Reviewer 1:

This study focuses on understanding the interannual and decadal variability in Southern Ocean CO_2 fluxes and their links to the Southern Annular Mode (SAM). This research is particularly significant because few numerical modeling studies investigate anthropogenic and natural carbon fluxes separately. As a result, this study is unique, aligns well with the scope of the journal, and makes a meaningful contribution to the existing literature.

Throughout the manuscript, I encountered difficulties following the connection between the figures and the text (see specific comments below). Additionally, certain statements in the manuscript are difficult to comprehend within the context provided (see specific comments below). Revising some of these points to create a more concise and coherent format would greatly benefit the manuscript.

Moreover, the authors emphasize that their study differs significantly from Lovenduski et al. (2008) due to the use of a high-resolution model. As a reader, when going through the introduction, I anticipated a more extensive discussion and conclusion section addressing the impact of using eddy-resolving models. Additionally, I expected to see some recommendations as a conclusion. However, this was not the case, as it was only briefly mentioned in one or two sentences in the discussion section. I suggest expanding the discussion on the effects of using high-resolution models in such studies, even in a broader context.

We thank the Reviewer for their helpful comments, which helped improve the manuscript. We are now connecting more clearly the text and figures, and have expanded the discussion and conclusion to highlight the use of an eddy-rich model. We have significantly modified the text and figures to include i) an improved and clearer comparison with observations, ii) an improved presentation of both full and detrended results, and iii) the inclusion of a similar simulation performed with the 1 degree version of the model.

We are answering all the Reviewer's comments below in blue, with suggested changes to the text in green.

Specific comments and minor changes

Methods:

Line 94: "has many improvements“

Could you provide more specific details regarding the improvements in the model? Additionally, please explain why the inclusion of "ocean biogeochemistry with two-way coupling to nutrient and algae carried in the sea ice model" is important for this study?

We are now adding more details on the main improvements that were made to the model from the version described in Kiss et al., 2020, and provide reference to the manuscript, which describes in more details the improvements (Solodoch et al., 2022).

That section now reads:

"ACCESS-OM2 is described in detail in Kiss et al. (2020), but the version presented here has many improvements as described in Solodoch et al., (2022). The main improvements relevant to this study are that the wind stress calculation now uses relative velocity over both ocean and sea ice (not just ocean), and the albedo of the ocean is now latitude-dependent following Large & Yager (2009)."

The sentence related to the nutrient and algae, which is an improvement of the model, was moved to the description of WOMBAT.

"This version also includes a two-way coupling of the ocean biogeochemistry with nutrient and algae carried in the sea ice model (Hayashida et al., 2021)."

Line 106: "The air-sea CO_2 exchange is a function of ….."

When reading this sentence, it seems to suggest that there is no effect of DIC concentration. However, this is not true. The authors also mention in the results (line 228, line 258-259) that outgassing primarily results from an increase in surface nDIC concentration.

This is amended as:

"The air-sea CO_2 exchange is a function of the difference in partial pressure of CO_2 at the air-sea interface, the wind speed (Wanninkhof et al., 1992) and sea ice concentration."

Line 132: "To better understand ….."

With the limited description provided, it is challenging to comprehend the method employed by the authors. They reference a substantial book that may not be accessible to everyone. As a result, a more detailed explanation of the equations (1 and 2) is essential in this manuscript.

We have now added some text to explain the derivation of the equations as follow:

"Oceanic natural pCO_2 is a function of nDIC, alkalinity (ALK) as well as ocean temperature and salinity.

Changes in pCO_2 can thus be described as:

$$\Delta pCO_2 = \frac{\partial(pCO2)}{\partial(DIC)}\Delta DIC + \frac{\partial(pCO2)}{\partial(ALK)}\Delta ALK + \frac{\partial(pCO2)}{\partial(Sal)}\Delta Sal + \frac{\partial(pCO2)}{\partial(T)}\Delta T$$

To better understand the processes leading to pCO2 changes, we can estimate the pCO_2 change from each of the above variables separately. Broecker et al., (1979) derived that if ALK, salinity and temperature are constant then:

$$\frac{\partial ln(pCO2)}{\partial\, ln\,(DIC)} = \gamma DIC$$

With γDIC being the Revelle factor of DIC.

