nature portfolio

Peer Review File

Poleward migration of the dominant CO2 sink region in the Southern Ocean under high emission-scenario

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

6th Jul 23

Dear Dr Mongwe,

Your manuscript titled "A shift in the mechanism of CO2 uptake in the Southern Ocean under high emission-scenario" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

In addition, we highlight the following editorial thresholds:

1. - Provide compelling new insights into the relative contributions, strength and seasonality of mechanisms influencing CO2 uptake Southern Ocean.

2.- Provide an in-depth explanation and discussion of model uncertainty and spread, and discuss alternative hypotheses,

3.- Compare and evaluate model performance against available in situ observations (previous studies and Argo floats),

4.- Consider how nutrients and mixing variables may contribute to the criteria for air-sea flux drivers in the Southern Ocean.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Jose Luis Iriarte Machuca, PhD
Editorial Board Member
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Mongwe et al.: A shift in the mechanism of CO2 uptake in the Southern Ocean under high emission-scenario

The paper addresses the important issue of understanding and simulating the mechanisms that drive air-sea CO2 flux and its variability in the Southern Ocean in an ensemble of Earth System Models to reduce uncertainties in future projections of the Southern Ocean CO2 sink. Relative to present-day climate, the results show a shift in the contribution from different Southern Ocean regions to the total CO2 flux by the end of the century, in particular a change from the Subtropics as the largest contributor at present-day to the Antarctic at the end of the century. For the Antarctic, the paper shows a range of changes in the physical and chemical properties of the surface waters, that ultimately reduce the mixing-driven winter outgassing and leave the solubility as the major driver of winter fluxes. The authors postulate a “hybrid mode” of the future Antarctic waters, in which both summer and winter exhibit CO2 uptake but sue to different dominant drivers (biology and solubility, respectively).

While the paper shows a large selection of evidence to support and explain the changes especially in the Antarctic, it has a large range of major shortcomings. First of all, the results do not demonstrate a “shift in mechanism” as the title claims. On the contrary, the paper contradicts this at multiple locations, 1) the mechanisms do not change, but their relative contributions, 2) the CO2 sink region doesn’t shift, i.e. the Subtropics remain a large sink but the Antarctic becomes a stronger sink, 3) they show this is not a new steady state the system shifts into (see changes in sink 2100-2300) nor that it is a steady shift (stronger after 2060). The paper doesn’t discuss this further, although showing that the typical look at the end of the century for projections has its limitations would be an interesting point to add to the literature. I am not well familiar with the climate science literature post 2100, this might as well have been addressed elsewhere. One thing to mention though is that climate science shifts towards using global warming levels (i.e. comparing time periods when individual models hit a certain global warming threshold) instead of years because of the significant inter model differences – this has not been done here.

Additionally, although using an ensemble of ESMs and mentions the goal to reduce model uncertainty, the paper actually doesn’t address the uncertainty and model spread in great detail. If the objective is to advance our mechanistic understanding of the Southern Ocean carbon sink, I would expect a detailed look at the individual model’s mechanisms and how that affects the projected CO2 uptake. This aspect could be extended with the material contained in the paper, especially supplementary figures. Alternative hypotheses are not discussed and disproven to a satisfactory extent. A final general note is that one can argue the Southern Ocean has always been in a “hybrid mode” because the summer and winter drivers of CO2 flux are distinct at present-day already. The results in the paper show that the dominant winter drivers change in the Antarctic but no fundamental change to the fact that drivers differ of CO2 flux seasonally.

Additional to my content-related concerns I find this paper unclear and not well-structured. For example, switches between describing absolute values and rates within paragraphs make it difficult to follow the storyline and understand the key points. The wording is inconsistent, for example, how the data-driven pCO2 products are referred to, or how terms of thermal/nonthermal components are abbreviated. Also figures are inconsistent in how data is presented, titles, etc. Several aspects that are key for the understanding are not well described, such as that understanding the seasonal variability is key for understanding changes in annual mean CO2 uptake. The supplementary material also needs a major update, as some figures appear there twice, some figures are not mentioned anywhere in the text (main, methods, or supplementary), and the figure descriptions contain a significant number of typos.

All in all, the paper addresses a key question in our understanding of the Southern Ocean CO2 uptake but has major shortcomings related to content, clarity, and consistency. Below are more detailed comments to individual sentences.

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Comments

Content

Line 37-45: You frame your problem around reducing uncertainty, but you are in the end not investigating this – please make this introduction more relevant for your results. Also emergent constraints are based on and exploit the mechanistic understanding of drivers to reduce uncertainty, this link is not well explained in your introduction.

L. 52: What do you mean by seasonality? Is that the amplitude of the seasonal cycle?

Line 55ff: As detailed in the summary, you describe a change in dominating mechanism rather than
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the mechanisms themselves
Line 59: Great you are using dynamical boundaries for each model!! Are they all based only on temperature? Temperature can be biased in models compared to observations. Using a temperature criterion may therefore not capture the actual transitions between zones in models. Better use something like meridional gradients or prove that your dynamical boundaries capture the boundaries of zones. If you are claiming the drivers in each zone change it is essential to show you are comparing equivalent regions between models and observations
L. 76: exaggerated upwelling – do you have a reference for that claim?
L. 77: I think that refers to Fig. 2 not 1
L. 80 (and throughout the text): It feels like you mix up model spread, uncertainty, and standard deviations. Is the standard deviation of the models actually a good choice to measure uncertainty? The ensemble size of ESMs is rather small. The models with the same dynamical cores may cluster. Using the standard deviation assumes the ESMs reproduce a normal distribution. Maybe the model spread (i.e. range) is a better choice?
L. 85: ESM ensemble mean for Antarctic is within uncertainty of Sub-Antarctic, i.e. the difference is not significant
L.88 Say what this remarkably consistent pattern is here – is that that the CO_2 uptake increases in all zones, or that it is by the same amount/percentage in all ESMs, or something else?
L.89: Rapid growth suggests to me that this happened over a short period of time, which you do not look at.
L. 88-89: Instead (or additional) to the differences in absolute and relative terms would be an estimate of the change in contribution of each region to the total SO flux, which is the point you are making
L.109: Reference that "atmospheric pCO_2 is almost uniform in Southern Ocean"
L. 115: Not clear why the seasons play a role here (this may be clear at this point if the introduction was tailored better to your results). Seasonal variability is not phrased as a key player to understand the mechanisms of FCO_2.
L.115: this is impractical also because their dominant drivers may differ (regionally)
L.128: Not just in the Southern Ocean.
L.137: The use of the word "skill" is not necessary here, and as terminology describes a statistical measure that is not your target here
L.156: You investigate the pCO_2 variability, not FCO_2 variability, right?
L. 158ff: Have you masked the ice-covered regions in each model? If not, this may dampen the seasonal variability of SST and include regions in the SST analysis that do not contribute to the CO_2 flux (in the model)
L.162: here (and at some later places) you pick out some models from the ensemble and say there are mechanistic differences between models, without actually describing this. It would be beneficial to look at the (dominance of) mechanisms per model
L.165-166: Shows that your method has limitations, i.e. looking at d/dt cannot answer all questions, despite making models in certain ways more comparable – that would be nice to highlight or discuss somewhere
L.168-192: This part is heavily using correlated changes to explain causation. Therefore I would phrase this less definite, especially since you are not stating and disproving other hypothesis of links.
L.183: I disagree that dAOU/dt is symmetric (Fig 6h), and using the word "nature" suggests this is the way it should be naturally
L.186: Sentence starting with "This is" is not an explanation of the previous sentence, why NPP affects the subsurface DIC.
L.184-192: To be honest I unfortunately don't understand the mechanism you are describing here. It

seems to be a key aspect of your reasoning. This should be phrased more clearly.
L.205: What role does the increased open ocean area play? I.e. can you prove this is a minor effect in increasing the sink in the Antarctic region?
L.205: This sentence is too general and phrasing it this way is not correct: Technically, the uptake in the Subtropics doesn't change much, but the relative contribution changes between zones. And again, the mechanisms don't change per se, their strength does.
L.214: Is that the standard deviations again, or are you considering the actual model spread (which would be inconsistent with your previous measure of uncertainty)
L.215: "long-term" as in projected?
L.216: Do you mean small annual mean "changes"
L.226-229: That's very interesting! But unfortunately not mentioned again. So actually what you see in this multi-centennial run is that the changes by the end of the century are not a new steady state but the Southern Ocean sink will change further. That would be an interesting point to make for looking at the end of this century in general. Or at least discuss this point later.
L.238: I would phrase this differently: The Revelle factor only measures the effect of DIC changes on pCO2 changes, it is not a driver that amplifies in my view (I'm open to arguments against that view).
L.253: "nearly scale" – If it was scaling, I would expect the driving mechanisms stay constant but increase due to the increase in atmospheric pCO2. Given that almost all drivers in that region change, I'd argue that the factor by which the uptake increases is similar to the increase in atmospheric pCO2 is a coincidence.
L.256: "larger" in terms of amplitude, magnitude,…?
L.263: …And having open water for longer means longer time to warm
L.282: Why is a minimum temperature of 8 degrees relevant? Having these temperature values and ranges is not immediately tangible for knowing the implications on solubility
L.316: post-2060: That means the changes in CO2 flux drives is not a shift but more a sharp transitions? Like a tipping point? Also, in research comparing projections of different ESMs it is more common nowadays to use "Global Warming Levels" instead of years, because ESMs reach different global mean temperatures at very different times depending on their climate sensitivity
L.356: changes in overturning were not mentioned beforehand in the results. Could they be affecting the CO2 flux as well when e.g. assuming the export of carbon decreases in the scenario?
S24-26: I don't understand how organic matter cannot leave the winter mixed layer but at the same time be respired below the winter mixed layer.
S30: The table needs more information about the ESMs used, their components, etc. (similar to the comparison of pCO2 products)

Structure
L. 88-98: You switch between absolute difference, relative difference, uncertainty, and new FCO2. This makes it very hard to follow and get the point
L.205ff: I see you want to tell the reader what to expect in advance which is a great signpost. However, this is more of a conclusion that should be drawn at the end then stated upfront. Maybe you can instead state a hypothesis or the reasoning how you attempt to disentangle the drivers. If that doesn't work having it phrased as a conclusion is better than not having it at all.
L212-229: When first reading it I had difficulty understanding the story because it switches back and forth between seasonal changes and annual means. Maybe you can clarify

Figures

- Almost all figures would profit from adding grid lines so that one can read off the values
- F1: Inconsistent titles (ensemble vs ensemble mean). Inconsistent description (variability vs variability obtained from the one stdev)
- F2 goes from North (left) to South (right), F3 the other way around
- F2: description incorrect for which bar/colour is which
- F3: shows pCO2 difference in a different way to FCO2, i.e. the figures are not comparable, but is used in the text to support that dpCO2 is the key determinant of FCOs
- F4: have you tested if a person with a visual impairment (e.g. red-green) can differentiate the lines?
- F5: a and b what is the front? C and d: Why is one Antarctic and the other South of 50S? d: why no observations. In description: typo in last line (modal), and mention that black line is observations is missing.
- F6: Order of the subplots is different to order in the text. Why are some the absolute values and others the derivatives? Shading showing the uncertainty is not overlapping/see-through as in Fig 4 – adjust so that one can see the actual range. Typo in description (modal)
- F7f: that relation is not significant
- F7: description: repetition of "vertical", typo (modal)
- F8: Label of colourbar missing in a, b: why show both and not just one region? Typo in description (repetition)
- Figures in Supplementary: Some of them are not reference anywhere, S6 = S9, several figures descriptions/labels/titles need correction of typos

Writing and wording

You sometimes write long sentences with verbs at the far end. For a better understanding consider rephrasing so that it is clear early in a sentence where the journey is going. e.g. l.208-2010 "is described below" can go further up to let the reader know when this sentence is about as early as possible

Inconsistencies in writing

- Observation-based pCO2-products (l. 69), pCO2-products ensemble (l.73), observationally-derived pCO2-products (l. 82), pCO2-products (l.85), observed dpCO2 seasonality (l.133, pCO2 products are not observations)
- CO2 flux (e.g. l. 69) or CO2 sink rate (l.90) or FCO2 (l.52) or CO2 ingassing (l.112)
- Shift (most of the text) or migration (l.194)
- Write dpCO2T/dt (l.235) or (dpCO2/dt)T elsewhere
- L.276-279: You use dpCO2 to mean "air-sea difference" as well as "change between projection and contemporary"

You sometimes put references to Figures at places that the Figure doesn't refer to, e.g. l.244: the figure shows changes in SSS and SST, not how these changes affect stratification

L. 70: Minor quibble but I find the phrasing "within the front" a bit odd, call it along the front?

L.126-127: "oppose each other on a seasonal scale" could be clarified, maybe call it timing or phase of their variability?

L.141: "This is…" – I assume you mean that the Subtropics are dominated by the thermal component. The way it is written, "This" here means the summer/winter difference in solubility is due to the **range** in temperature changes, which it isn't.

L.161: "this feature", what is this referring to?

L.196ff: You may want to phrase information about projections this less definite – i.e. "under this scenario" rather than "in the future", as it is only one scenario you are considering and whether that

becomes true is not definite
L.197: “decreases poleward” refers to the subject of the sentence which is “Southern Ocean”, but you mean the warming signal decreases poleward
L.212: Unclear if the dpCO2 decreases or the change in dpCO2 decreases
L.236: I know that you mean the effect of the mean pCO2 on the amplitude of pCO2 changes given a certain change in temperature, but only because I am familiar with the subject. For a broader audience this needs explaining.
L.240: Instead of “while” do you mean “despite the amplitude decreases”?
L.248: “tracer” – you only talk about DIC, not tracers in general, and the DIC gradient does not affect the exchange of other tracers
L.262: “of the MLD [the] increases” – remove “the”
L.270: Lengthy first sentence that takes away the focus from the main message that is that the primary driver changes.
L.275: “ESMs” replace with “ocean” or “Southern Ocean”
L.278 and 281: Just omit “ice-free region” and write Sub-Antarctic and Subtropics to stay consistent
L.294-298: You switch from seasonal variability to annual means to uptake rates – can you phrase this in a consistent way
L.305: Just write “biological properties”
L.307: “biological CO2 uptake” can be misunderstood as forming biomass/photosynthesis, but you are referring to biologically-driven air-sea CO2 flux.
L.313-314: “which on one hand shoals and decreases the differences in MLD seasonal characteristics among ESMs” – this groups together separate topics i.e. the ensemble-mean MLD and the difference between ESMs, also where is “the other hand”.
L.340: Polar oceans probably won’t become similar to the Subtropics in absolute terms. But the dominating drivers of CO2 flux may be.
L.481: bar missing in last term
L.496-298: How does this comparison between Orsi and Fay& McKinley biomes relate to your work?

