Response to RC2 (reviewer comments in grey)

Wong et al. analyze the global and regional variability of air-sea CO2 fluxes based on the LDEO-HPD data set. The data set integrates observations and global ocean biogeochemical models using machine learning techniques, allowing to extend the air-sea CO2 flux data set back to the 1960s. The authors extract the dominant modes of variability from the CO2 flux data and interpret them based on climate modes of variability and their associated physical imprints.

In my view, the study advances the research field because: 1) the observation-based data set provides an extended estimate going back to 1960s (while most CO2 data sets start in the 1980s), 2) the results show a synchronous variability between tropical Pacific and Southern Ocean air-sea CO2 fluxes, mediated by climate teleconnections between El Niño and Southern Ocean. This result has not been (to my knowledge) extensively addressed in past studies.

The analyses methods are to my understanding correctly applied and the figures are clear.

However, I believe that the story is not yet fully mature. I think the authors should do more to contextualize their results into the framework of past studies and to further analyze the data to explain the mechanisms at play. Especially the connection between tropical Pacific variability and Southern Ocean air-sea CO2 fluxes, which is a prominent result of the paper, could be better explored.

This a summary of my main comments:

Literature: there are a few studies on the role of ENSO for Southern Ocean air-sea CO2 fluxes (e.g. Verdy et al., 2007) and many more on the connection between ENSO and the Southern Ocean physics (e.g. Ciasto et al., 2011; Huguenin et al., 2024; Shi et al., 2024). Referring to these (or other) studies could already help explaining the processes at play in this study.

We thank the reviewer for their detailed assessment and comments to improve the paper. We now further contextualize our results within ENSO-Southern Ocean literature while framing the mechanistic questions as open problems, revising the manuscript accordingly.

We thank the reviewer for these references. We have incorporated these studies into our expanded discussion of the ENSO-Southern Ocean connection to better contextualize our Southern Ocean results.

Analysis: the authors conclude that the reasons for which a La Niña phase causes CO2 outgassing anomalies in the Southern Ocean remain unresolved. Have the authors considered analyzing changes in upwelling or in sea ice cover? Once a clearer picture emerges, I always think it's a good idea to include a schematic showing the mechanisms at play. This is of course just a suggestion.

We thank the reviewer for this suggestion; we explored both avenues. For upwelling, we computed wind stress curl and Ekman pumping composites from both ORAS5 stress components (Figure RC2-1). (The vertical velocity ORAS5 diagnoses, shown in panel D, is derived from the horizontal divergence of currents and is too noisy at the grid scale to interpret, so we focus on the wind-derived estimate instead.) The result is regionally divided. In the Pacific sector between 50 and 60°S the zonal wind stress and the implied Ekman upwelling both strengthen during the positive PC1-FCO_2 phase (of order +3 m yr^{-1}), co-located with cooler SST and with the largest $\Delta fCO_2'$ anomalies in that sector (roughly +4 µatm), and the two composite fields are positively correlated across the Pacific sector north of 60°S. Increased wind-driven upwelling is therefore a plausible contributor there, as the reviewer suggests. South of 60°S the sign reverses: Ekman pumping weakens (by roughly 3 to 5 m yr^{-1}) while $\Delta fCO_2'$ is largest (roughly +6 to +7 µatm), so the high-latitude outgassing requires a different explanation. We now restructure the Southern Ocean discussion around this contrast, presenting the co-location as consistency rather than attribution.

For sea ice, the ice fraction used in the LDEO-HPD flux calculation enters FCO_2 through the (1-fice) scaling, so compositing it is not an independent test. We repeated the composite with ORAS5 sea ice concentration (Figure RC2-2) and obtained the same answer: more ice during the positive PC1-FCO_2 phase, with a Southern Ocean mean change of +0.004 in both products and a spatial correlation of 0.78 between them. We now add sea ice to the discussion as a candidate mechanism, noting that ORAS5 is not fully independent of the Hadley lineage.