Equation 1 can be re-written as:

$$\frac{DIC}{pCO2} \cdot \frac{\partial(pCO2)}{\partial(DIC)} = \gamma DIC$$

One can then derived the pCO_2 change due to a change in DIC (ΔpCO_2dic) as:

$\Delta pCO2_{DIC} = \gamma DIC \; pCO_2 \; \Delta DIC / DIC$,

Here we use a mean high latitude estimate of γDIC of 13.3 (Gruber & Sarmiento, 2006) to estimate ΔpCO2dic.

pCO_2 sensitivities to ALK and salinity can be derived with similar equations:

$\Delta pCO2_{alk} = \gamma ALK \; pCO2 \; \Delta ALK / ALK$,

$\Delta pCO2_{Sal} = \gamma Sal \; pCO2 \; \Delta Sal / Sal$,

With γALK of -12.6, and γSal of 1 (Gruber & Sarmiento, 2006).

Finally, Takahashi et al., (1993) suggest that the pCO_2 sensitivity to temperature (T) follows the relationship:

$\frac{\partial lnpCO2}{\partial T} \sim 0.0423$ °C^{-1}

This implies (Takashi et al., 2002 & 2009) that the change in pCO_2 due to temperature is:

$\Delta pCO2_T = (e^{(0.0423*\Delta T)} - 1) \; pCO2$

Results:

Line 154-155:

"tco2 fluxes can be compared to observational ….."

A more detailed description of the observations and the model, along with quantification, is needed. I expect the authors to present a comparison, explaining their approach and how the models and observations compare. Furthermore, the manuscript contains a statement about being in agreement with observations. How and where can a reader verify this? If applicable, please clarify the connection to the relevant figures.

To avoid confusion, we want to make clear that, as per its title, this section compares mean simulated and observationally-derived CO_2 fluxes, i.e. the longest time-average covered by both simulation and data. Time-varying fluxes are then described in section 3.2.

The self-organizing map-feed-forward neural network (SOM-FFN) provides observational estimates of total CO_2 flux for years 1982-2021 (Landschutzer et al., 2019). To appropriately assess the mean model performances, simulated tCO2 fluxes (Fig. 1b) are compared to the time-average of the SOM-FFN observational estimates (Fig. 1a). To make this clearer, we are now adding more information about the observational estimates as well as a more detailed comparison between the mean simulated fluxes and estimates.

The paragraph starting L.153 now reads:

"We first assess the performances of the model by comparing the time-mean simulated SO tCO_2 fluxes to observational estimates (Fig. 1a,b). The Surface Ocean CO_2 ATlas version 6 (SOCATv6) [Bakker et al., 2016] provides surface ocean CO_2 measurements. However, due to the spatial and temporal heterogeneity of these measurements, it does not provide an appropriate dataset for a comparison with simulated fields. To fill this gap, Landschutzer et al., (2016) developed a method to provide a global gridded monthly observational estimate. The ocean is first clustered into biogeochemical provinces using Self-Organizing Map (SOM). Then, within each biogeochemical province, pCO_2 estimates are generated based on a non-linear relationship between the SOCATv6 observations and the CO_2 driver variables through a feed-forward neural network (FFN) approach.

If averaged over the available period of 1982-2021, the observationally-derived SOM-FFN dataset (Landschutzer et al. 2020, Fig. 1a) displays a strong tCO_2 uptake north of 50S (-1.59 mol/m^2/yr, zonal averaged between 50S and 35S) and a weak tCO_2 uptake (-0.38 mol/m^2/yr) south of 50S, even though there are some areas with outgassing (~0.2 mol/m^2/yr) south of 50S.

These features are relatively well reproduced by the simulated tCO_2 fluxes (Fig. 1b), which display a similar strong uptake (-1.59 mol/m^2/yr) north of 50S, that is north of the SAF (Sokolov et al. 2009). As in the observations, some tCO_2 outgassing is simulated south of the SAF, but particularly south of the PF. While both observational estimates and simulation suggest a tCO_2 outgassing south of the PF at 0-60E, 150E-180E and downstream of the Drake passage, the simulated tCO2 outgassing is particularly confined to some hotspots, namely over the eastern part of the Southeast Indian Ridge, east of the Drake Passage and over the Southwest Indian Ridge (Fig. 1b). Overall, a similarly weak tCO_2 uptake (-0.59 mol/m^2/yr) is estimated south of 50S."

Line 164:

"While simulated nco2 ….."

What is the reason for the sudden comparison of these two specific time periods (1980-1984 and 2017-2021)?

In the new Figures 5 and S3, we compare the end of the simulation with the beginning of the simulation. This highlights the impacts of the long-term positive SAM trend on the SO carbon cycle. The text has been modified to clarify this point.

Line 167:

"Through Ekman transport ….."

There seems to be no link between this statement and the figures included in the paper, so I suggest establishing a connection with the appropriate figure. Moreover, the manuscript first mentions the impact of phytoplankton on DIC here. I recommend elaborating on this effect in the methods section. If the effect is substantial, a more detailed description of the biogeochemical model (WOMBAT here) and a summary figure would be beneficial.