Reviewer #2 (Remarks to the Author):

The paper by Mongwe and colleagues uses Earth System Models (ESM) to examines mechanisms of CO2 uptake in the Southern Ocean (SO) and the influence of climate scenarios on patterns of uptake. They ground their analysis in observations, using multiple gridded products derived from observations as points of comparison to ESM estimates in the present, and a baseline for comparison of future scenarios. I am reviewing this paper from the point of view of an observational biological oceanographer with experience in biogeochemistry and a functional understanding of Earth System Models.

The paper builds on previous studies that noted polewards shifts in the CO2 sink rate by providing a detailed account of mechanisms driving this shift. It significantly contributes to the state of the art by identifying overarching mechanisms and linking them to specific oceanographic changes, with shifts in sea ice cover and impacts on mixed layer depth, heat absorption, and pCO2 uptake a dominant driver in the Antarctic. The paper acknowledges shortcomings of ESMs while nevertheless presenting patterns that are reasonable with respect to observations, presenting hypotheses as to the mechanisms driving shifts in future pCO2 that could be tested with present and future networks of observations (e.g. bio-argo as part of SOCCOM). I appreciate this balance, and attempts by the author to link model observations to paleoclimate studies. As a whole, the paper presents a nice narrative. While I remain curious as to the impact of the ESM biases on the conclusions of the paper (i.e., impact of stratification bias on the conclusion regarding pCO2 uptake), the paper presents a reasonable approach in integrating results from multiple ESMs, which constitutes an appropriate statistical approach. The data is presented in a consistent manner, which allows for easy interpretation even for someone not familiar with the detail of ESMs.

I would appreciate if the paper contextualized these changes along with other expected changes in the Antarctic, specifically changes in the ice sheet which will impact mixed layer depths and other processes discussed in the paper. Ice sheet dynamics are currently not coupled to ESMs, but a short review of possible implication for the mechanisms discussed here would be helpful. There have also been significant efforts to expand the observational networks for the Southern Ocean, with for example Gray et al. (2018) providing a SO-wide estimate of CO2 flux. I suggest adding a comparison to observation only datasets such as Gray et al. 2018 (i.e., non-spatially interpolated), perhaps in a supplement, as observations, while still sparse, are nevertheless the most direct data source for relevant parameters discussed in this paper.

The paper is well written, logically organized, interesting from both a modeling and observational point of view, and presents findings that are likely to guide future research. With minor edits, the paper is ready for publication.

Minor comments:

L 171 - wrong fig? Should be fig 6 a, b if NPP
L 171 - what is your threshold for start and end? It would be helpful for these and other phenology metrics to specify how you derived the timescales you are discussing, and include on the figures vertical bars to guide the eye of the reader. I found it hard to distinguish the month offset discussed in the paper
L 176 - no fig 5 h - should be 6 h

Fig 5 - Are observations missing in panel d.?
Fig 6 - add vertical bars for seasonality metrics (see comment above)
Fig 8 - edit labels, unclear as is. Add Year, replicate FCO2 to assist in comprehension

Mattias R. Cape

Reviewer #3 (Remarks to the Author):

The study addresses a fundamental question of how the Southern Ocean carbon sink will alter in the future. This region is highly complex in the present day involving ocean uptake in the subtropics north of the subtropical front and weak outgassing south of the polar front. There is a comparison with observational pCO2 products based the Surface Ocean CO2 atlas. There was though no mention of the Bio-Argo float based estimates that reveal stronger outgassing south of the polar front.

The study diagnoses the output of a set of Earth system models to document how the Southern Ocean carbon sink alters in the future, both in terms of latitudinal contrast in response and the effect of the controlling processes. This analysis is insightful and challenges an often-quoted viewpoint that the biological drawdown is likely to increase. Instead the primary response involves a seasonal, solubility-driven change involving the melting of sea ice leading to a shallowing of the mixed layer, decreasing the entrainment of carbon-rich deep waters and so leading to an uptake of CO2 south of the polar front.

I like this study and have found it to be insightful. I only have two comments designed to strengthen the work:

1. The mechanistic insight is provided by comparing an estimate of the temperature-driven tendency in pCO2 and the non-temperature-driven tendency in pCO2 involving the sum of mixing and biological effects. The relative magnitude of each of those contributions to the tendency in pCO2 are then illustrated and discussed. While that analysis is insightful, I did feel that combining together the mixing and biological effects missed a critical part of the story. My understanding of the outgassing south of the polar front is based upon the large pool of regenerated carbon in that region that is entrained into the winter mixed layer and then leads to the winter outgassing. This viewpoint was first set out by Ito and Follows (2005) JMR advocating the importance of preformed versus regenerated phosphate to understand the ocean sequestration of CO2. Taking that viewpoint further, Lauderdale et al. (2013) Climate Dyn. sets out how the different carbon pools are controlled and Lauderdale et al. (2016) GBC [cited] sets out the different drivers for the air-sea CO2 flux. The manuscript provides much additional information, but is missing any separation of nutrients or dissolved inorganic carbon into preformed or regenerated components. The study already shows the AOU, so it might be relatively easy step to show this split for nutrients and/or dissolved inorganic carbon and then gain some insight into how the supply of regenerated nutrients and carbon to the winter mixed layer has dramatically declined south of the polar front.

2. There is a plausible speculation that the new solubility feature is only active during relatively cool sea surface temperatures (L338). Can the authors go further and document any criteria for this response to hold that links to for example, the shallowing of the winter mixed layer, while retaining the presence of sea ice. I think adding criteria would be very useful that separate the emergence of the same sign in air-sea CO2 flux (the new polar response) versus the opposing signs in the air-sea CO2 flux linked to the seasonal pCO2 evolution (subtropical regime).

In summary, this study provide new mechanistic insight into how the Southern Ocean carbon uptake may vary in the future. Providing more detail of how the preformed and regenerated nutrient and carbon contributions compare may consolidate that insight given the processes acting south of the polar front. Adding more detail as the criteria for this new air-sea flux response to hold would be helpful.

Detailed points:
L85-86. There was though no mention of the Bio-Argo float based estimates that reveal stronger outgassing south of the polar front.
L97. FCO2 not yet defined.
L186-190. I think that point being made here is important, but as above recommend illustrating that response further.
L187. Should not really say that the subsurface DIC gradient is a key driver of entrainment in terms of causality. What I think you mean is that the entrainment flux of carbon varies in magnitude with the subsurface DIC gradient.

L262. Slip in the sentence construction.
L306 Better to qualify what “this” refers to.
L479 to 484 for equations (1) to (4). I recommend improving the explanation of these relationships. In the cited Mongwe et al. (2018) study there is a more complete explanation, where equation (2) is cited first with the coefficient based on an empirical fit to carbonate-chemistry coefficients. In addition, the non thermal term in (3) is split up into different contributions.

Ric Williams

Rebuttal report

A shift in the mechanism of CO_2 uptake in the Southern Ocean under high emission-scenario

New proposed title

Poleward migration of the dominant CO_2 sink region in the Southern Ocean under high emission-scenario

Dear Reviewers

Thank you for the extensive feedback we received; your feedback and comments were instrumental in improving the quality of the manuscript. The following is the full rebuttal report.

Reviewer 1

The paper addresses the important issue of understanding and simulating the mechanisms that drive air-sea CO2 flux and its variability in the Southern Ocean in an ensemble of Earth System Models to reduce uncertainties in future projections of the Southern Ocean CO2 sink. Relative to present-day climate, the results show a shift in the contribution from different Southern Ocean regions to the total CO2 flux by the end of the century, in particular a change from the Subtropics as the largest contributor at present-day to the Antarctic at the end of the century. For the Antarctic, the paper shows a range of changes in the physical and chemical properties of the surface waters, that ultimately reduce the mixing-driven winter outgassing and leave the solubility as the major driver of winter fluxes. The authors postulate a “hybrid mode” of the future Antarctic waters, in which both summer and winter exhibit CO2 uptake but sue to different dominant drivers (biology and solubility, respectively). All in all, the paper addresses a key question in our understanding of the Southern Ocean CO2 uptake but has major shortcomings related to content, clarity, and consistency. Below are more detailed comments to individual sentences.

Comment: First of all, the results do not demonstrate a “shift in mechanism” as the title claims. On the contrary, the paper contradicts this at multiple locations, 1) the mechanisms do not change, but their relative contributions. 2) The CO_2 sink region doesn’t shift, i.e. the Subtropics remain a large sink but the Antarctic becomes a stronger sink,

Reply: We thank the reviewer for his extensive feedback. The reviewer points out that mechanisms do not change but their relative contribution is true for the Subtropics but not so in the Antarctic region. Namely, the weakening of CO_2 uptake in the Subtropics in the projected climate is not caused by new mechanisms but by the imbalance of winter CO_2 uptake in summer; warming enhances the summer CO_2 outgassing. On the other hand, the emergence of the solubility-driven CO_2 uptake in the Antarctic region in the projected climate is a new Antarctic feature, this is not in the present contemporary climate. Models showing a solubility-driven CO_2 uptake in contemporary climate do not agree with observed estimates. Further, the melting of sea ice stimulates significant changes in the upper ocean, including the weakening of surface-subsurface DIC mixing and the enhancement of the seasonal warming-cooling rates (Fig. 7-8). Thus, we argue that the combination of these three changes represents a key regime shift in the Antarctic region in the projected climate. Nevertheless, in recognition of the reviewer's point, we adjusted the title of the manuscript to focus on poleward migration and leaving mechanisms change out of the title. The new proposed title is "Poleward migration of the dominant CO_2 sink region in the Southern Ocean under high emission-scenario". We also further refined the introduction and results section consistent with this title.. We also further refined the introduction and results section consistent with this title.

Comment: they show this is not a new steady state the system shifts into (see changes in sink 2100-2300) nor that it is a steady shift (stronger after 2060). The paper doesn't discuss this further, although showing that the typical look at the end of the century for projections has its limitations would be an interesting point to add to the literature. I am not well familiar with the climate science literature post 2100, this might as well have been addressed elsewhere. One thing to mention though is that climate science shifts towards using global warming levels (i.e. comparing time periods when individual models hit a certain global warming threshold) instead of years because of the significant inter model differences – this has not been done here.

Reply: We thank the reviewer for pointing this out. In the revised manuscript, we used the behaviour of CanESM2 beyond 2100 a key piece of evidence to supports the role of the warming feedback on weakening CO_2 uptake in the Subtropics in the projected climate in the discussion section.
The reviewer's suggestion to use global warming levels is well-taken and may be an essential tool to analyse the time of the emergence of the carbon cycle variables in addition to temperature.
Nevertheless, our since study is primarily focused on the mechanistic of the analysed changes in the position of the Southern Ocean CO_2 sink. We instead addressed this comment by modifying Fig. 8 to provide a more intuitive description of mechanism behind changes in the region of dominant CO_2 sink in the Southern Ocean. The revised Fig. 8 provides a more holistic view of key variables responsible for regime change in the Antarctic region in the projected climate.

Comment: Additionally, although using an ensemble of ESMs and mentions the goal to reduce model uncertainty, the paper actually doesn't address the uncertainty and model spread in great detail. If the objective is to advance our mechanistic understanding of the Southern Ocean carbon sink, I would expect a detailed look at the individual model's mechanisms and how that affects the projected CO2 uptake. This aspect could be extended with the material contained in the paper, especially supplementary figures. Alternative hypotheses are not discussed and disproven to a satisfactory extent. A final general note is that one can argue the Southern Ocean has always been in a "hybrid mode" because the summer and winter drivers of CO2 flux are distinct at present-day already. The results in the paper show that the dominant winter drivers change in the Antarctic but no fundamental change to the fact that drivers differ of CO2 flux seasonally.

Reply: We thank the reviewer for highlighting this point. We have made an effort to be more explicit in the description of model disagreement, explain possible sources of model bias, and where necessary use the supplementary figures to illustrate individual model behaviour. Overall, we found that the analysed ESMs show a good agreement in the Subtropics and Sub-Antarctic region as shown by the inter-model standard deviation, hence no further description of the individual models in the supplementary was included. The Antarctic region however generally shows a large model spread, and in the revised manuscript we highlight this point more clearly and discuss the possible source of bias from our analysis and literature.

This shown in **line 163 – 184**: "In the Antarctic region, the seasonal cycle of pCO_2 is primarily nonthermally controlled for both pCO_2 -products and ESMs in the contemporary climate ($M_{T\text{-nonT}} < 0$; Fig. 4 l), ESMs show a large spread for both ΔpCO_2 and $M_{T\text{-nonT}}$ in this region. We now examine how nonthermal processes (net primary production {NPP}, apparent oxygen utilization {AOU}, DIC, dDIC/dt, $dDIC_{preformed}$ /dt, and $dDIC_{regenerated}$ /dt) and physical forcings (dSST/dt, mixed layer depth {MLD}, stratification {estimated through d$\boldsymbol{\rho}$/dz} and sea-ice) regulate pCO_2 variability in the Antarctic region (Fig 5-7). Sea-ice is an essential distinguishing feature of Antarctic FCO_2 properties relative to the Sub-Antarctic and Subtropics. The seasonal presence of sea ice limits heat fluxes into the ocean [45,46] constraining the surface temperatures to near freezing (-1 < SST < 1 ºC, Fig. 5c-d). This keeps dSST/dt relatively low (Fig. 5j & S1), and hence the observed ($dpCO_2$/dt) T seasonal amplitude is lower than in the Subtropics and Sub-Antarctic region (Fig. 4f). While some ESMs shows this feature (e.g. CanESM5 and UKESM1-0LL, Fig. S3), not all ESMs' are consistent with the observed estimate, other models show a larger than observed dSST/dt (Fig. 5j). Reasons for the large model spread and bias in dSST/dt in the Antarctic remain unclear, nevertheless, possible sources of bias are stated below. We find that ESMs also show a large model spread in the MLD (Fig. 6 c-d) and stratification (d$\boldsymbol{\rho}$/dz subsurface maximum, Fig. 7a-b); ESMs generally overestimate stratification in the Antarctic region. Further, model temperature bias in the Southern Ocean is a well-known feature of CMIP models since inception 34 . Some studies have linked the Southern Ocean warm bias in models to in the AMOC related biases in models [66, 67] , other studies suggest that cloud-related biases manifesting through shortwave errors may be the source [69,38] . All these mechanisms may be responsible, causing biases in the vertical heat exchange and stratification in Antarctic region, and thus seasonal warming and cooling rate biases in the analysed ESMs."

Further, the reviewer points out a fair argument that the Antarctic region may already be considered a hybrid in a general sense, implying seasonality. This is however not consistent with our proposition, our findings suggest a hybrid CO_2 sink which is not the case in the present climate. The contemporary Antarctic is a CO_2 sink in the spring-summer seasons and a CO_2 source in winter. As explained above, the emergence of the solubility-driven CO_2 sink in the Antarctic winter is a new feature in projected climate, making the Antarctic a hybrid CO_2 sink. This argument has been further strengthening in the revised manuscript.