We agree a schematic would be valuable once the mechanisms are better constrained. Since our analysis supports a wind-driven pathway in one sector but not at the high latitudes where the outgassing is largest, and the processes that would close that budget are ones we cannot yet quantify with available observations, a schematic now would risk implying a settled mechanism. We instead sharpen the discussion to set these out as motivated open problems.

Figure RC2-1: Southern Ocean (polar stereographic, >40∘S) composite changes in A) $|\tau|$, wind stress magnitude (units: N m^{-2}), B) curl(τ), wind stress curl (units: 10^{-8} N m^{-3}), C) w_{Ek}, Ekman pumping (units: m yr^{-1}), and D) w_{100}, vertical velocity at 100m (units: m yr^{-1}), over the full period of PC1-FCO2 (1962-2021). Composites are positive minus negative PC1-FCO2 phases; multiyear means (1959-2024) are shown to the right of each for context. No variable is detrended. Sign conventions: w_{Ek} and w_{100} are positive upwards, so positive composite values indicate upwelling; since $f < 0$ in the Southern Hemisphere, negative curl(τ) corresponds to positive w_{Ek}. All panels derive from ORAS5: A-C from the zonal and meridional wind stress components, D from the vertical velocity diagnosed from the horizontal divergence of currents.

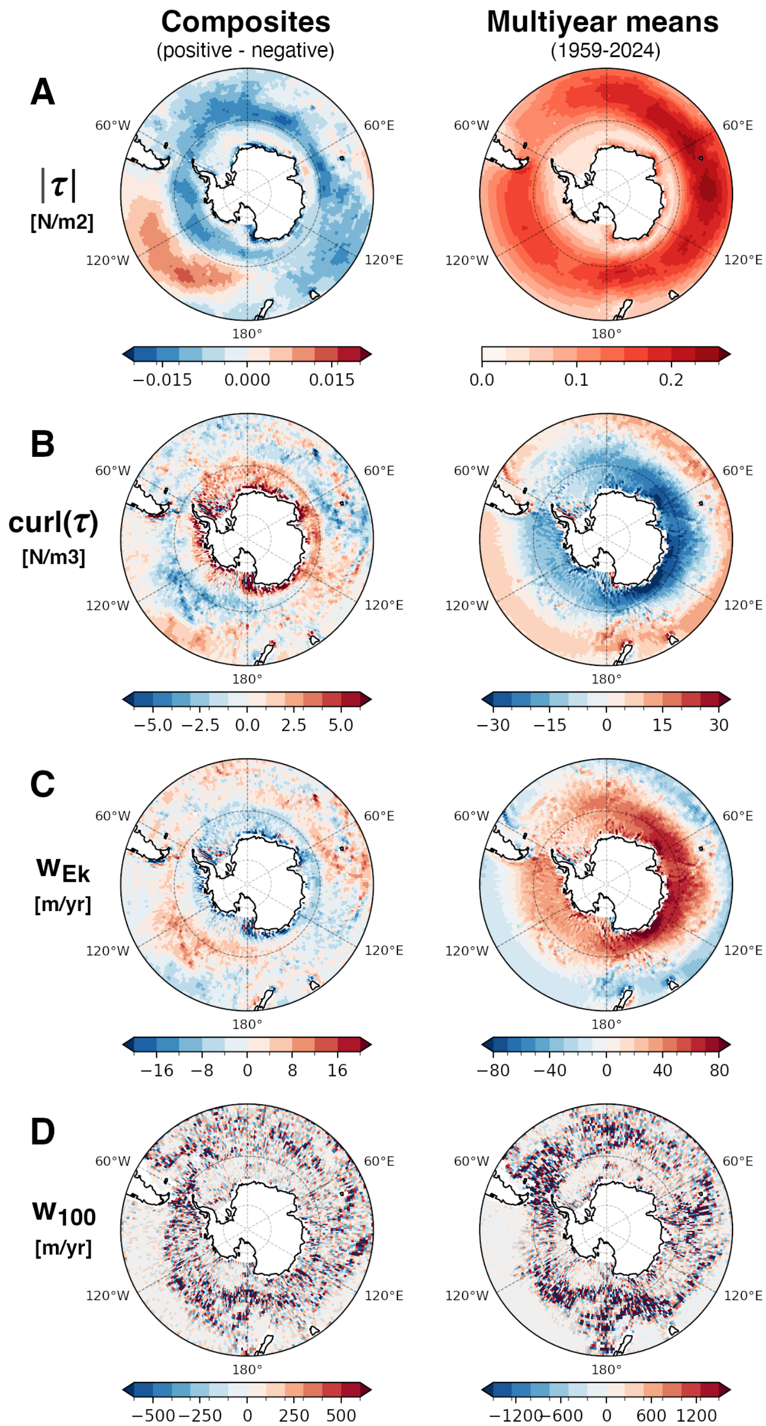
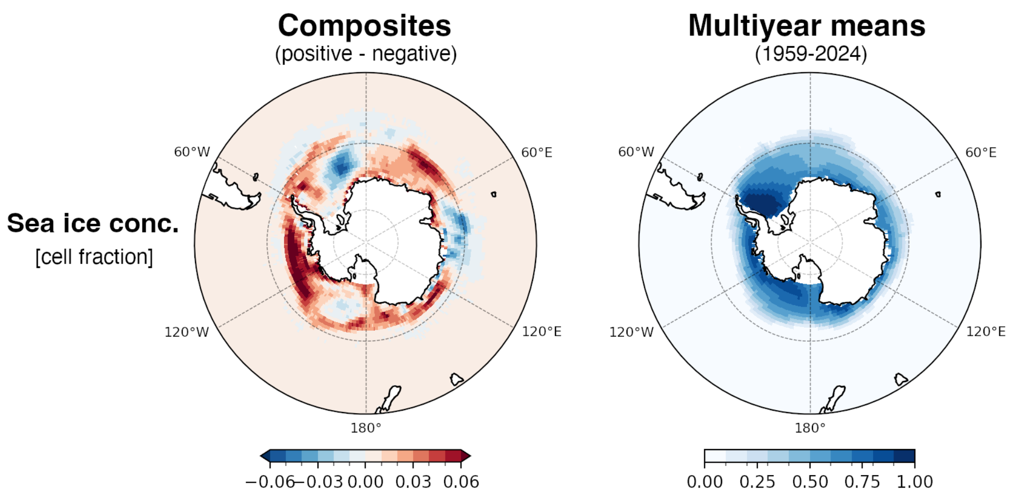


Figure RC2-2: Southern Ocean (polar stereographic, >40∘S) composite change in ORAS5 sea ice concentration (units: fraction), over the full period of PC1-FCO2 (1962-2021). The composite is positive minus negative PC1-FCO2 phases; the multiyear mean (1959-2024) is shown to the right for context. The variable is not detrended. Positive composite values indicate greater sea ice concentration during the positive PC1-FCO2 phase. ORAS5 sea ice concentration is a separate product from the HadISST ice fraction used for LDEO-HPD's flux calculation.



Abstract: somewhat related to my comments above, I also find the abstract too vague. It could be more informative, concrete, and quantitative. What is the main goal? What are the main results? Maybe, what are the implications?

We have revised the abstract to be more detailed as the reviewer suggests. The main goal of this study is to use the extended 66-year LDEO-HPD product to look at the decadal variability of global and regional air-sea CO2 fluxes, placing previously-reported variability in the 1980s and 2000s in a longer-term context for the first time. Our main results are that we find synchronous signals between the tropical Pacific and the Southern Ocean, where the dominant mode of decadal flux variability strongly correlates with climate modes like ENSO. The mechanisms between decadal flux variability and the ENSO-Southern Ocean connection remain unresolved, and we say so explicitly rather than leaving it implicit. In the abstract, we now add quantitative detail accordingly. We also add the main implication of the extended record: that the widely reported weakening of the sink in the 1990s represents a return to the state of the 1960s and 1970s rather than an anomalous excursion, and that attributing sink changes from records beginning in the 1980s or 1990s risks mistaking one phase of decadal variability for a secular trend.