We are now adding a meridional Ekman transport into the new figure S3 as well as the zonally integrated detritus flux at 100m depth. The text now reads:

"Through Ekman transport, surface waters in the SO move equatorward (Fig. S3b), and nutrients and DIC are consumed by phytoplankton, leading to a maximum detritus flux at ~42S (Fig. S3d) and nCO_2 ocean uptake north of the SAF (Fig. S3e), where Antarctic Intermediate Waters (AAIW) and Subantarctic Mode Waters (SAMW) are formed."

Line 171:

"The nco2 ….. "

Could you please provide a reference to a particular figure where this information can be easily followed?

Figure 2c is mentioned L. 170. To make it clearer, we are now also referring to Fig. 2c in the sentence starting L. 171 and finishing L. 172.

Line 171:

"While this is compared SAM index ….. "

The comparison made by the authors is not evident within the paper, requiring readers to consult the Marshall 2003 paper. For improved clarity, could you add Marshall's data to the relevant figure and mention that figure in this statement?

We have removed the reference to Marshall et al. 2003 in that sentence, but have added a sentence in the Methods about the agreement between the SAM index derived from the JRA55-do dataset and the one of Marshall et al., 2003.

Line 178:

"The nco2 ….. "

The authors suddenly give a spatial pattern. What is the reason for this? It does not appear to be relevant to this subsection.

We understand this might not be the best location for this sentence, which was thus removed here.

Line 185:

Similar to the previous comment for line 178. The authors suddenly report a spatial pattern.

We have removed the reference to the spatial pattern here.

Line 189-193:

Is a correlation alone enough to indicate agreement with the observations? A more comprehensive explanation would be helpful.

This part was significantly modified. We now compare with two observational products and provide a more detailed comparison:

> "The simulated tCO_2 uptake increases by only 0.003 GtC/yr^2 between 1980 and 1998 (Fig. 2e), in agreement with both observational estimates (Fig. 2f). While the simulated tCO_2 uptake decreases between 1998 and 2001 as in the observations, the magnitude of this simulated change is smaller than in the observational estimates.
>
> Similarly, while both simulation and observational estimates display an increase in tCO_2 uptake in the early 2000s, the reinvigoration only lasts until 2003 in the simulation, while it lasts until 2010 in both observational datasets. Finally, similar to the SOCAT only product, the simulation suggests a stagnation of the tCO_2 uptake between 2011 and 2018, while the SOCAT+SOCCOM product suggests a decrease in tCO_2 uptake.
>
> While the simulated tCO_2 changes are within the uncertainty range of the observational estimates (+/-0.15 GtC/yr) (Bushinsky et al., 2019) for most of the simulated period, the simulated variations are lower and outside of the uncertainty range between 1998 and 2005."

Line 196:

"The detrended tco2 ….. "

This statement needs to be linked to a particular figure for better understanding.

References to figures were added:

"The detrended tCO_2 flux (Fig. 2h) thus presents variations similar to nCO_2 (Fig. 2c)..."

Line 205:

"Changes in SST ….. "

This information is not visible or easily accessible to the reader.

This part of the manuscript was significantly changed and the figure now shows a map of the trend in pCO2 due to SST changes.

Line 208:

" On the other hand ….. "

The year 2015/16 is unexpectedly mentioned. What is the reason for this, and where can we locate this information?

This part of the manuscript was significantly changed and we are now discussing the overall pCO_2 trends between 1980 and 2021.

Line 214:

" The inter-basin….. "

This point requires a clearer association with the relevant data or figures.

This part of the manuscript was significantly changed, but the changes in westerly wind in the different basins are shown in Fig. S5 (now Fig. S6).

Line 220-225:

The explanation is not easily understandable.

This part of the manuscript was significantly modified.

Line 250-251:

"This is however ….. "

Why is this the case? Where can the reader locate information that supports this statement?

This part of the text was removed.

Line 265:

The abbreviation AABW has not been mentioned in the text previously.

AABW is now defined as Antarctic Bottom Water.

Line 268:

It's unclear from the sentence which figure supports this statement. What is the relation between oxygen and remineralized DIC in your model, can you provide some more explanation?

While remineralization of organic matter consumes oxygen, in this context we simply wanted to show that the changes in dissolved oxygen were also showing enhanced proportion of old waters within the SO upwelling branch, while the dissolved oxygen content was reduced within AABW and AAIW.

We are now only showing natural DIC and remineralized DIC anomalies. The text was amended accordingly (now in Section 3.3.1).