Comment: Additional to my content-related concerns I find this paper unclear and not well-structured. For example, switches between describing absolute values and rates within paragraphs make it difficult to follow the storyline and understand the key points. The wording is inconsistent, for example, how the data-driven pCO2 products are referred to, or how terms of thermal/nonthermal components are abbreviated. Also figures are inconsistent in how data is presented, titles, etc. Several aspects that are key for the understanding are not well described, such as that understanding the seasonal variability is key for understanding changes in annual mean CO2 uptake. The supplementary material also needs a major update, as some figures appear there twice, some figures are not mentioned anywhere in the text (main, methods, or supplementary), and the figure descriptions contain a significant number of typos.

Reply: We thank the reviewer for pointing this out. The revised manuscript has been significantly improved for the readability and presentation of the figures. We noticed that there was a sloppy mistake in the supplementary material and we apologize for this, we have updated the supplementary and removed unnecessary figures.

Comment: Line 37-45: You frame your problem around reducing uncertainty, but you are in the end not investigating this – please make this introduction more relevant for your results. Also emergent constraints are based on and exploit the mechanistic understanding of drivers to reduce uncertainty, this link is not well explained in your introduction.

Reply: We reframe the first paragraph of the introduction section to address this point,
This is shown in **line 37 – 47:** "The Southern Ocean (south of 30°S) takes up approximately 40% of ocean anthropogenic CO_2 and ~75% of excess heat [1–4] , making it one of the most pivotal ocean buffer of climate warming. In addition, it supplies 33 – 75% of the nutrients required for new primary

production in the global oceans [5–7] . Nevertheless, existing model projections indicate large uncertainty in the future Southern Ocean sink of anthropogenic CO_2 emissions[8] . In recent years, emergent constraints have shown success in constraining uncertainty in the Southern Ocean CO_2 sink projections [8,9] , nevertheless, changes in future mechanisms remain poorly understood. Understanding how climate warming alters the Southern Ocean's ability to regulate CO_2 and heat exchanges, and their governing mechanisms is crucial to strengthening our confidence in the simulated future changes. Further, improved process understanding of the behaviour of the Southern Ocean under extreme conditions like high-emission scenario is essential to anticipate related ecosystems and climate feedbacks in the future."

Comment: Line 59: Great you are using dynamical boundaries for each model!! Are they all based only on temperature? Temperature can be biased in models compared to observations. Using a temperature criterion may therefore not capture the actual transitions between zones in models. Better use something like meridional gradients or prove that your dynamical boundaries capture the boundaries of zones. If you are claiming the drivers in each zone change it is essential to show you are comparing equivalent regions between models and observations

Reply: We use dynamic boundaries based on subsurface temperature consistent with the Orsi et al. 1995 criterion. As the reviewer points out, choosing a dynamic boundary is a nontrivial exercise, particularly in a multi-model study where models show different strengths and weaknesses as well as compensating biases. Although subsurface temperature has limitations, we choose it because it provides a reliable and comparable indicator for distinguishing water masses of similar properties in different models and observed estimates. This approach is less impacted by high variability in the upper ocean where models differ the most. Having that said, it is also a reasonable expectation that the selected Orsi et al. (1995) based dynamic boundaries will slightly differ between the models, and in comparison with the observed estimate because of differences in the model's mean states. Nevertheless, Orsi et al. (1995) provide a standardized approach which is also comparable with previous studies. The reviewer may be correct that using temperature gradients is more accurate, nevertheless, using temperature gradients also requires choosing a boundary criterion that reasonably reflects physical-biogeochemical boundaries and deciding on the depth where models show the least disagreements and are comparable to previous studies. An alternative approach may be to use geographic boundaries where the selected surface area is consistent in all models, but using this approach compares different water masses due to differences in the model's mean state which is potentially a greater weakness. Consequently, addressing the problem of choosing appropriate boundary definition in the Southern Ocean is a stand-alone study which I'm addressing in independent lead by an MSc student.(Orsi et al., 1995)

Comment: L. 158ff: Have you masked the ice-covered regions in each model? If not, this may dampen the seasonal variability of SST and include regions in the SST analysis that do not contribute to the CO2 flux (in the model)

Reply: The ice-covered region has been not masked. We address this comment by stating explicitly that longer seasons of open water may have an effect (line 311 – 312). Nevertheless, contrary to the reviewer's suggestion we find that the melting of sea ice enhances seasonal warming and cooling rates (Fig. 8c). This enhancement of seasonal warming-cooling rates is key to shifting to the solubility-driven CO_2 uptake in the projected climate.

Comment. L. 52: What do you mean by seasonality? Is that the amplitude of the seasonal cycle?

Reply: Thanks for point this out, we meant amplitude; this sentence was rephase to clarify. This is shown in **line 55 – 58**: "The decrease of the CO_2 buffering capacity for example is projected to enhance biological-induced CO_2 uptake in summer, and amplify the seasonal cycle of air-sea CO_2 fluxes (FCO_2) as well as hydrogen ion concentration ($[H^+]$), both of which may have a significant implications for the efficiency of the ocean CO_2 uptake and marine calcifying organisms in the Southern Ocean[18] ."

Comment. Line 55ff: As detailed in the summary, you describe a change in dominating mechanism rather than the mechanisms themselves

Reply: We took note of this comment, have changed the title of manuscript to be consistent with study. "Poleward migration of the dominant CO_2 sink region in the Southern Ocean under high emission-scenario"

Comment. L. 76: exaggerated upwelling – do you have a reference for that claim?

Reply: Since we could find reference that support this speculation, we removed this exaggerated upwelling speculation:
The revised sentence is shown in **line 79-81**: "Differences between ESMs and pCO_2 products in the Subtropics mainly occur in the eastern Pacific, where most of the ESMs show a CO_2 outgassing feature."

Comment. L. 77: I think that refers to Fig. 2 not 1

Reply: The reviewer may be mistaken here, we verified that we are refereeing to Fig. 1

Comment. L. 80 (and throughout the text): It feels like you mix up model spread, uncertainty, and standard deviations. Is the standard deviation of the models actually a good choice to measure uncertainty? The ensemble size of ESMs is rather small. The models with the same dynamical cores may cluster. Using the standard deviation assumes the ESMs reproduce a normal distribution. Maybe the model spread (i.e. range) is a better choice?

Reply: We thank the reviewer for pointing this out and suggesting a solution, we corrected this throughout the manuscript and have standardized the description of uncertainty as the model spread throughout the text.

Comment. L. 85: ESM ensemble mean for Antarctic is within uncertainty of Sub-Antarctic, i.e. the difference is not significant

Reply: We thank the reviewer for pointing this out, we corrected this.
The revised sentence is shown in **line 89 – 91**: "The Antarctic region is the weakest CO_2 sink of the three subdomains in the contemporary Southern Ocean, showing an annual mean FCO_2 of -8.4±5.4 gC m^{-2} yr^{-1} in ESMs and -1.81±1.46 gC m^{-2} yr^{-1} in pCO_2-products (Fig. 2c)"

Comment. L.88 Say what this remarkably consistent pattern is here – is that that the CO2 uptake increases in all zones, or that it is by the same amount/percentage in all ESMs, or something else?

Reply: We were here referring to the switch in pattern where the lowest region CO_2 sink becomes the largest and verse visa. We clarified this in revised the text.
Line 93 – 95: "At the end of the 21st century (2080 - 2099) a remarkably consistent pattern emerges in the ocean carbon uptake across ESMs; the region of the weakest CO_2 sink in the contemporary climate becomes the most intense sink."

Comment. L.89: Rapid growth suggests to me that this happened over a short period of time, which you do not look at.
Reply: We thank the reviewer for point this out, the sentence has now been corrected:
Line 95 – 96: "Namely, the region of the strongest CO_2 sink shift poleward from the Subtropics to the Antarctic (Fig. 2a-c)."

Comment. L. 88-89: Instead (or additional) to the differences in absolute and relative terms would be an estimate of the change in contribution of each region to the total SO flux, which is the point you are making

Reply: We have added percentage contribution of each regions and how they change in the projected climate

Line 92 – 98: "The CO_2 sink increases by only 6.6±1.1 gC m^{-2} yr^{-1} in the Subtropical region, the smallest margin (43%) of the three subdomains. In the Sub-Antarctic region, the annual CO^2 sink increased by 208% relative to the contemporary period to -27.7±5.4 gC m^{-2} yr^{-1} . The Antarctic region on the other hand displays the most extensive net increase (-37.9±7.3 gC m^{-2} yr^{-1} , ~ 450%) becoming the largest CO_2 sink at the end of the 21st century (-46.4±10.1 gC m^{-2} yr^{-1}). The Antarctic region also carries proportionally the largest annual mean FCO_2 model spread from the contemporary climate, and it has the largest uncertainty in the future climate (Fig. 2c)."

Comment. L.109: Reference that "atmospheric pCO_2 is almost uniform in Southern Ocean"

Reply: We considered this to be general knowledge, the lack of land mass in the Southern Ocean makes atmospheric CO_2 seasonality neglect.

Comment. L. 115: Not clear why the seasons play a role here (this may be clear at this point if the introduction was tailored better to your results). Seasonal variability is not phrased as a key player to understand the mechanisms of FCO2.

Reply: This whole paragraph was designed to introduce the seasonal aspect of the study and the tools needed for the seasonal cycle interpretation. Results are only described in the next paragraph. The introduction section gave a high-level framing of questions and gaps we are addressing in this study without losing the reader on details of seasonal properties. This paragraph, therefore, provides the necessary seasonality properties needed for the subsequent analysis.

Comment. L.115: this is impractical also because their dominant drivers may differ (regionally)

Reply: In this particular case impractical refers to the virtualization of the seasonality results, regional differences are not the issue at hand since model annual mean differences can be compared on the same scale. Rather, our focus on seasonal scale properties requires a standardization/normalization of the models' variables to a comparable scale without losing information on their magnitudes. Hence the first-time derivative was a reasonable choice.

Comment. L.128: Not just in the Southern Ocean.

Reply: Thanks for point this out, this sentence is now corrected

Line 136 – 138: "The thermal and nonthermal components of pCO_2 oppose each other on a seasonal scale 32,33 (Fig. 4 d-f), and hence the larger of the two determines the observed seasonal cycle phasing of pCO_2, and ultimately FCO_2[29]."

Comment. L.137: The use of the word "skill" is not necessary here, and as terminology describes a statistical measure that is not your target here

Reply: Thanks for point this point, this sentence has been reframed, we removed the word skill. Line 146 – 147: "ESM's $M_{T\text{-}nonT}$ display good inter-model agreement with respect to the observed estimates in the Subtropics but show a degrading comparability poleward (Fig. 4 d-l)."

Comment. L.156: You investigate the pCO2 variability, not FCO2 variability, right?

Reply: Thanks for point this point, this whole paragraph has been reframed to explicitly state pCO_2 when refereeing to mechanistic links.

Comment. L. 158ff: Have you masked the ice-covered regions in each model? If not, this may dampen the seasonal variability of SST and include regions in the SST analysis that do not contribute to the CO2 flux (in the model)

Reply: The ice-covered region has been not masked. We address this comment by stating explicitly that longer seasons of open water may have an effect (line 311 – 312). Nevertheless, contrary to the reviewer's suggestion we find that the melting of sea ice enhances seasonal warming and cooling rates (Fig. 8c). This enhancement of seasonal warming-cooling rates is key to shifting to the solubility-driven CO_2 uptake in the projected climate.

Comment. L.162: here (and at some later places) you pick out some models from the ensemble and say there are mechanistic differences between models, without actually describing this. It would be beneficial to look at the (dominance of) mechanisms per model:

Reply: We thank the reviewer for pointing this out. Indeed the mechanistic description of the individual models would be insightful if the mechanisms each model and their source of bias were known, however, we do not know, and this is speculative. To address this comment we reframed this paragraph using literature to state possible sources of model spread and general model bias in the Antarctic region.

The revised text is show in **line 175 – 184**: "Reasons for the large model spread and bias in dSST/dt in the Antarctic remain unclear, nevertheless, possible sources of bias are stated below. We find that ESMs also show a large model spread in the MLD (Fig. 6 c-d) and stratification (d$\boldsymbol{\rho}$/dz subsurface maximum, Fig. 7a-b); ESMs generally overestimate stratification in the Antarctic region. Further, model temperature bias in the Southern Ocean is a well-known feature of CMIP models since inception 34 . Some studies have linked the Southern Ocean warm bias in models to in the AMOC related biases in models [66, 67] , other studies suggest that cloud-related biases manifesting through shortwave errors may be the source [69,38] . All these mechanisms may be responsible, causing biases in the vertical heat exchange and stratification in Antarctic region, and thus seasonal warming and cooling rate biases in the analysed ESMs.”

Comment. L.165-166: Shows that your method has limitations, i.e. looking at d/dt cannot answer all questions, despite making models in certain ways more comparable – that would be nice to highlight or discuss somewhere

Reply: Fair point and well taken, our study uses d/dt and d/dz mainly for scale purposes. In the revised manuscript, we state more explicitly results we cannot yet explain, particularly in the Antarctic region where models show a large differences. An example of this is shown in the comment right above.

Comment. L.168-192: This part is heavily using correlated changes to explain causation. Therefore I would phrase this less definite, especially since you are not stating and disproving other hypothesis of links.

Reply: Thanks for pointing this out. This whole paragraph was rephrased and split into two to provide a more careful and clearer narrative of the claims we cannot support and speculations not definite.

The revised text is shown in **line 186 – 235**: “In addition to constraining surface waters near freezing temperature, the seasonal presence of sea-ice also plays an essential role in regulating biological and physical-driven variations of upper ocean DIC. The simulated NPP seasonal cycle in the Antarctic region is linked to sea-ice variation; the NPP increase initializes only after the sea-ice maximum (September), this leads to one to two months offset in comparison to the ice-free Sub-Antarctic (Fig. 6b,d). Post the sea-ice maximum, light becomes available which initializes primary production and hence reducing surface DIC. The NPP-related surface DIC consumption is shown by dDIC/dt < 0 (Fig. 6f) during the high production season, and it coincides with the $(dpCO_2/dt)_{nonT}$ minima (Fig. 4f). The timing of the NPP seasonal maxima also aligns with a minimum in the apparent oxygen utilization rate (dAOU/dt) (Fig. 5 h). AOU is defined as the difference between oxygen at saturation and the in situ dissolved oxygen concentration; here, it is used to estimate respiration within the MLD[34] . Negative dAOU/dt magnitude when NPP is high reflects oxygen production during photosynthesis, whereas positive dAOU/dt is indicative of respiration or the oxidation of organic matter back to DIC in the near surface. Indeed, ESMs show positive dAOU/dt at the tail of the NPP maxima. Moreover, AOU and DIC rates are aligned, which highlights the role of biology in setting the DIC levels (Fig. 6 a-b & e-h). The nearly symmetric feature between dAOU/dt magnitudes and NPP seasonal phasing suggests that the NPP likely dictates respiration rates in the near surface. The decomposition of the DIC into preformed and regeneration (Fig. 5a-b, see methods for description) show that indeed, AOU is a reliable indicator of biological driven DIC variations; $dDIC_{regenerated}$ /dt, and dAOU/dt has a similar seasonal cycle phasing, and both follows NPP. Further, while NPP magnitudes are comparable between in the Sub-Antarctic and Antarctic region, we note that $dDIC_{regenerated}$ /dt, dAOU/dt and $(dpCO_2/dt)_{nonT}$ displays relatively larger seasonal amplitudes in the Antarctic region (Fig. 4 h-i, 6a-b). This is partly because the seasonal cycle of $dDIC_{preformed}$ /dt, and $dDIC_{regenerated}$ /dt is slightly out of phase in the Sub-Antarctic which as a dampening effect, while the nearly in phase seasonality in the Antarctic region has a superposition effect. A higher Revelle factor in the Antarctic region also be contributing factor (Fig. 5c).

