This reframing probably includes a change in the title. I would even go so far in pointing out in the dicsussion, that due to these shifts in wind pattern found here the LIG is no good analog for what to expect from future warming for the Southern Ocean carbon cycle. The reason for these differences in winds patterns are suggested to be due the the changes in orbital parameters (which seems to make sense), and this shows that past analogies for the future are often problematic.

We agree with the Reviewer's comment and have now changed the manuscript accordingly. Changes in the westerlies as simulated in our LIG experiment are indeed opposite to the observed changes over the past 20 years and to the projected changes over the coming century. This was mentioned on p1 L. 21, p2 L. 28 and p10 L. 297. We are now further clarifying the differences between our LIG simulation and future projections, as presented below. However, since simulated Southern Ocean SSTs are higher than PI and sea-ice cover is reduced by up to 41%, we do believe that our LIG simulation allows us to study the carbon cycle response to generally warmer conditions. We therefore decided not to change the title, which is very general and does not refer to future climate change.

i. Abstract:

The projected strengthening and poleward shift of the SH westerlies coupled to warmer conditions at the surface of the SO should thus weaken the capacity of the SO to absorb anthropogenic CO_2 over the coming century.

ii. Introduction in p1-2 L. 19-23:

The SO CO_2 uptake weakened during the 1990s due to a strengthening and poleward shift of the Southern Hemisphere (SH) mid-latitude westerlies (Lovenduski et al., 2007; Le Quéré et al., 2007; Zickfeld et al., 2007, Gruber et al., 2019a, b), but strengthened in the 2000s due to cooling over the Pacific and increased stratification over the Atlantic and Indian sectors of the SO (Landschuetzer et al., 2016; Gruber et al., 2019b).

iii. Introduction in p2, L. 28-29:

At the same time, SH westerlies are projected to strengthen and shift poleward over the coming century (Collins et al., 2013; Zheng et al., 2013; Goyal et al., 2021).

iv. We have removed the comparison with future simulations in P9, L263-264 in the Discussion.

v. We have also completely reworded the concluding statements to explicitly clarify that the LIG is not a good analogue for future changes. The current concluding paragraph in Discussion in P10, L. 302-306 now reads:

SH westerlies are projected to strengthen and shift poleward over the coming century (Collins et al., 2013; Zheng et al., 2013; Goyal et al., 2021), contrary to the LIG simulations presented here. Thus, changes in the carbon cycle simulated at the LIG may not serve as good analogues for potential future changes. Nevertheless, the simulated enhanced SO CO_2 outgassing despite a slight equatorward shift of the westerlies support a weaker capability of the Southern Ocean to take up anthropogenic CO_2.

Detailed comments:

lines 18-19: „of which 40% has been attributed to the SO" It is not clear on what this 40% is related to since before it is said "Both land and ocean act as sink". Does it refer only to the ocean part? And you probably mean that land and ocean EACH absorbs 25% of the anthropogenic emissions. You might furthermore consider citing the most recent version of the global carbon project here, thus „Friedlingstein et al 2020" (instead of 2019) https://doi.org/10.5194/essd-12-3269-2020â

A. We have now clarified this. The text now reads:

Both land and ocean presently act as sinks of anthropogenic carbon, each absorbing about 25% of anthropogenic emissions (Friedlingstein et al., 2020), with 40% of the ocean sink being attributed to the SO (Caldeira and Duffy, 2000; DeVries, 2014).

line 35: „The Last Interglacial (LIG, 129-115 thousand years ago, ka) was the warmest interglacial of the last million years". This statement is problematic, since the paper PAGES2016 cited here analyse only the last 800 kyr.

A. We have corrected this in the text, which now reads:

The Last Interglacial (LIG, 129-115 thousand years ago, ka) was the warmest interglacial of the last 800 thousand years (Masson-Delmotte et al., 2013; PAGES, 2016).

line 36ff: „The warmer climate at the LIG is primarily attributed to a stronger northern hemispheric summer insolation (Laskar et al., 2004) owing to the orbital configuration of higher eccentricity and obliquity (Berger, 1978), rather than higher greenhouse gas concentrations as projected for the future". The role of orbital forcing vs greenhouse gases on temperature have been analysed in detail in Yin and Berger 2012 DOI 10.1007/s00382-011-1013-5.

A. We now added this in the text:

The warmer climate at the LIG is primarily attributed to a stronger northern hemispheric summer insolation (Laskar et al., 2004) owing to the orbital configuration of higher eccentricity and obliquity (Berger, 1978), rather than higher greenhouse gas concentrations as projected for the future. The role of orbital forcing versus greenhouse gases on temperature have been analysed in detail in Yin and Berger (2012).

line 38: „The LIG is associated with sea levels 6-9 m higher than pre-industrial (PI) (Dutton et al., 2015)". This knowledgee on LIG sea level has recently been revised downward, please reframe according to Dyer etal (2021) https://doi.org/10.1073/pnas.2026839118.