Novelty: I have the impression that the question of how the PDO/ENSO impacts the Southern Ocean carbon uptake has not been extensively explored in past studies. This offers a big opportunity for the current paper to fill this gap. This novelty could be highlighted, maybe also in the title?

We thank the reviewer for this observation and agree that this is among the most significant results of the extended record. We now give the tropical Pacific-Southern Ocean connection greater prominence in the title, abstract and conclusions.

We would characterize the novelty precisely. The influence of Pacific decadal variability on the Southern Ocean has been examined previously (as far back as Garreaud and Battisti 1999, J. Climate), in Earth System Models (Resplandy et al., 2015), in observation-based products over shorter records (Landschützer et al., 2019), and for Southern Ocean mixed layer properties (Li and England, 2020); its pan-Pacific, pole-to-pole expression is likewise established (Di Lorenzo et al., 2023). What is new here is that the synchronous, same-sign covariation of the two regions emerges as the leading mode of global decadal air-sea CO_2 flux variability, explaining 36% of the variance, and that it is resolved observationally across more than one full cycle of that variability - which the 66-year LDEO-HPD record makes possible for the first time.

We will frame this as an observed covariation rather than a formally attributed mechanism. As set out in our response to the reviewer's comment on upwelling and sea ice, establishing the causal pathway would require constraints on biological processes that are not available over this period, and we will therefore present the mechanism as an open problem.

Title: I find the title somewhat too general. Most of the paper focuses on the role of the PDO/ENSO for the global and regional variability of air-sea CO2 fluxes. Maybe the authors could consider a more informative title such as: “60 years of global air-sea CO2 flux observations reveal strong synchronous behavior of the tropical Pacific and the Southern Ocean”. If the authors want to keep the title more general, I would at least mention that the study is observation-based, such as “60 years of observed global air-sea CO2 flux variability”.

We thank the reviewer for this suggestion and adopt it. The revised title is: "Six decades of observations-based global air-sea CO_2 flux variability: synchronous decadal signals in the tropical Pacific and Southern Ocean". This signals the observational basis of the study and highlights the synchronicity of the two regions, as the reviewer suggests here and in the preceding comment.

We will use "observations-based" rather than "observations" because LDEO-HPD is a hybrid product in which ocean biogeochemical models provide the prior; as noted in the Appendix, the models determine the decadal variability of the product prior to 1982. We note that the title describes the synchronous behaviour of the two regions without asserting a causal mechanism, consistent with our framing of the underlying mechanism as an open problem.

Specific Comments
Abstract:

We thank the reviewer for the constructive comments, which we address point by point below.

Line 1: “Ocean” instead of “ocean carbon sink”?

We reworded the opening sentence to refer to the ocean rather than the ocean carbon sink as the agent of uptake, since the sink is the flux itself.

Line 9: define MEI before

We will define MEI at first use in the abstract.

Line 10: “weakening of the Southern ocean carbon sink in the past decades.” This is not what your observations show (Fig. 2)

The reviewer is correct, and we have checked this against Fig. 2. The Southern Ocean decadal flux anomaly is negative (stronger sink) over 1980-1997, positive (weaker sink) over 1998-2014, and returns to negative values over 2015-2024 (regional decadal anomalies of -0.12, +0.10 and -0.04 PgC yr^{-1} respectively). "Recent decades" is therefore imprecise: the weakening is confined to the period from the late 1990s to the mid-2010s, after which the anomaly reverses. We have replaced the phrase with that specific interval in the abstract and wherever the same wording appears.

Introduction:
Line 30: growth rate of atmospheric CO2?