In addition to the NPP links to surface respiration, we also find that NPP plays a key role in setting the subsurface vertical DIC gradients which in turn influences seasonal DIC entrainment and hence vertical DIC exchange rates (Fig. 7 a-f). ESMs with high NPP tend to have stronger vertical DIC gradients (Fig. 7 e-f, Fig. S4 d-f & S5 a-b). Using all nine ESMs, we find that subsurface $(dDIC/dz)_{max}$ and seasonal NPP max has a robust relationship in the Sub-Antarctic ($p < 0.01$) but a non-significant regression in the Antarctic region (Fig. 7 e-f). Nevertheless, given the relatively large model spread shown by nearly all nonthermal and physical processes in the Antarctic region, it might be that this relationship is robust with more models. The role of NPP in setting DIC entrainment rates in the Southern Ocean has also been shown in previous studies [35,36] , this relationship is re-iterated here to establish mechanistic links for the drivers of the nonthermal pCO_2 component. Further, the relationship between entrainment rates and NPP is also affected by near surface respiration. Nearsurface respiration plays a joint leading role in the timing of the DIC seasonal maximum in addition to entrainment mixing in the Antarctic region. This is in contrast to Sub-Antarctic region where ESM show clear a offset in the seasonal maximum of regenerated and preformed DIC with respect to total DIC (Fig. 5a). The ESMs dDIC/dt seasonal maximum in the Sub-Antarctic occurs in early winter consistent with deep MLD in early winter when the entrainment mixing expected to be strongest37 (Fig. 5a). In the Antarctic region, while ESMs show a large spread in the seasonal cycle of preformed and regenerated DIC (Fig. 5b), they broadly depict a two month early peak in dDIC/dt, and coincides with dAOU/dt and $dDIC_{regenerated}$ /dt. The peaking of dDIC/dt in the Antarctic occurs prior the maximum mixing in winter, instead it occurs while MLDs are relatively shallow (~ 80 m) (Fig. 6 b,f,h). This suggest that the Antarctic seasonal nonthermal pCO_2 variability maybe first-order driven by near-surface respiration and NPP but sustain the buoyancy mixing in the contemporary climate. The oxidation of near-surface organic matter may be playing a more significant role than previously thought on the seasonal variations of surface DIC in the Antarctic region."

Comment. L.183: I disagree that dAOU/dt is symmetric (Fig 6h), and using the word "nature" suggests this is the way it should be naturally

Reply: Thanks for point this out, we rephased this sentence in the revised text.

Line 194 – 205: "AOU is defined as the difference between oxygen at saturation and the in situ dissolved oxygen concentration; here, it is used to estimate respiration within the MLD34 . Negative dAOU/dt magnitude when NPP is high reflects oxygen production during photosynthesis, whereas positive dAOU/dt is indicative of respiration or the oxidation of organic matter back to DIC in the near surface. Indeed, ESMs show positive dAOU/dt at the tail of the NPP maxima. Moreover, AOU and DIC rates are aligned, which highlights the role of biology in setting the DIC levels (Fig. 6 a-b & e-h). The nearly symmetric feature between dAOU/dt magnitudes and NPP seasonal phasing suggests that the NPP likely dictates respiration rates in the near surface. The decomposition of the DIC into preformed and regeneration (Fig. 5a-b, see methods for description) show that indeed, AOU is a reliable indicator of biological driven DIC variations; $dDIC_{regenerated}$ /dt, and dAOU/dt has a similar seasonal cycle phasing, and both follows NPP"

Comment. L.186: Sentence starting with "This is" is not an explanation of the previous sentence, why NPP affects the subsurface DIC.

Reply: Thanks for point this out: This whole paragraph has been reframed, we also added preformed and regenerated DIC strengthen the mechanistic links description.

Line 219 – 235: "The role of NPP in setting DIC entrainment rates in the Southern Ocean has also been shown in previous studies [35,36] , this relationship is re-iterated here to establish mechanistic links for the drivers of the nonthermal pCO_2 component. Further, the relationship between entrainment rates and NPP is also affected by near surface respiration. Near surface respiration plays a joint leading role in the timing of the DIC seasonal maximum in addition to entrainment mixing in the Antarctic region. This is in contrast to Sub-Antarctic region where ESM show clear a offset in the seasonal maximum of regenerated and preformed DIC with respect to total DIC (Fig. 5a). The ESMs dDIC/dt seasonal maximum in the Sub-Antarctic occurs in early winter consistent with deep MLD in early winter when the entrainment mixing expected to be strongest[37] (Fig. 5a). In the Antarctic region, while ESMs show a large spread in the seasonal cycle of preformed and regenerated DIC (Fig. 5b), they broadly depict a two month early peak in dDIC/dt, and coincides with dAOU/dt and $dDIC_{regenerated}$ /dt. The peaking of dDIC/dt in the Antarctic occurs prior the maximum mixing in winter, instead it occurs while MLDs are relatively shallow (~ 80 m) (Fig. 6 b,f,h). This suggest that the Antarctic seasonal nonthermal pCO_2 variability maybe first-order driven by near-surface respiration and NPP but sustain the buoyancy mixing in the contemporary climate. The oxidation of near-surface organic matter may be playing a more significant role than previously thought on the seasonal variations of surface DIC in the Antarctic region."

Comment. L.184-192: To be honest I unfortunately don't understand the mechanism you are describing here. It seems to be a key aspect of your reasoning. This should be phrased more clearly. Reply: Things for point out that our description was clearer here. As descried above this paragraph was split to provide a refined the mechanistic description as it key part of reasoning as the reviewer point it out.
Reply. Thank you for pointing out this consistency, we significantly revised this text and added another paragraph to provide a rather description. The revised text is shown in lines 186 – 235 and is displayed two comments above.

Comment. L.205: What role does the increased open ocean area play? I.e. can you prove this is a minor effect in increasing the sink in the Antarctic region?
Reply: This is comment as been addressed previous comments above.

Comment. L.205: This sentence is too general and phrasing it this way is not correct: Technically, the uptake in the Subtropics doesn't change much, but the relative contribution changes between zones. And again, the mechanisms don't change per se, their strength does.

Reply: We removed this sentenced and rephased.

Comment. L.214: Is that the standard deviations again, or are you considering the actual model spread (which would be inconsistent with your previous measure of uncertainty)

Reply: We standardized the description of uncertainty as "model spread" throughout the manuscript consistent with the measured metric: inter-model standard deviation.

Comment. L.215: "long-term" as in projected?

Reply: We have corrected the use of "long-term" to be projected climate throughout the manuscript.

Comment. L.216: Do you mean small annual mean "changes"
Reply: Yes, we rephased to clarify.

Line 258 – 261: "North of the Polar Front, projected ΔpCO_2 seasonal averages (winter-summer) change has opposite signs, which leads to a relatively small annual mean, whereas south of the Polar

Front, ΔpCO_2 winter-summer averages have the same sign (Fig. 8c). Future changes in the relative contribution of the thermal and nonthermal $dpCO_2/dt$ components provides an insightful guide to the mechanistic links to these ΔpCO_2 changes and ultimately ΔFCO_2."

Comment. L.226-229: That's very interesting! But unfortunately not mentioned again. So actually what you see in this multi-centennial run is that the changes by the end of the century are not a new steady state but the Southern Ocean sink will change further. That would be an interesting point to make for looking at the end of this century in general. Or at least discuss this point later.
Reply: Thanks for point this out. In the revised manuscript we make use of this result to strengthen the discussion.

Comment. L.238: I would phrase this differently: The Revelle factor only measures the effect of DIC changes on pCO2 changes, it is not a driver that amplifies in my view (I'm open to arguments against that view).

Reply: Thanks for the comment. Contrary to the reviewer's argument, the increase of the Revelle factor does amplify the seasonal cycle amplitude, this has been shown in multiple studies referenced in this study (Hauck and Völker, 2015; Fassbender et al., 2022; Kwiatkowski and Orr, 2018)

Comment. L.253: "nearly scale" – If it was scaling, I would expect the driving mechanisms stay constant but increase due to the increase in atmospheric pCO2. Given that almost all drivers in that region change, I'd argue that the factor by which the uptake increases is similar to the increase in atmospheric pCO2 is a coincidence.

Reply: Fair point, thanks for point this out. We rephased this sentence in the revised manuscript.
Line 300 – 302: "Because of this seasonal scale near-balance in thermal and nonthermal contributions in the Sub-Antarctic region, annual mean CO_2 uptake nearly increases with the atmospheric forcing, increasing by 208% by end of the 21st century."

Comment. L.256: "larger" in terms of amplitude, magnitude,…?
Reply: The magnitude, now corrected.
Line 304 – 305: "In the Antarctic region, the magnitudes of the thermal and nonthermal components are also larger at the end of the century compared to the present climate (Fig. 4i)."

Comment. L.263: …And having open water for longer means longer time to warm
Reply: Thanks for point this out, we now included this point.

Line 311 – 312: "In addition, a longer open water seasons, consequently shallower MLDs require less energy to warm SST."

Comment. L.282: Why is a minimum temperature of 8 degrees relevant? Having these temperature values and ranges is not immediately tangible for knowing the implications on solubility

Reply: Thanks for highlight this. The specific temperature here is referring to Fig. S1 and it is meant to highlight that surface ocean temperatures are significantly above freezing temperature north of the Polar front, and near freezing south of the Polar front. This is a key point to our narrative, specifically that the anthropogenic forcing reduces CO_2 solubility due rising temperatures in ice-free regions because ocean warming is restricted by sea-ice and upwelling of cold deep waters, while the Antarctic show the opposite. We have refined this description in the revised manuscript to make it more tangible.

Line 335 – 344: "This is because surface waters in the Antarctic region is relative cool (1 - 5°C) (Fig. S1), and the future surface ocean warming is constrained by the presence of sea-ice and upwelling of the circumpolar deep water from warming significantly above the freezing temperature, even in late summer in projected climate (Fig. 8e). In contrast, sea-ice-free regions (Sub-Antarctic and Subtropical) are already further from freezing temperature (8 °C minimum, Fig. S1), and hence further warming further reduces gas solubility. This is for example illustrated comparing the Subtropics with Antarctic region, projected warming further reduces CO_2 solubility during summer subtropics, weakening the seasonal ΔpCO_2 amplitude (Fig. 8c). Thus, in principle, the Antarctic surface oceans can still take up CO_2 through gas solubility even at the end of the century in the high-emission scenario, more so that atmospheric CO_2 is higher than the present climate."

Comment. L.316: post-2060: That means the changes in CO2 flux drives is not a shift but more a sharp transitions? Like a tipping point? Also, in research comparing projections of different ESMs it is more common nowadays to use "Global Warming Levels" instead of years, because ESMs reach different global mean temperatures at very different times depending on their climate sensitivity.

Reply: Fair point, we rephased to use the word "transition" in this sentence
Further, the reviewer's suggestion to use global warming levels is well-taken and may to essential in analysing the time of the emergence of the carbon cycle variables in addition to temperature. Nevertheless, our study focused on the mechanistic insight of the analysed changes in the position of the Southern Ocean CO_2 sink, temperature is not singularly essential variables that help us understand model difference, but an important key variable nevertheless. We added the ensemble mean of surface warming in the revised Fig. 8e.

Comment. Line 338 – 383: "The role of sea-ice melt is well illustrated by a stronger corelation between sea-ice loss and FCO_2 post-2060 (Fig. 8 a-b) when sea-ice loss surpasses 5-10%. It is at this stage (post-2060) that the domain of the largest CO_2 uptake transitions to the Antarctic region (Fig. 8a). The strong correlation between CO_2 uptake and sea-ice melt post-2060 reinforces the central role of sea-ice in driving the CO_2 dynamics of the Antarctic, and further highlights the importance of improving the representation of sea-ice in ESMs among other biases (Fig. 8 a-b)."

Reply: Corrected.

Comment. L.356: changes in overturning were not mentioned beforehand in the results. Could they be affecting the CO2 flux as well when e.g. assuming the export of carbon decreases in the scenario?
Reply: Thanks for point this point, in the revised discussion section we added a section explaining other factors that may be playing a role including overturning circulation and ice-sheet melt (Line 429 – 439**)**. And yes, it could be playing a role, especially in the Antarctic region.

Comment. S24-26: I don't understand how organic matter cannot leave the winter mixed layer but at the same time be respired below the winter mixed layer.
Reply: Thanks for point this out. This was a typo and is now corrected, we spent to state respiration within not below the mixed layer.

Comment. S30: The table needs more information about the ESMs used, their components, etc. (similar to the comparison of pCO2 products)
Reply: Table S1 have been updated with more information.
Table S1. The list of the nine Earth System models used in this study. For the vertical grid ρ means isopycnic and several symbols means hybrid

No.	**Earth System Model**	**Country**	**Horizontal resolution**	**Vertical resolution**	**Reference**
1.	CanESM5	Canada	1° x 1°	z 45	Swart et al., 2019(Swart et al., 2019)
2.	CMCC-ESM2	Italy	1° x 1°	z 50	Lovato et al., 2022(Lovato et al., 2022)
3.	CESM2-WACCM	USA	1° x 1°	z 60	Danabasoglu et al., 2020(Danabasoglu et al., 2020)
4.	IPSL-CM6A-LR	France	1° x 1°	z 75	Dufresne et al., 2013(Vial et al., 2013)

5.	NorESM2-LM	Norway	1° x 1°	z- ρ 53	Bentsen et al., 2013(Bentsen et al., 2013)
6.	MPI-ESM1-2-LR	Germany	1.5° x 1.5°	40	Mauritsen et al., 2019(Mauritsen et al., 2019)
7.	MPI-ESM1-2-HR	Germany	0.4° x 0.4°	z 40	Müller et al., 2018(Müller et al., 2018)
8.	UKESM1-0-LL	UK	1° x 1°	Z 75	Sellar et al., 2019(Sellar et al., 2019)
9.	AWI-CM1	Germany	0.25° x 0.25°		

Structure

Comment. L. 88-98: You switch between absolute difference, relative difference, uncertainty, and new FCO2. This makes it very hard to follow and get the point

Reply: Thanks for point this out, we corrected this in the revised manuscript.