Line 271-275:

This paragraph cannot be fully understood or supported by the figures presented in the manuscript. If the authors claim a relationship between a specific variable and weak biological pump efficiency, they should provide a clear link between their statements and the relevant figures. However, it appears that the claim of weak biological pump efficiency is not presented or supported by any of the figures in the manuscript. Therefore, the authors should consider revising their analysis or adding a new figure to better support this claim or revise their statements to more accurately reflect the evidence presented in their figures. In general, it's important for authors to ensure that their claims are well-supported by the evidence they present and to provide clear links between their statements and the relevant figures to help readers understand and interpret their results.

As this section was significantly modified, this part of the text was deleted.

Line 278:

The abbreviation "NADW" is not introduced or defined in the manuscript.

The abbreviation NADW is removed from the manuscript, and North Atlantic Deep Water is spelled out.

Line 281:

"At both ….. "

What is visible? If it is visible, please provide a reference to the relevant figure to support your claim or statement.

A reference to Figure 8 is now added.

Line 292-295:

Please provide a clear link to the relevant figure to support your claim or statement.

Following a comment from Reviewer 2, we decided to remove that part of the manuscript.

Line 296-299:

How can the reader follow the CDW in Figure 7? It needs a better description of the figure.

Following a comment from Reviewer 2, we decided to remove that part of the manuscript.

Discussion and conclusion:

Line 315-316:

What are the numbers. Please quantify.

We have now also added numbers for the decadal-scale variability in SO tCO2 fluxes as inferred from observational estimates (0.25 GtC/yr, L. 360).

Line 318-319:

"It should be noted …. "

What are the authors trying to convey with it? It would be better to be more specific.

We have rephrased this sentence as follow:

“ Such a mismatch between simulated SO tCO_2 variations and observations is prevalent in hindcast simulations (Gruber et al., 2019b), and could be due to an overestimation of the observed SO CO_2 flux variability (Gloege et al., 2021). The underestimation of the changes in tCO_2 uptake in the simulation could also be due a mis-representation of Southern Ocean stratification. “

Line 320-321:

"In addition, underestimation …. "

Does your model have this problem? If so, could you please mention it and show it first in the results section?

As indicated in the response to the comment on Line 189-193, this is now clearly mentioned in the Result section.

Line 345:

"we find that biological processes …. "

How did the authors reach this conclusion? There was not much related to biological processes throughout the results section. Could you please specify what you mean by 'biological processes'?

We are showing and discussing changes in detritus flux as a function of latitude for positive phases of the SAM compared to negative phases in figure 5. Now we are also showing the mean detritus flux as well as the changes occurring throughout the simulation. Changes in detritus flux are at least an order of magnitude lower than changes in air-sea CO2 fluxes as well as changes in DIC due to physical processes (i.e. Ekman pumping and diffusion at the base of the mixed layer). Nevertheless, this sentence was modified as follow:

As in previous studies, we find that changes in oceanic circulation are the primary driver of changes in SO CO_2 fluxes on decadal-time scales (Dufour et al., 2013, Resplandy et al., 2015, Nevison et al., 2019).

Figures:

Figure 1:

I suggest adding the PF and SAF contours to subfigure a.

This was added.

It would be helpful to provide the full names of the abbreviations 'PF' and 'SAF' in the figure label.

PF and SAF were re-defined in the figure caption.

Figure 2:

The x-axis should also include a tick mark for the year 1970.

The x-axis now includes a tick mark for the first year.

Figure 3:

The x-axis should also include a tick mark for the year 1980.

The x-axis now includes a tick mark for the first year.

Figure 4:

The unit of nCO_2 flux (mol C/ m2/yr) like in Figure 1?

The units for the CO_2 fluxes are now properly defined in all figure captions.

Figure 5:

What is the maximum and minimum extent of the y-axis in the plots?

What is meant by detritus flux, is it export production?

What is meant by "actual data"?

The maximum and minimum ticks were added to the y-axis. The caption was modified so that "actual data" is now replaced by "simulated fields". Within our modelling framework the detritus flux is similar to export production. This is now made clearer in the text.

Figure 7:

Why were these two time periods chosen for analysis?

Why is the contour in Figure 7(j) different from those in Figures 7(c), 7(f), and 7(l)?

What is it being compared to Gruber et al. 2019. ?

The manuscript was restructured and the analysis now focuses on the period 1980-2021. As such only the natural DIC anomalies are now shown in a figure that is being discussed with the processes leading to the long-term changes in natural CO_2 flux. Since we are not showing the total and anthropogenic DIC changes anymore, the comparison to Gruber et al., 2019 was removed.

Figure 8:

Please specify the time frame being discussed to avoid confusion.

The time period is now added in the caption of the figure.