Yes, we mean the growth rate of atmospheric CO_2, and now make this explicit.

Lines 35-36: is there evidence of a weakened sink post 2000? Or was prior 2000 meant?

This is an error. The mechanisms cited here (Le Quéré et al., 2007; Landschützer et al., 2016) concern the weakening of the sink through the 1990s, not after 2000. We have corrected the period.

Line 45: do you really mean “carbon-climate feedbacks” (implying a climate effect on atmospheric CO2 which then feeds back on climate) or is the meaning here rather “climate impacts”?

We do mean carbon-climate feedbacks in the strict sense: a climate-driven change in ocean carbon uptake alters atmospheric CO_2, which in turn influences climate. This terminology is consistent with the literature on Earth System Modeling, including Friedlingstein et al. 2014 on CMIP5 and Arora et al. 2020 on CMIP6.

Arora, V. K., Katavouta, A., Williams, R. G., Jones, C. D., Brovkin, V., Friedlingstein, P., et al. (2020). Carbon-concentration and carbon-climate feedbacks in CMIP6 models and their comparison to CMIP5 models. *Biogeosciences*, *17*(16), 4173-4222. https://doi.org/10.5194/bg-17-4173-2020

Friedlingstein, P., Meinshausen, M., Arora, V. K., Jones, C. D., Anav, A., Liddicoat, S. K., & Knutti, R. (2014). Uncertainties in CMIP5 Climate Projections due to Carbon Cycle Feedbacks. *Journal of Climate*, *27*(2), 511-526. https://doi.org/10.1175/jcli-d-12-00579.1

Line 50: maybe a matter of taste, but I would suggest using a different definition of fCO2 (e.g. closely related to pCO2)

We reworded this to define fCO_2 as the fugacity of CO_2, that is the partial pressure corrected for the non-ideal behaviour of the gas.

Lines 62-64: I’m not sure I follow the meaning of this sentence. Does the divergence between data sets identify a lack of understanding? I would also add that it highlights the methodological issues related to data sparseness and model deficiencies.

We have rephrased. Divergence between independent estimates does not by itself locate a gap in process understanding; it establishes that at least one estimate is in error, which may reflect incomplete understanding of processes, sparseness of the observations, or deficiencies in the models. We now state all three rather than only the first.

Lines 68-69: "As the climatology of these misfits is the dominant component of the total misfit for all models": in the sense that the model-data misfit remains relatively stable in time so that the corrections can be used also to extrapolate in the past? I suggest rephrasing.

Yes, that is the intended meaning. We rephrased to state that the model-data misfit is dominated by a component that is stable in time, which is what justifies applying a climatological correction derived from the observed period to the pre-observation period.

Lines 70-72: since Bennington et al. (2022) also deals with variability of the ocean carbon sink since the 1950s, could you outline which are remaining knowledge gaps and in which way your study marks a step forward?

Bennington et al. (2022) introduced the extension of LDEO-HPD back to 1959 and documented its construction and skill. The present study is the first to use that extended record to characterize the decadal variability it contains: the dominant spatial mode and its time series, the regional contributions, the partition between ΔfCO_2- and wind-driven variability, and the relationship to Pacific climate indices. We have added a sentence making this distinction explicit.

In general, on the Introduction: since a large focus of the study is the impact of climate modes on air-sea CO2 fluxes, could you give some background on past studies dealing with this topic?

We have added background in the Introduction on climate-mode influences on air-sea CO_2 fluxes, covering ENSO in the equatorial Pacific (Feely et al., 1999; McKinley et al., 2004; Wong et al., 2022), Pacific decadal variability (Landschützer et al., 2019), and ENSO influences on the Southern Ocean (Verdy et al., 2007; Ciasto and England, 2011; Huguenin et al., 2024; Shi et al., 2024).

Methods:
Lines 101-106: it's not so clear to me which variables are used for the reconstruction of missing fCO2, which are used for FCO2 computation based on eq. 1 and which are used to study the connections between climate variability and FCO2. Could you please specify?