Line 93 – 103: “At the end of the 21st century (2080 - 2099) a remarkably consistent pattern emerges in the ocean carbon uptake across ESMs; the region of the weakest CO_2 sink in the contemporary climate becomes the most intense sink. Namely, the region of the strongest CO_2 sink shift poleward from the Subtropics to the Antarctic (Fig. 2a-c). By the end of the 21 st century, Subtropics contributes only about 23% (-22.0±2.5 gC m^{-2} yr^{-1}) of total Southern Ocean CO_2 uptake under the high-warming scenario in comparison to 47% in the contemporary climate. The CO2 sink in the Subtropics increase by the smallest margin (6.6±1.1 gC m^{-2} yr^{-1}) in comparison of other Southern Ocean subdomains. The Sub-Antarctic region contributes 29% (-27.7±5.4 gC m^{-2} yr^{-1}) to the projected future, which is comparable to 27% in the present climate. The Antarctic region on the other hand displays the most extensive CO_2 sink increase of about 450% (-37.9±7.3 gC m $^{-2}$ yr^{-1}) becoming the largest CO_2 sink region (48%) at the end of the 21st century (Fig. 1-2).”

Comment. L.205ff: I see you want to tell the reader what to expect in advance which is a great signpost. However, this is more of a conclusion that should be drawn at the end then stated upfront. Maybe you can instead state a hypothesis or the reasoning how you attempt to disentangle the drivers. If that doesn’t work having it phrased as a conclusion is better than not having it at all.

Reply: Thanks for point this out, we removed this sentence in the revised manuscript.

Comment. L212-229: When first reading it I had difficulty understanding the story because it switches back and forth between seasonal changes and annual means. Maybe you can clarify

Reply: We modified this paragraph and split into two paragraphs as explained above,

Figures

- Almost all figures would profit from adding grid lines so that one can read off the values

Reply: We strength the grid lines all the figures, expect for Fig. 7.

- F1: Inconsistent titles (ensemble vs ensemble mean). Inconsistent description (variability vs variability obtained from the one stdev)

Reply: To be corrected

- F2 goes from North (left) to South (right), F3 the other way around w

Reply: All the figures goes from North (left) to South (right) except for Fig. 3. This order follows the description order in the text. The exception made for Fig. 3 is because it shows latitudinal averages which is different from other figures. Figure 3 shows all three domains in each panel, and we decided to keep the Antarctic end on the left to keep up the convectional depiction of such figures, I thought revising the order will make it unintuitive.

- F2: description incorrect for which bar/colour is which

Reply: We rechecked this text to make sure it is correct.

- F3: shows pCO2 difference in a different way to FCO2, i.e. the figures are not comparable, but is used in the text to support that dpCO2 is the key determinant of FCOs

Reply: In line 108 – 125 of the main text, we made a deliberate effort to justify the transition from FCO_2 to ΔpCO_2 and surface pCO_2. Since this study is not focused on budget attribution, but on the mechanistic understanding of the drivers of the analysed FCO_2 changes, we choose to focus on the thermodynamic driver of FCO_2 which is the primary driver: ΔpCO_2. This simplification allows us to explain the processes regulating FCO_2 seasonality. In this context, we argue that ΔpCO_2 and FCO_2 are comparable; similar seasonal phasing.

Line 108 – 125: "Air-sea CO_2 fluxes (FCO_2) are regulated by thermodynamic and kinematic forcings19 . The thermodynamic forcing, the air-sea pCO_2 gradient (ΔpCO_2), is considered the primary driver of FCO_2 ; it determines the direction of the flux [20] . The kinematic forcing, on the other hand, controls the efficiency of gas transfer, and it is principally regulated by near-surface wind speeds. We note that kinematic forcing can induce indirect effects on the surface pCO_2 , e.g., through changing the ocean circulation or water mass ventilation patterns[21] . On short timescales (hourly to weekly), kinematic forcing can also determine the magnitude and direction of FCO2 [22–25] . However, ΔpCO_2 plays a leading role in seasonal-scale FCO_2 variability [26,27] . Therefore, mechanisms regulating

FCO_2 variability can be estimated from processes regulating ΔpCO_2 . Further, considering that atmospheric pCO_2 is almost uniform in the Southern Ocean, ΔpCO_2 is ultimately controlled by the ocean pCO_2. Indeed, observed and ESMs ΔpCO_2 properties broadly delineate a similar latitudinal structure with FCO_2 (Fig. 2 & 3), showing a strong annual mean CO_2 ingassing flux in the Subtropics consistent with negative ΔpCO_2 (~ -40 µatm) and decreasing poleward; > -30 µatm in the Sub-Antarctic, and near-zero value in the Antarctic region (Fig. 3a-c). Given that ESMs mean state magnitudes differ for some variables (e.g. pCO_2 and dissolved inorganic carbon {DIC}among others), comparing a multimodel seasonality is often impractical. Henceforth, we instead use monthly rates of change (first-order temporal derivatives, see Methods) for selected variables to highlight the changes in model and observed features at the seasonal scale."

- F4: have you tested if a person with a visual impairment (e.g. red-green) can differentiate the lines?
- F5: a and b what is the front? C and d: Why is one Antarctic and the other South of 50S? d: why no observations. In description: typo in last line (modal), and mention that black line is observations is missing.

Reply: This figure has removed in the revised manuscript.

- F6: Order of the subplots is different to order in the text. Why are some the absolute values and others the derivatives? Shading showing the uncertainty is not overlapping/see-through as in Fig 4 – adjust so that one can see the actual range. Typo in description (modal)

Reply: We provided the justification for using the derivatives for some variables in lines 121 – 125, derivative are used for variables that models generally show different mean states and their seasonal cycle is not comparable on the same scale.

We corrected the modal typo, thanks for pointing this out.

Line 121 – 125: "Given that ESMs mean state magnitudes differ for some variables (e.g. pCO 2 and dissolved inorganic carbon {DIC}among others), comparing a multimodel seasonality is often impractical. Henceforth, we instead use monthly rates of change (first-order temporal derivatives, see Methods) for selected variables to highlight the changes in the model and observed features at the seasonal scale."

- F7f: that relation is not significant

Reply: Thanks for pointing this out, we made an effort to make mention of the non-significance of the regression in the text.

Line 212 – 221: "In addition to the NPP links to surface respiration, we also find that NPP plays a key role in setting the subsurface vertical DIC gradients which in turn influences seasonal DIC entrainment and hence vertical DIC exchange rates (Fig. 7 a-f). ESMs with high NPP tend to have stronger vertical DIC gradients (Fig. 7 e-f, Fig. S4 d-f & S5 a-b). Using all nine ESMs, we find that subsurface (dDIC/dz)max and seasonal NPP max has a robust relationship in the Sub-Antarctic ($p < 0.01$) but a non-significant regression in the Antarctic region (Fig. 7 e-f). Nevertheless, given the relatively large model spread shown by nearly all nonthermal and physical processes in the Antarctic region, it might be that this relationship is robust with more models. The role of NPP in setting DIC entrainment rates in the Southern Ocean has also been shown in previous studies [35,36] , this relationship is re-iterated here to establish mechanistic links for the drivers of the nonthermal pCO 2component."

- F7: description: repetition of "vertical", typo (modal)

Reply: Typo corrected, thanks for pointing this out.

- F8: Label of colorbar missing in a, b: why show both and not just one region? Typo in description (repetition)

Reply: This figure has been modified to correct this, we also added more panels to make its interpretation more intuitive.

- Figures in Supplementary: Some of them are not reference anywhere, S6 = S9, several figures descriptions/labels/titles need correction of typos

Reply: The supplementary material was revised to this issues

Writing and wording

You sometimes write long sentences with verbs at the far end. For a better understanding consider rephrasing so that it is clear early in a sentence where the journey is going. e.g. l.208-2010 "is described below" can go further up to let the reader know when this sentence is about as early as possible.

Inconsistencies in writing

- Observation-based pCO2-products (l. 69), pCO2-products ensemble (l.73), observationally-derived pCO2-products (l. 82), pCO2-products (l.85), observed dpCO2 seasonality (l.133, pCO2 products are not observations)
- CO2 flux (e.g. l. 69) or CO2 sink rate (l.90) or FCO2 (l.52) or CO2 ingassing (l.112)
- Shift (most of the text) or migration (l.194)
- Write dpCO2T/dt (l.235) or (dpCO2/dt)T elsewhere
- L.276-279: You use dpCO2 to mean "air-sea difference" as well as "change between projection and contemporary"

You sometimes put references to Figures at places that the Figure doesn't refer to, e.g. l.244: the figure shows changes in SSS and SST, not how these changes affect stratification

Reply: Thanks for the attention to detail, we made effort to improve the readability of the manuscript in the revised text.

L. 70: Minor quibble but I find the phrasing “within the front” a bit odd, call it along the front? L.126-127: “oppose each other on a seasonal scale” could be clarified, maybe call it timing or phase of their variability?

L.141: “This is…” – I assume you mean that the Subtropics are dominated by the thermal component. The way it is written, “This” here means the summer/winter difference in solubility is due to the **range** in temperature changes, which it isn’t.
Reply: Corrected
Line 150 – 152: “The strong thermal dominance in Subtropics is partly because the Southern Ocean has the largest seasonal temperature contrast (summer-winter difference) in the northern edge, decreasing poleward (Fig. S2b).”

L.161: “this feature”, what is this referring to?
Reply: Rephrased.
Line 172 – 175: “This keeps dSST/dt relatively low (Fig. 5j & S1), and hence the observed (dpCO_2 /dt) T seasonal amplitude is lower than in the Subtropics and Sub-Antarctic region (Fig. 4f). While some ESMs shows this feature (e.g. CanESM5 and UKESM1-0LL, Fig. S3), not all ESMs’ are consistent with the observed estimate, other models show a larger than observed dSST/dt (Fig. 5j).”

L.196ff: You may want to phrase information about projections this less definite – i.e. “under this scenario” rather than “in the future”, as it is only one scenario you are considering and whether that becomes true is not definite
Reply: We addressed this issue by using “projected future” ins of “in the future” throughout the text.

L.197: “decreases poleward” refers to the subject of the sentence which is “Southern Ocean”, but you mean the warming signal decreases poleward
Reply: Corrected.
Line 239 – 240: “At the end of the 21st century, the ocean is warmer; the Southern Ocean warms the most in the Subtropics (> 3^{o}C), and the warming signal decreases poleward, reaching a maximum of 1^{o}C in the Antarctic by the end of the 21st century (Fig. 8d) .”

L.212: Unclear if the dpCO2 decreases or the change in dpCO2 decreases
Reply: Rephased. Line 255 – 256:

“By the end of the 21st century, ΔpCO_2 indicates extensive changes in the Antarctic and decreases equatorward consistent with ΔFCO_2 (Fig. 2 & 3).”

L.236: I know that you mean the effect of the mean pCO2 on the amplitude of pCO2 changes given a certain change in temperature, but only because I am familiar with the subject. For a broader audience this needs explaining.

Reply: Description added,

Line 279 – 283: “The thermal and nonthermal components increase by nearly equivalent amounts by the end of the 21st century (Fig. 4h). Since seasonal warming and cooling rates show little to no change in the future climate (Fig. 5i), the increase in $dpCO_{2T}/dt$ is primarily due to the ocean pCO_2 increase (Eq. 1 in Methods). pCO_2 have a higher sensitivity to temperatures in a high pCO_2 environment [12] .”

L.240: Instead of “while” do you mean “despite the amplitude decreases”?

Reply: We rephased the sentence to clarify the meaning.

Line: 287 – 289: “Because of the Revelle factor effect, although dDIC/dt shows a small decline in the projected future (Fig. 6 e-f), its impact on the nonthermal pCO_2 contribution is larger than the present climate.”

L.248: “tracer” – you only talk about DIC, not tracers in general, and the DIC gradient does not affect the exchange of other tracers

Reply: We rephased the sentence to specifically refer to DIC.

Line 296 – 297: “The combination of these two factors reduces surface-subsurface DIC exchange, and hence the entrainment of subsurface DIC is weaker in the projected future climate.”

L.262: “of the MLD [the] increases” – remove “the”

Reply: Corrected

L.270: Lengthy first sentence that takes away the focus from the main message that is that the primary driver changes.

Replay: Thanks for comment, we however thought this sentence is essential in setting the context what follows, where we expand and go into a detail description of the mechanisms.

L.275: “ESMs” replace with “ocean” or “Southern Ocean”

Reply: Thanks for the comment, we thought making this change would change the meaning of the sentence. We wanted to make the point that this is how the current generation of ESMs behaves, this may not be the case in the future ocean.

L.278 and 281: Just omit “ice-free region” and write Sub-Antarctic and Subtropics to stay consistent
Reply: Corrected

L.294-298: You switch from seasonal variability to annual means to uptake rates – can you phrase this in a consistent way
Reply: Thanks for this comment. This phrasing is consistent with how the analysis is framed, providing an essential summary of the results just before going into the discussion, we therefore could not modify without losing the meaning.

L.305: Just write “biological properties”
L.307: “biological CO2 uptake” can be misunderstood as forming biomass/photosynthesis, but you are referring to biologically-driven air-sea CO2 flux.
Reply: In this context assuming either will have the same meaning we intended; by biological CO_2 uptake here implies CO_2 uptake due to increased air-sea gradient simulated by the transformation of surface DIC to particulate carbon through photosynthesis.

L.313-314: “which on one hand shoals and decreases the differences in MLD seasonal characteristics among ESMs” – this groups together separate topics i.e. the ensemble-mean MLD and the difference between ESMs, also where is “the other hand”.
Reply: The one hand here is meant to state a realization of the impact of sea and the rest of the paragraph describes the rest of the impacts.

L.340: Polar oceans probably won’t become similar to the Subtropics in absolute terms. But the dominating drivers of CO2 flux may be.

L.481: bar missing in last term
Reply: To be corrected.

L.496-298: How does this comparison between Orsi and Fay& McKinley biomes relate to your work?
Reply: In recent years Fay& McKinley biomes have been commonly used in the family of studies similar to ours, we therefore put this comment in anticipation of questions on our choice of boundary definition.

Reviewer 2

Major comment: The paper by Mongwe and colleagues uses Earth System Models (ESM) to examines mechanisms of CO2 uptake in the Southern Ocean (SO) and the influence of climate scenarios on patterns of uptake. They ground their analysis in observations, using multiple gridded products derived from observations as points of comparison to ESM estimates in the present, and a baseline for comparison of future scenarios. I am reviewing this paper from the point of view of an observational biological oceanographer with experience in biogeochemistry and a functional understanding of Earth System Models.