A. We have now amended the text and added a reference to (Dyer et al., 2021):

The LIG is associated with annual mean SSTs around 0.5˚C higher than pre-industrial (PI) (Capron et al., 2014; Hoffman et al., 2017) and sea level 6-9 m higher than PI (Dutton et al., 2015, Rohling et al., 2019), although a recent study suggests the LIG sea level was 1.2 to 5.3 m higher than present-day (Dyer et al., 2021).

lines 91-94: Two different DIC tracers. I cannot remember that one of the tracers (the one not being constant at 280 ppm) is ever mentioned again. If so, it can be deleted here. You should also mention here, that since atmospheric CO2 is prescribed this approach misses the feedbacks which are related to CO2 in/outgassing. Also, absolute CO2 fluxes are biased since *the C cycle is simplified by this fixed CO2, which is acceptable for these interglacial conditions, but nevertheless might introduce a bias.*

A. We think that it is best to mention the two different tracers to avoid confusion, as in most models the CO_2 value used for the radiative forcing would also be used to force the marine carbon cycle. However, the Reviewer raises valid concerns regarding missing feedbacks and biases, and we have now addressed these concerns in the discussion section, and have added the following:

> All our analysis is based on a constant atmospheric CO_2 concentration of 280 ppm to allow quantification of the effects of the LIG climate on the carbon cycle independently of the background CO_2 concentration. However, this constant atmospheric CO_2 concentration neglects feedbacks related to CO_2 uptake and outgassing. Nevertheless, the lower CO_2 at LIG (275 ppm) compared to PI (284.3 ppm) would suggest the LIG SO to be an even greater CO_2 source to the atmosphere, implying a stronger sink somewhere else in the ocean or on land (Brovkin et al., 2016).

line 110: γ_{SST} = 0.0423°C^{-1} is called the Revelle factor. I am completely lost here. For me, the Revelle factor R is the relative change in atm CO2 over the relative change in DIC (unitless) R = Δ(CO2)/CO2 / Δ(DIC)/DIC, eg. Egleston et al (2010) doi:10.1029/2008GB003407, while you here describe some temperature-dependency. Please revise, or explain.

A. The reviewer correctly points out that the Revelle factor for DIC is unitless and represented as R = Δ(CO_2)/CO_2/ Δ(DIC)/DIC (Equation 2). However, the temperature sensitivity of CO_2 is slightly more complicated. This is because the equilibrium constant for solubility itself has a temperature dependence. This temperature dependence of solubility leads to a logarithmic relationship between temperature and CO_2 (Equation 8.3.4 in Sarmiento and Gruber, 2006), and has been verified experimentally (Takahashi et al., 1993):

$$\frac{1}{pCO_2}\frac{\partial pCO_2}{\partial T} = \frac{\partial \ln pCO_2}{\partial T} = \gamma_{SST} \approx 0.0423^{o}C^{-1}$$

Whereas for DIC, the sensitivity is unitless and referred to as the 'Revelle factor' (Equation 8.3.14 in Sarmiento and Gruber, 2006):

$$\frac{DIC}{pCO_2}\frac{\partial pCO_2}{\partial DIC} = \frac{\partial \ln pCO_2}{\partial \ln DIC} = \gamma_{DIC}$$

We have now clarified the temperature 'sensitivity' in the methods section 2.2.

> γ =0.0423˚C^{-1} is the sensitivity of pCO_2 to changes in SST (Sarmiento and Gruber, 2006). This SST sensitivity results from the equilibrium constant of solubility being temperature-dependent.

line 19: weaker and stronger upwelling: by how much stronger or weaker?

A. We have now updated the text to clarify the changes in upwelling in section 3.1, which now reads:

This leads to ~10% weaker upwelling south of 55°S and up to ~20% stronger upwelling north of 55°S.

Fig 3g: xaxis title is missing

A. This was fixed, thank you.

Reference:

1. Dyer, B., Austermann, J., D'Andrea, W. J., Creel, R. C., Sandstrom, M. R., Cashman, M., Rovere, A., and Raymo, M. E.: Sea-level trends across The Bahamas constrain peak last interglacial ice melt, Proc. Natl. Acad. Sci., 118, 2021.
2. Sarmiento, J. L. and Gruber, N.: Ocean biogeochemical dynamics, Princeton University Press, 2006.
3. Takahashi, T., Olafsson, J., Goddard, J. G., Chipman, D. W., and Sutherland, S. C.: Seasonal variation of CO2 and nutrients in the high-latitude surface oceans: A comparative study, Global Biogeochem. Cycles, 7, 843–878, 1993.