The paper builds on previous studies that noted polewards shifts in the CO2 sink rate by providing a detailed account of mechanisms driving this shift. It significantly contributes to the state of the art by identifying overarching mechanisms and linking them to specific oceanographic changes, with shifts in sea ice cover and impacts on mixed layer depth, heat absorption, and pCO2 uptake a dominant driver in the Antarctic. The paper acknowledges shortcomings of ESMs while nevertheless presenting patterns that are reasonable with respect to observations, presenting hypotheses as to the mechanisms driving shifts in future pCO2 that could be tested with present and future networks of observations (e.g. bio-argo as part of SOCCOM). I appreciate this balance, and attempts by the author to link model observations to paleoclimate studies. As a whole, the paper presents a nice narrative. While I remain curious as to the impact of the ESM biases on the conclusions of the paper (i.e., impact of stratification bias on the conclusion regarding pCO2 uptake), the paper presents a reasonable approach in integrating results from multiple ESMs, which constitutes an appropriate statistical approach. The data is presented in a consistent manner, which allows for easy interpretation even for someone not familiar with the detail of ESMs.

I would appreciate if the paper contextualized these changes along with other expected changes in the Antarctic, specifically changes in the ice sheet which will impact mixed layer depths and other processes discussed in the paper. Ice sheet dynamics are currently not coupled to ESMs, but a short review of possible implication for the mechanisms discussed here would be helpful. There have also been significant efforts to expand the observational networks for the Southern Ocean, with for example Gray et al. (2018) providing a SO-wide estimate of CO2 flux. I suggest adding a comparison to observation only datasets such as Gray et al. 2018 (i.e., non-spatially interpolated), perhaps in a supplement, as observations, while still sparse, are nevertheless the most direct data source for relevant parameters discussed in this paper.

The paper is well written, logically organized, interesting from both a modeling and observational point of view, and presents findings that are likely to guide future research. With minor edits, the paper is ready for publication.

Reply: We thank the reviewer for this helpful feedback and kind words. I particularly appreciated the suggestion to make links with the ice-sheet melting. The revised manuscript is grounded in a comparison of the ESMs with in situ observations and previous studies. Comparisons with observed estimates for key variables are made throughout the manuscript. For the FCO_2, pCO_2, and ΔpCO_2 specifically, we chose to use the lasted six machine-learning-based data products which comprise nearly all variable CO_2 measurements from SOCAT at the time of writing. We did not include Argo floats separately for two reasons. Firstly, While Argo floats are a significant advancement in ocean CO_2 measurements, particularly for winter measurements. Their winter estimate of FCO_2 has also been challenged as potentially overestimation the winter Southern Ocean CO_2 source e.g. (Long et al., 2021). Further, studies that have compared the inclusion of Argo floats (e.g. Bushinsky et al., 2019) on data products in addition to the SOCAT data have shown that Argo floats data enhances the CO_2 outgassing winter FCO_2 but does not change the phasing of the seasonal cycle of pCO_2 and FCO_2. Thus, for our study, it was not clear that analysing Argo float separately added a stronger constraint to observed estimates. Moreso that our study primarily focuses on the ESMs simulation of future change, the present climate comparisons are only used to establish a foundation of model comparison with observed estimates. Having that said, the editor's point is well taken, Argo floats are a key addition to the Southern Ocean CO_2 measurements and will be included in future studies.

The potential impacts of the ice-sheet melt are added to the discussion

Line 429 - 439: "The long-term perspective of this carbon sink may depend on the circulation changes that transfer carbon absorbed from the atmosphere to the water masses in the intermediate and deep-water reservoirs. On other hand, anthropogenic ice sheet melt in Antarctica is projected to slow down the Southern Ocean overturning and enhance surface stratification [71] which may weaken this northward DIC advection in the future. Ice sheet melt is also projected to enhance Antarctic sea-ice and slow-down warming through the albedo feedback [71, 72]. Stronger stratification may continue to constrain winter DIC surface-subsurface mixing and allowing the surface ocean to take up CO_2 through solubility in winter although sea-ice is abundant, but this CO_2 sink may eventually be weakened by poor overturning. It remains unclear how these processes will work together; the inclusion interactive ice sheet in the next generation of ESMs will be key to understanding this mechanism."

Minor comments:

L 171 - wrong fig? Should be fig 6 a, b if NPP

Reply: Corrected

L 171 - what is your threshold for start and end? It would be helpful for these and other phenology metrics to specify how you derived the timescales you are discussing, and include on the figures vertical bars to guide the eye of the reader. I found it hard to distinguish the month offset discussed in the paper

Reply: Thanks for comments, we enhanced the vertical lines in Fig. 6, and mentioned specific months where timing is necessary.

L 176 - no fig 5 h - should be 6 h

Reply: To be corrected

Fig 5 - Are observations missing in panel d.?

Reply: This figure has been removed.

Fig 6 - add vertical bars for seasonality metrics (see comment above)

Reply: Vertical lines added.

Fig 8 - edit labels, unclear as is. Add Year, replicate FCO2 to assist in comprehension

Reply: Figure 8 modified and corrected.

Reviewer 3

The study addresses a fundamental question of how the Southern Ocean carbon sink will alter in the future. This region is highly complex in the present day involving ocean uptake in the subtropics north of the subtropical front and weak outgassing south of the polar front. There is a comparison with observational pCO2 products based the Surface Ocean CO2 atlas. There was though no mention of the Bio-Argo float based estimates that reveal stronger outgassing south of the polar front.

The study diagnoses the output of a set of Earth system models to document how the Southern Ocean carbon sink alters in the future, both in terms of latitudinal contrast in response and the effect of the controlling processes. This analysis is insightful and challenges an often-quoted viewpoint that the biological drawdown is likely to increase. Instead the primary response involves a seasonal, solubility-driven change involving the melting of sea ice leading to a shallowing of the mixed layer, decreasing the entrainment of carbon-rich deep waters and so leading to an uptake of CO2 south of the polar front.

I like this study and have found it to be insightful. I only have two comments designed to strengthen the work:

Comment: The mechanistic insight is provided by comparing an estimate of the temperature-driven tendency in pCO2 and the non-temperature-driven tendency in pCO2 involving the sum of mixing and biological effects. The relative magnitude of each of those contributions to the tendency in pCO2 are then illustrated and discussed. While that analysis is insightful, I did feel that combining together the mixing and biological effects missed a critical part of the story. My understanding of the outgassing south of the polar front is based upon the large pool of regenerated carbon in that region that is entrained into the winter mixed layer and then leads to the winter outgassing. This viewpoint was first set out by Ito and Follows (2005) JMR advocating the importance of preformed versus regenerated phosphate to understand the ocean sequestration of CO2. Taking that viewpoint further, Lauderdale et al. (2013) Climate Dyn. sets out how the different carbon pools are controlled and Lauderdale et al. (2016) GBC [cited] sets out the different drivers for the air-sea CO2 flux. The manuscript provides much additional information, but is missing any separation of nutrients or dissolved inorganic carbon into preformed or regenerated components. The study already shows the AOU, so it might be relatively easy step to show this split for nutrients and/or dissolved inorganic carbon and then gain some insight into how the supply of regenerated nutrients and carbon to the winter mixed layer has dramatically declined south of the polar front.

Reply: We thank the reviewer for this insightful suggestion and kind words. In the revised version of the manuscript, we included the seasonal cycle of preformed and regenerated DIC based on Ito and Follows. 2005 decomposition as suggested. This was only done for models with all required variables. The decomposition of DIC into preformed and regenerated DIC together with AOU and mixed layer depth enabled us to isolate the biological (nutrients) contribution from the physical (mixing) on the seasonal cycle of the nonthermal pCO_2 in the Southern Ocean (New Fig. 5). This addition has indeed helped bring much clarity to our analysis.

Comment: There is a plausible speculation that the new solubility feature is only active during relatively cool sea surface temperatures (L338). Can the authors go further and document any criteria for this response to hold that links to for example, the shallowing of the winter mixed layer, while retaining the presence of sea ice. I think adding criteria would be very useful that separate the emergence of the same sign in air-sea CO2 flux (the new polar response) versus the opposing signs in the air-sea CO2 flux linked to the seasonal pCO2 evolution (subtropical regime).

In summary, this study provide new mechanistic insight into how the Southern Ocean carbon uptake may vary in the future. Providing more detail of how the preformed and regenerated nutrient and carbon contributions compare may consolidate that insight given the processes acting south of the polar front. Adding more detail as the criteria for this new air-sea flux response to hold would be helpful.

Reply: We thank the reviewer for this comment, it helped us clarify this section of the manuscript. To address this comment, we modified Fig. 8 to include long-term changes in the seasonal amplitude of ΔpCO_2 and temperature as well as surface warming. These variables together with the sea-ice fraction have given a clearer description for the emergence same sign change in ΔpCO_2, and its implication to FCO_2. It helped provide a clearer distinction in the impact of warming for Subtropics vs. the Antarctic.

Detailed points:

Comment: L85-86. There was though no mention of the Bio-Argo float based estimates that reveal stronger outgassing south of the polar front.

Reply: The revised manuscript is grounded in a comparison of the ESMs with in situ observations and previous studies. Comparisons with observed estimates for key variables are made throughout the manuscript. For the FCO_2, pCO_2, and ΔpCO_2 specifically, we chose to use the lasted six machine-learning-based data products which comprise nearly all variable CO_2 measurements from SOCAT at the time of writing. We did not include Argo floats separately for two reasons. Firstly, While Argo floats are a significant advancement in ocean CO_2 measurements, particularly for winter measurements. Their winter estimate of FCO_2 has also been challenged as potentially overestimation the winter Southern Ocean CO_2 source e.g. (Long et al., 2021). Further, studies that have compared the inclusion of Argo floats (e.g. Bushinsky et al., 2019) on data products in addition to the SOCAT data have shown that Argo floats data enhances the CO_2 outgassing winter FCO_2 but does not change the phasing of the seasonal cycle of pCO_2 and FCO_2. Thus, for our study, it was not clear that analysing Argo float separately added a stronger constraint to observed estimates. Moreso that our study primarily focuses on the ESMs simulation of future change, the present climate comparisons are only used to establish a foundation of model comparison with observed estimates. Having that said, the editor's point is well taken, Argo floats are a key addition to the Southern Ocean CO_2 measurements and will be included in future studies.

L97. FCO2 not yet defined.

Reply: FCO_2 defined in line 108

L186-190. I think that point being made here is important, but as above recommend illustrating that response further.

Reply: This section was modified consistent with above recommended changes.

L187. Should not really say that the subsurface DIC gradient is a key driver of entrainment in terms of causality. What I think you mean is that the entrainment flux of carbon varies in magnitude with the subsurface DIC gradient.

Reply: Corrected,

Line 212 – 214: "In addition to the NPP links to surface respiration, we also find that NPP plays a key role in setting the subsurface vertical DIC gradients which in turn influences seasonal DIC entrainment and hence vertical DIC exchange rates (Fig. 7 a-f)."

L262. Slip in the sentence construction.

Reply: Corrected, Line 310 – 312: "The melting of sea-ice (Fig. 8b) and shallowing of the MLD (Fig. 6d) increases the summer-winter surface temperature contrast (Fig. 8d). In addition, a longer open water seasons, consequently shallower MLDs require less energy to warm SST."

L306 Better to qualify what "this" refers to.

L479 to 484 for equations (1) to (4). I recommend improving the explanation of these relationships. In the cited Mongwe et al. (2018) study there is a more complete explanation, where equation (2) is cited first with the coefficient based on an empirical fit to carbonate-chemistry coefficients. In addition, the non-thermal term in (3) is split up into different contributions.

Reply: Thanks for point this out, we have now provided a complete description of methodological approach.

Line 554 – 572: "Surface ocean pCO_2 and ΔpCO_2 variability is controlled by the relative contribution of thermal and nonthermal components 28,29 . We here estimate the thermal component using the Takahashi et al. (1993) formulation[30] (Eq. 1-2) and we estimate the nonthermal component by subtracting the thermal component from the total (Eq. 3) . Thermal component is driven by temperature variations through changes in gas solubility (Henry's law). The nonthermal pCO_2 component on other hand is mainly controlled by mixing and biology31 , which also includes the role of total alkalinity and salinity changes. However, total alkalinity and salinity have been shown to play a minor role in the seasonal cycle of ocean pCO_2 in the contemporary Southern Ocean 29,31 , thus, here we focus on processes responsible for sources and sinks of DIC, i.e., primary production, respiration, and seasonal buoyancy change-driven mixing. The thermal and nonthermal components of pCO_2 oppose each other on a seasonal scale [32,33] (Fig. 4 d-f), and hence the larger of the two determines the observed seasonal cycle phasing of pCO_2 , and ultimately FCO_2 [29]. The relative contributions of thermal and nonthermal components are assessed here through the absolute difference in their monthly rates ($M_{T\text{-}nonT}$, Eq. 4). The larger rate of change between $|(dpCO_2 /dt)T|$ and $|(dpCO_2 /dt)_{nonT}|$ is therefore considered the dominant driver of ocean surface pCO2 change (monthly in our case). This is estimated by the absolute difference of the time derivative of the thermal and nonthermal components, and we term this diagnostic metric as $M_{T\text{-}nonT}$ (Eq. 3), consistent with Mongwe et al. (2018). $M_{T\text{-}nonT} > 0$ indicates periods when temperature variance drives the pCO_2 , while $M_{T\text{-}nonT} < 0$ is indicative of periods when nonthermal processes play a leading role in surface pCO_2 . While simple, $M_{T\text{-}nonT}$ provides a useful diagnostic for identifying the predominant mechanisms driving seasonal pCO_2 variations, in particular, given that the thermal and nonthermal pCO_2 oppose each other on a seasonal scale, thus isolating the leading driver provides key information.

$$pCO_{2T} = 0.0423 \times \overline{pCO_2} \times SST \quad (1)$$

$$\left(\frac{\partial pCO_2}{\partial t}\right)_T = 0.0423 \times \overline{pCO_2} \times \left(\frac{\partial SST}{\partial t}\right) \quad (2)$$

$$\left(\frac{\partial pCO_2}{\partial t}\right)_{nonT} = \left(\frac{\partial pCO_2}{\partial t}\right)_{Tot} - \left(\frac{\partial pCO_2}{\partial t}\right)_T \quad (3)$$

$$M_{T-nonT} = \left|\left(\frac{\partial pCO_2}{\partial t}\right)_T\right| - \left|\left(\frac{\partial pCO_2}{\partial t}\right)_{nonT}\right| \quad (4)”$$

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19th Jan 24

Dear Dr Mongwe,

Your manuscript titled "Poleward migration of the dominant CO2 sink region in the Southern Ocean under high emission-scenario" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Review: Poleward migration of the dominant CO2 sink region in the Southern Ocean under high emission-scenario

Thank you for the edited manuscript. It has greatly improved in accuracy and clarity from the first draft. From my point of view, the manuscript will benefit from some restructuring to further clarify the key points and to have a thread through the text. With the plethora of processes you investigate, it is a challenge to describe their role for the bigger picture.
Most of the comments address clarity and accuracy but not the bigger science questions. For future work, I would suggest going with recent progress when analysing CMIP data, for example, using more than one scenario and/or a more realistic one than SSP5-8.5, or using global warming levels instead of time periods to intercompare models with very different climate responses.
Thanks.

Comments about content and clarity:
Your introduction and Results 2.1.1 are clear to read now! My comments focus on Results 2.2, which covers a lot of processes and complexity. I summarised a few locations where I think the narrative

thread is lost, which prevents me from understanding the content. In general, I think having shorter paragraphs with digestible information would greatly help the understanding the complexity. When explaining changes in one sub-region, you sometimes refer to other sub-regions which can interrupt the reading flow (e.g. L316). Maybe consider explaining regional changes individually and then summarising them at the end of a Section to highlight the key take-aways.

L200 until the end of the paragraph: That feels like a detour, I do not understand what the motivation for this is. Maybe add a few sentences for context.
Para starting L212 Sorry I am totally lost and I cannot give ideas of what may help to clarify. Things I find confusing: I think I lost the storyline in the paragraph before, so I don't understand what the aim is/the hypothesis. At the moment I do not find the references to the Sub-Antarctic particularly useful, although I agree this can be interesting to contrast the Antarctic. Mentioning Oxidation in the last sentence adds to my confusion.
L239 highlight this is just for the case of this one high-end scenario upfront, it is "projected" under these assumptions. This information is a bit hidden
L245 I suggest moving this context about the Revelle factor to L283 where it is applied, so that the dots are connected.
L252: I would remove the note about the expected scaling with atmospheric CO2 because you don't show what is expected. It may very well be that one region takes up more than expected and another less. However, your results do not distinguish that (and don't need to)
L265ff. I may misunderstand the point, are you saying in winter the CO2 uptake increases because of higher atmospheric CO2 but in summer it remains similar because the effect of solubility and atmospheric CO2 compensate. That would not lead to an approx. zero change in the annual mean though?
L269 It may again be my understanding of the phrasing, why would warming decrease the amplitude? Under climate change, i.e. higher temperatures and higher pCO2 concentrations, the seasonal variability of the pCO2 should be more sensitive to temperature changes, i.e. an increase in the seasonal amplitude (Takahashi et al. 1993, Landschuetzer et al. 2018, and model studies…)
Paragraph L304: could be structured in a clearer way. The sentences jump between topics, e.g. L309 about temperature, L310 about sea ice and MLD, L312 about temperature, L314 about sea ice – for the reading flow it would be nicer to have individual arguments explained in full
L338 reads as if these regions would warm more because they are already warmer. I think what you mean is they warm more because there are additional processes in the Antarctic that limit warming
L343: I don't understand what you mean with the phrase starting "more so that…"
L374 I believe it is more accurate to say that sea ice is responsible for increasing the regional CO2 sink. That, together with the local changes in the Subtropics (i.e. not increasing the CO2 sink as strongly which is not linked to sea ice), leads to the total shift in contribution/magnitude to the CO2 flux.
L387 If I understand this correctly this is about the "net" CO2 uptake, not the variability/ dominant driver of seasonal variability.
L410 I can't follow you how the solubility should undermine the role of respiration. Do you mean the effect of meting sea ice on the stratification which also affects the net effect of solubility?
L413 Suggest to rephrase "they may become similar" to something like the drivers of CO2 flux may become similar

Other comments:
The paper needs spell-checking throughout and you jump between tenses e.g. L375.
I prefer “climate change” over “climate warming” because it is not just warming.
L45, L53, and many more: just call it “projected changes” instead of “future” or “predicted” or “projected future

L54 regime shifts in “projections” or “scenarios”
L90 FCO2 -> flux (acronym is only introduced later)
L90 pCO2-product values given with two digits, the others with one
L174 repetitive clause: “not all ESMs” and “other models”
L200 What is the “nearly symmetric feature”
L255 It “changes” in the Antarctic – but how? – and the change (?) “decreases” relative to? – can you either rephrase or just delete the sentence please, because the following sentence gives the same information.
L255 Does the delta refer to the air-sea difference? If so, why is it delta-FCO2? Or the difference between present-day and projection? If so, I would rephrase because delta-pCO2 has the connotation of air-sea difference rather than changes over time
L265 “. ESM” instead of comma

Reviewer #2 (Remarks to the Author):

The paper by Mongwe and colleagues, a resubmission in which they've considered, addressed, and integrated comments by 3 reviewers, uses Earth System Models (ESM) to examines mechanisms of CO2 uptake in the Southern Ocean (SO) and the influence of climate scenarios on patterns of uptake. They ground their analysis in observations, using multiple gridded products derived from observations as points of comparison to ESM estimates in the present, and a baseline for comparison of future scenarios.

The authors have significantly modified the manuscript to address reviewer comments, adding clarifying details, context, and results. Additional text and figures have strengthened the manuscript. The reviewers have addressed my prior minor comments, alongside extensive feedback by author reviewers.

You'll find below additional minor feedback for consideration. The paper is an important contribution to the field and merits publication.

Minor comments:
L 194: Should be Fig 6 h
L 290-292: This section is very useful to introduce, but consider revising sentence structure here and in following portions (L 293-295, L 297-300). As written there are sentence fragments which impede lecture (at least from a US reader's perspective).
L 353-357: I would restructure to highlight your main finding in the first 1-2 sentences before going into uncertainty (which you discuss in L 363 onwards). I suggest some variation of: "Examining multi-ESM projections of physical and biogochemical processes, a robust pattern emerged whereby anthropogenic impacts manifested themselves through altering the seasonal carbon cycle with a large-scale shift in the carbon uptake from the Subtropics to the Antarctic region. This major finding was robust even when accounting for uncertainties in multi-model projections"

L362: I would separate uncertainty discussion into a separate paragraph, leaving the first paragraph to highlight the novelty of the result and how it stands in context of previous findings.
L428: advect instead of advent
L438: the inclusion of ice sheets in... (awkward as written)

Reviewer #3 (Remarks to the Author):

The study addresses a fundamental question of how the Southern Ocean carbon sink will alter in the future. This region is highly complex in the present day involving ocean uptake in the subtropics north of the subtropical front and weak outgassing south of the polar front.

The study diagnoses the output of a set of Earth system models to document how the Southern Ocean carbon sink alters in the future, both in terms of latitudinal contrast in response and the effect of the controlling processes. The primary response involves a seasonal, solubility-driven change involving the melting of sea ice leading to a shallowing of the mixed layer, decreasing the entrainment of carbon-rich deep waters and so leading to an uptake of CO_2 south of the polar front.

My concerns were:
1. Combining together the mixing and biological effects missed a critical part of the story. The authors have addressed this concern and added an analysis of preformed and regenerated DIC.
2. There is a plausible speculation that the new solubility feature is only active during relatively cool sea surface temperatures. Can the authors go further and document any criteria for this response to hold that links to for example, the shallowing of the winter mixed layer, while retaining the presence of sea ice. The authors have modified a figure to address this concern. A minor point is that the text could have been improved here, L334 to L350. Many of the sentences start with “This is…”, which is sometimes unclear to the reader what is referred to. Much better to be explicit.

As a general comment, I think that the mechanistic insight is very useful and the diagnostics are very comprehensive, although the text could have been more concise and clearer.

In summary, this study provide new mechanistic insight into how the Southern Ocean carbon uptake may vary in the future. I support acceptance.

Review report.

Poleward migration of the dominant CO_2 sink region in the Southern Ocean under high emission-scenario

Dear Reviewers

Thank you for your feedback; your feedback and comments were instrumental in improving the quality of the manuscript. The following is the full rebuttal report.

Reviewer # 1

Comment: Your introduction and Results 2.1.1 are clear to read now! My comments focus on Results 2.2, which covers a lot of processes and complexity. I summarised a few locations where I think the narrative thread is lost, which prevents me from understanding the content. In general, I think having shorter paragraphs with digestible information would greatly help the understanding the complexity. When explaining changes in one sub-region, you sometimes refer to other sub-regions which can interrupt the reading flow (e.g. L316). Maybe consider explaining regional changes individually and then summarising them at the end of a Section to highlight the key take-aways.

Reply: We thank the reviewer for making this suggestion. We applied this suggestion in the reversed manuscript, each domain is now described in each paragraph (s), and we provide at the end. References to other domains are only made where they are mechanistically relevant.

Comment: L200 until the end of the paragraph: That feels like a detour, I do not understand what the motivation for this is. Maybe add a few sentences for context.

Reply: We this paragraph modified, and removed the most of the text from line 200 into separate paragraph. The revised text in *Line 187 – 201.*

"The seasonal presence of sea-ice also regulates biological and physical-driven variations of upper ocean DIC. The NPP seasonal cycle timing is linked to sea-ice variation in the Antarctic[46]; NPP increase initializes only after the sea-ice maximum (September), this leads to one to two months offset in comparison to the ice-free Sub-Antarctic (Fig. 5b,d). The NPP-related surface DIC consumption is evident in the negative DIC rate of change (Fig. 5f) during the high production season, and it coincides with the $(dpCO_2/dt)_{nonT}$ minima (Fig. 4f). Further, the timing of the NPP seasonal maxima also aligns with a minimum in the apparent oxygen utilization rate (Fig. 5 h). Negative AOU rate of change reflects oxygen production during photosynthesis, whereas positive AOU rates of change is indicative of respiration or the oxidation of organic matter back to DIC in the near surface. Indeed, ESMs show positive AOU rate of change after the NPP maxima indicating subsequent remineralization. The alignment of AOU and DIC rates of change highlights that the role of biology in setting the DIC levels (Fig. 5 a-b & e-h). Further, the decomposition of DIC into preformed and regeneration components (see methods for description, Eq. 5-10) further substantiate that AOU is indeed a reliable indicator of biological-driven DIC variations. Namely, $dDIC_{regenerated}/dt$, and dAOU/dt display a similar seasonal cycle phasing, and both follows the NPP seasonality (Fig. 6)."

Comment: Para starting L212 Sorry I am totally lost and I cannot give ideas of what may help to clarify. Things I find confusing: I think I lost the storyline in the paragraph before, so I don't understand what the aim is/the hypothesis. At the moment I do not find the references to the Sub-Antarctic particularly useful, although I agree this can be interesting to contrast the Antarctic. Mentioning Oxidation in the last sentence adds to my confusion.

Reply: We thank the reviewer for highlighting the unreadability of this paragraph. We clarified this paragraph first by defining on terms used much earlier in the text and break up into two paragraphs, the revised text shown in *Line 203 – 223.*

"In addition to the NPP links to biological DIC variation in the upper ocean, NPP plays an essential role in regulating the physical DIC component[35,36]. According to the models, NPP levels sets vertical DIC gradients between the epipelagic and mesopelagic layers, which in turn determines the DIC entrainment rates during seasons (or events) of upper ocean mixing (Fig. 7). ESMs with high NPP tend to have stronger vertical DIC gradients (Fig. 7 e-f, Fig. S4 d-f & S5 a-b). The relationship between $(dDIC/dz)_{max}$ and seasonal NPP_{max} is robust relationship in the Sub-Antarctic ($p < 0.01$) but is non-significant in the Antarctic region (Fig. 7 e-f). We note that the low number of models may affect the significance in this region where models have a large spread in the seasonal sea ice.

In summary, the processes characterising the present-climate Southern Ocean-DIC seasonality is primary production, respiration and entrainment mixing. In the Sub-Antarctic, entrainment fluxes are responsible for the DIC seasonal maximum; the dDIC/dt seasonal maximum occurs in early winter consistent with deep MLDs when maximum entrainment mixing is expected [37] (Fig. 5a). On the other hand, the dDIC/dt maximum occurs earlier than MLD maximum in the Antarctic, while MLDs are relatively shallow (~ 80 m) (Fig. 5 b,f,h). In this region, the dDIC/dt maximum coincides with respiration and remineralization, as demonstrated by AOU and regenerated DIC (Fig. 5 f, h and Fig. 6). The Antarctic DIC seasonal maximum is therefore first-order driven by near-surface respiration which peaks in autumn, but is sustained by entrainment mixing through winter when vertical mixing onsets in the models. This outcome suggests that the near surface respiration may be playing a more significant role than previously thought on the seasonal variations of surface DIC in the Antarctic region."

Comment: L239 highlight this is just for the case of this one high-end scenario upfront, it is "projected" under these assumptions. This information is a bit hidden

Reply: Corrected as suggested. The revised text is shown in Line 227 - 238

"At the end of the high warming scenario (2080 – 2099), the ocean is warmer; the Southern Ocean warms the most in the Subtropics (> 3°C), and the warming signal decreases poleward, reaching a maximum of 1°C in the Antarctic by the end of the 21st century (Fig. 8d). Upwelling of the cool deep circumpolar water and sea-ice minimizes warming in the Antarctic region, keeping the surface waters relatively cool [38]. In the projected future, the surface ocean is saltier in the Subtropics and fresher in the Antarctic region relative to the present climate (Fig. S5 e-f). Further, the increase of atmospheric CO_2 lowers the ocean CO_2 buffering capacity as the ocean take more CO_2, diagnosed by the increased Revelle Factor [11, 12,13,17] . The combination of these factors leads to a poleward migration of the dominant region of CO_2 sink from the Subtropics to the Antarctic region (Fig. 8 a-b). The mechanistic insight related to this poleward shift and why CO_2 uptake in the Subtropical region, although being the largest sink region in the present climate, do not increase as atmospheric CO_2 increase in the projected future is described below."

Comment: L245 I suggest moving this context about the Revelle factor to mL283 where it is applied, so that the dots are connected.

Reply: We removed the Revelle factor description out of the paragraph as suggested.

Comment: L252: I would remove the note about the expected scaling with atmospheric CO2 because you don't show what is expected. It may very well be that one region takes up more than expected and another less. However, your results do not distinguish that (and don't need to)

Reply: We removed this sentence in revised manuscript as suggested

Comment: L265ff. I may misunderstand the point, are you saying in winter the CO2 uptake increases because of higher atmospheric CO2 but in summer it remains similar because the effect of solubility and atmospheric CO2 compensate. That would not lead to an approx. zero change in the annual mean though?

Reply: The answer is yes, this is our argument for explaining why the CO_2 uptake increase the least in the Subtropics although atmospheric CO_2 has more than doubled. We don't however dwell too much on this point because it has already been shown by several studies. In revised manuscript we added add additional sentence that clarify this description, below is the revised paragraph.

Comment: L269 It may again be my understanding of the phrasing, why would warming decrease the amplitude? Under climate change, i.e. higher temperatures and higher pCO2 concentrations, the seasonal variability of the pCO2 should be more sensitive to temperature changes, i.e. an increase in the seasonal amplitude (Takahashi et al. 1993, Landschuetzer et al. 2018, and model studies…)

Reply: The decrease in ΔpCO_2 amplitude is because of the weakening of CO_2 solubility during the summer season, this lead to reduction in ΔpCO_2 in subtropics overtime as CO_2 solubility. Nevertheless, we agree that seasonal amplitude may be confusing, so we replaced with Fig. 8c with the net ΔpCO_2 change which has same meaning but more intuitive.

The text was modified in *Line 312 – 324* clarify this description.

"Thus, ΔpCO_2 magnitudes in the Antarctic region has the same sign in winter and summer in the Antarctic, this leads to an extensive net annual mean ΔpCO_2 change in comparison to the Sub-Antarctic and Subtropical regions where ΔpCO_2 seasonal averages have opposing signs (Fig. 3, 8c). The same-sign change in seasonal of ΔpCO_2 averages (Fig. 3 d-f) in the Antarctic region also applies to ESMs with a year-round thermally-driven ocean pCO_2 (e.g. CanESM5 and NorESM2, Fig. S11). Antarctic surface waters are constrained by seasonal sea-ice presence and upwelling of the circumpolar deep water from warming significantly above the freezing temperature (Fig. 8e). In contrast, sea-ice-free regions (Sub-Antarctic and Subtropical) are already significantly above freezing temperature (8 ^{o}C minimum, Fig. S1), and hence further warming reduces gas solubility. In the Antarctic region, warming-driven sea-ice melt increase the volume of near freezing surface waters which has a lower molecular kinetic energy of CO_2 and therefore strengthening the solubility of atmospheric CO_2. Subsequently, warming reduces the ΔpCO_2 in the Subtropics and enhances it in the Antarctic (Fig. 8c). Thus, in principle, the Antarctic surface oceans can still take up CO_2 through gas solubility even at the end of the 21st century in the high-emission scenario. "

Comment: Paragraph L304: could be structured in a clearer way. The sentences jump between topics, e.g. L309 about temperature, L310 about sea ice and MLD, L312 about temperature, L314 about sea ice – for the reading flow it would be nicer to have individual arguments explained in full.

Reply: We thank the reviewer for point this out, in the revised text we have defined the all terms used much earlier and explained framing at which their applied much earlier in section 212 (Line 163 – 172). This makes this section a lot easier to follow with some modification, the revised text is shown in Line 305 – 322

Line 163 – 172

"In the Antarctic region, the seasonal cycle of pCO_2 is primarily nonthermally controlled for both pCO_2-products and ESMs in the contemporary climate (Fig. 4 l), ESMs show a large spread for both ΔpCO_2 and $M_{T\text{-}nonT}$ in this region. The nonthermal processes that can be isolated from model outputs are net primary production, respiration and remineralization, indicators of mixing and stratification, and changes in sea-ice cover. Physical mixing processes will be here diagnosed through the rate of change of SST, mixed layer depth (MLD) and a stratification index based on the vertical density gradient (Fig. 5, 7). We will use the apparent oxygen utilization (AOU) to estimate respiration, and will decompose the total rate of change of DIC into preformed and regenerated components (Fig. 5-6). AOU is defined as the difference between oxygen at saturation and the in situ dissolved oxygen concentration and it is used here to estimate respiration within the MLD[34]."

Line 305 – 322

"The melting of sea-ice, stratification increase, MLD shallowing, DIC rates decline, and SST rates increase leads to a regime change in the primary driver of ocean pCO_2 from the nonthermal to thermal drivers in the winter to mid-spring seasons (JJASO) in the Antarctic region (Fig 4l). Ocean pCO_2 shifts from the seasonal mixing-driven CO_2 outgassing in the present climate (Fig. 4l) to a solubility-driven CO_2 uptake during winter in the projected climate. The shift to gas solubility as the primary driver of ocean pCO_2 changes in winter allows ESMs to take up CO_2 in both the winter and summer seasons by a combination of solubility and biological CO_2 uptake. A higher Revelle factor also enhances the effect of biological driven DIC changes and hence CO_2 uptake in summer. Thus, ΔpCO_2 magnitudes in the Antarctic region has the same sign in winter and summer in the Antarctic, this leads to an extensive net annual mean ΔpCO_2 change in comparison to the Sub-Antarctic and Subtropical regions where ΔpCO_2 seasonal averages have opposing signs (Fig. 3, 8c). The same-sign change in seasonal of ΔpCO_2 averages (Fig. 3 d-f) in the Antarctic region also applies to ESMs with a year-round thermally-driven ocean pCO_2 (e.g. CanESM5 and NorESM2, Fig. S11). Antarctic surface waters are constrained by seasonal sea-ice presence and upwelling of the circumpolar deep water from warming significantly above the freezing temperature (Fig. 8e). In contrast, sea-ice-free regions (Sub-Antarctic and Subtropical) are already significantly above freezing temperature (8 °C minimum, Fig. S1), and hence further warming reduces gas solubility. In the Antarctic region, warming-driven sea-ice melt increase the volume of near freezing surface waters which has a lower molecular kinetic energy of CO_2 and therefore strengthening the solubility of atmospheric CO_2. Subsequently, warming reduces the seasonal ΔpCO_2 amplitude in the Subtropics and enhances it in the Antarctic (Fig. 8c). Thus, in principle, the Antarctic surface oceans can still take up CO_2 through gas solubility even at the end of the 21st century in the high-emission scenario. Therefore, in the projected climate, the Antarctic operates in a hybrid mode between biologically-driven summertime and solubility-driven wintertime uptake. While the analysed ESMs show a large model spread in the Antarctic, the emergence of the Antarctic region as dominant CO_2 sink region in the projected climate is evident in all analysed ESMs. This outcome suggests that although ESMs still show significant differences the representation of biological and physical characteristics in the Antarctic region, the high-emission forcing projects a > 450% enhancement of the Antarctic CO_2 sink, suggesting this feature a robust."

Comment: L338 reads as if these regions would warm more because they are already warmer. I think what you mean is they warm more because there are additional processes in the Antarctic that limit warming

Reply: We thank the reviewer for pointing this out. We noticed this section was not clear, below the updated version shown in comment right above.

Comment: L343: I don't understand what you mean with the phrase starting "more so that…"

Reply: We removed this sentence and modified this section as shown two comments above.

Comment: L374 I believe it is more accurate to say that sea ice is responsible for increasing the regional CO2 sink. That, together with the local changes in the Subtropics (i.e. not increasing the CO2 sink as strongly which is not linked to sea ice), leads to the total shift in contribution/magnitude to the CO2 flux.

Reply: We thank the reviewer for highting this, we rephrased this sentence. We now points out that sea-ice is mainly responsible for setting the conditions to host the largest CO_2 sink in the Antarctic. The revised text is shown in Line 356 - 359

"First, the melting of sea-ice plays a major role in setting the conditions for the major shift in the Antarctic region. The melting of sea-ice will freshen and stratify the upper ocean, which on the one hand shoals and decreases the differences in MLD seasonal characteristics among ESMs, reducing the model spread significantly by the end of the 21st century (Fig. 5d)."

Comment: L387 If I understand this correctly this is about the "net" CO2 uptake, not the variability/ dominant driver of seasonal variability.

Reply: We apologies that we could address this comment. It is not clear what reviewer is point out, maybe he/she used the wrong line refence.

Comment: L410 I can't follow you how the solubility should undermine the role of respiration. Do you mean the effect of meting sea ice on the stratification which also affects the net effect of solubility?

Reply: In this section we are referring to shift to a thermal dominated systems, where respiration (and/or mixing) is longer leading the seasonal pCO_2 variability. This now clarified as in Line 391 - 394.

"On the other hand, CO_2 solubility is capable of becoming a significant contributor to CO_2 uptake when sea-ice melt is evident by subverting the role of near-surface respiration and/or seasonal mixing in winter CO_2 outgassing through the shifting surface carbonate system to a thermally-driven system."

Comment: L413 Suggest to rephrase "they may become similar" to something like the drivers of CO2 flux may become similar
Reply: Corrected as suggested

Other comments:
Comment: The paper needs spell-checking throughout and you jump between tenses e.g. L375.
I prefer "climate change" over "climate warming" because it is not just warming.
L45, L53, and many more: just call it "projected changes" instead of "future" or "predicted" or "projected future
Reply: We apologies for this inconsistency, in the revised manuscript now use "climate change" and "projected future" throughout the text as suggested

Comment: L54 regime shifts in "projections" or "scenarios"
Reply: Corrected as suggested.

Comment: L90 FCO2 -> flux (acronym is only introduced later)
Reply: Corrected as suggested

Comment: L90 pCO2-product values given with two digits, the others with one
Reply: Corrected as suggested

Comment: L174 repetitive clause: "not all ESMs" and "other models"
Reply: Corrected

Comment: L200 What is the "nearly symmetric feature"
Reply: This phrase was indeed confusing, we removed it.

Comment: L255 It "changes" in the Antarctic – but how? – and the change (?) "decreases" relative to? – can you either rephrase or just delete the sentence please, because the following sentence gives the same information.
Reply: We deleted this sentence, it was not necessary as the reviewer points outs suggested.

Comment: L255 Does the delta refer to the air-sea difference? If so, why is it delta-FCO2? Or the difference between present-day and projection? If so, I would rephrase because delta-pCO2 has the connotation of air-sea difference rather than changes over time
Reply: This sentence was removed.

Comment: L265 ". ESM" instead of comma
Reply: Corrected.

Reviewer #2

The paper by Mongwe and colleagues, a resubmission in which they've considered, addressed, and integrated comments by 3 reviewers, uses Earth System Models (ESM) to examines mechanisms of CO2 uptake in the Southern Ocean (SO) and the influence of climate scenarios on patterns of uptake. They ground their analysis in observations, using multiple gridded products derived from observations as points of comparison to ESM estimates in the present, and a baseline for comparison of future scenarios.

The authors have significantly modified the manuscript to address reviewer comments, adding clarifying details, context, and results. Additional text and figures have strengthened the manuscript. The reviewers have addressed my prior minor comments, alongside extensive feedback by author reviewers.

You'll find below additional minor feedback for consideration. The paper is an important contribution to the field and merits publication.

Minor comments:
Comment: L 194: Should be Fig 6 h
Reply: Corrected

Comment: L 290-292: This section is very useful to introduce, but consider revising sentence structure here and in following portions (L 293-295, L 297-300). As written there are sentence fragments which impede lecture (at least from a US reader's perspective).
Reply: We thank the review highlighting this. We modified this paragraph as per suggestion. Below the updated text *Line 262 – 283*.
"In the Sub-Antarctic region, the sign of the projected $M_{T\text{-}nonT}$ (Fig. 4k) also remains unchanged with respect to the present climate; the majority of the ESMs still show a weak thermally driven seasonal cycle of ocean pCO_2 as in the present climate. The thermal and nonthermal components are increased by nearly equivalent amounts (Fig. 4h). Since the seasonal warming and cooling rates show little to no change in the projected climate (Fig. 5i), the increase in the thermal pCO_2 component is primarily due to the ocean pCO_2 increase (Eq. 1 in Methods). pCO_2 is more sensitivity to temperature in a high pCO_2 environment because of the Revelle factor increase [12]. The Revelle Factor increase enhance the sensitivity of pCO_2 to DIC and temperature changes, this effect is increases poleward and is strongest

in the Antarctic consistent with Revelle factor pattens [12,13,17] (Fig. S12). In particular, the Revelle Factor increase enhances the sensitivity of pCO_2 to primary production and respiration (and mixing) driven DIC changes on the nonthermal pCO_2 components with nearly equivalent magnitudes but opposing directions (Fig. 4 h-i). Although DIC rate of change declines in the projected future (Fig. 5 e-f), its impact on the nonthermal pCO_2 contribution is larger than the present climate. This decline in DIC rates is driven by two factors. First, the upper ocean is more stratified; stronger density vertical gradients (Fig. 7a-b) due to warming (Fig. 8e) and freshening of the upper ocean (Fig. S5 f, Fig. 5 c-d). Secondly, the anthropogenic ocean DIC increase from rising atmospheric CO_2 propagates from the surface: DIC increases more at the surface than at depth (Fig. S9), consequent weakening of the vertical DIC gradients (Fig. 7 c-d), leading to a weaker DIC entrainment potential during vertical mixing. In summary, the impact of the Revelle factor increase partly self-compensates in Sub-Antarctic; it enhances the impact of mixing and respiration-driven DIC changes on surface pCO_2 in one direction, and primary production in the opposing direction, Fig 4i."

Comment: L 353-357: I would restructure to highlight your main finding in the first 1-2 sentences before going into uncertainty (which you discuss in L 363 onwards). I suggest some variation of: "Examining multi-ESM projections of physical and biogochemical processes, a robust pattern emerged whereby anthropogenic impacts manifested themselves through altering the seasonal carbon cycle with a large-scale shift in the carbon uptake from the Subtropics to the Antarctic region. This major finding was robust even when accounting for uncertainties in multi-model projections"
Reply: We very much appreciate this suggestion, it was adapted it as suggested.

Comment: L362: I would separate uncertainty discussion into a separate paragraph, leaving the first paragraph to highlight the novelty of the result and how it stands in context of previous findings.
Reply. We appreciate the reviewer's suggestion, however we only applied minor modification because we thought uncertainties in the Antarctic are part of the problem and story we are presenting.

Comment: L428: Advect instead of advent
Reply: Corrected

Comment: L438: the inclusion of ice sheets in... (awkward as written)

Reviewer #3

The study diagnoses the output of a set of Earth system models to document how the Southern Ocean carbon sink alters in the future, both in terms of latitudinal contrast in response and the effect of the controlling processes. The primary response involves a seasonal, solubility-driven change involving the melting of sea ice leading to a shallowing of the mixed layer, decreasing the entrainment of carbon-rich deep waters and so leading to an uptake of CO_2 south of the polar front.

My concerns were:

1. Combining together the mixing and biological effects missed a critical part of the story. The authors have addressed this concern and added an analysis of preformed and regenerated DIC.
2. There is a plausible speculation that the new solubility feature is only active during relatively cool sea surface temperatures. Can the authors go further and document any criteria for this response to hold that links to for example, the shallowing of the winter mixed layer, while retaining the presence of sea ice. The authors have modified a figure to address this concern. A minor point is that the text could have been improved here, L334 to L350. Many of the sentences start with "This is…", which is sometimes unclear to the reader what is referred to. Much better to be explicit.

As a general comment, I think that the mechanistic insight is very useful and the diagnostics are very comprehensive, although the text could have been more concise and clearer.

In summary, this study provide new mechanistic insight into how the Southern Ocean carbon uptake may vary in the future. I support acceptance.

Reply: We thank the reviewer for this feedback.