The predictor variables used to reconstruct fCO_2 are specified in Gloege et al. (2022) and Bennington et al. (2022) and are not re-derived here; we now refer the reader to those papers explicitly. We also add a short statement distinguishing the three sets of variables: those used as machine learning predictors, those entering the flux calculation of Eq. 1 (wind speed, solubility, ice fraction and ΔfCO_2), and those composited against PC1-FCO_2 (u_{10}^2, SST and MLD).

Line 125: "northern boundary at 40N" is certainly a typo.

Thank you for catching this typo. We will correct this to 40°S

Line 129: I suggest merging Section 2.3 and 2.4 in one single one (Variability analysis)

We have merged §2.3 and §2.4 into a single "Variability analysis" section.

Line 131: comma too much after FCO2?

We have corrected this.

Line 134: in which way are the time series detrended? Do you remove also the intercept so that the time series fluctuate around zero or not? Referring to Fig.1b,c would be helpful.

We remove a least-squares linear fit at each grid cell, including the intercept, so the detrended series fluctuate about zero; the monthly climatology is then removed to give anomalies. We now state this explicitly and cross-reference Fig. 1b,c.

Line 137: "allowing for fluxes to be compared between regions that have different areas." I'm not sure I follow. One can compare also mean (instead of intergral) values for regions having different areas. Is your meaning rather: "to take into account the fact that different regions have different area sizes".

The reviewer's reading is correct and we now adopt their wording: area integration accounts for the differing sizes of the regions, so that their contributions to the global flux are comparable. We report both the spatially averaged flux rate and the area-integrated flux for this reason.

Line 146: could you explain how you treat the time series before computing the EOF (detrending, smoothing, spatial weighting, normalization?)

We now add the preprocessing details. Before the decomposition the field is detrended at each grid cell, de-seasonalized, low-pass filtered with a 3rd-order Butterworth filter at a 7-year cutoff applied forwards and backwards, and trimmed by 42 months at each end to remove filter edge effects. The EOF is computed with cosine-latitude weighting, without standardization, and the principal components are not normalized.

Lines 158-162: Do I understand correctly that the primed quantities are "simply" anomalies computed with respect to the lon-term mean? If yes, maybe one could introduce them this way.

Yes; we now introduce the primed quantities explicitly as anomalies relative to the long-term mean.

Lines 168-171: I am bit confused on the procedure. Long-term means of both ΔfCO2 and wind speed are always computed on the raw data or just for wind speed? Why are ΔfCO2 deviations computed on the detrended time series but the same is not for wind speed? I imagine this is because in the case of ΔfCO2 we wish to remove the external CO2-effect, but for wind speed we want to retain the internal climate effects. I suggest to explain the reasons for using this procedure

The reviewer's interpretation is correct and we now add the rationale. ΔfCO_2 deviations are detrended so that the externally forced direct impact on atmospheric CO_2, which dominates the ΔfCO_2 trend, does not project onto the decadal analysis. Wind speed deviations retain their trend because there is no comparable external imprint to remove and because the trend in Southern Hemisphere westerlies is itself part of the physical signal of interest. Long-term means are computed on raw data for both variables. We now note in the revised text that this choice matters for interpreting Fig. 6B (see our response to the comment on L328-329).

Line 164: the quantity FOC2' is called later "flux-proportionate variability". Could you explain here that you call it this way and why?

We now introduce "flux-proportionate variability" at first use, with a one-line explanation that FCO_2' is proportional to, but not equal to, the flux because the gas transfer coefficient is held aside.

Line 165-166: typically, figure numbers apprear in order in the text.

We have reordered the figure references so that they appear in numerical order.

Line 174-175: To compute the composites, I understand that you select all months in which the PC1-FCO2 is positive or negative. In past studies, a threshold is often used, so that only the months/years when the anomaly is more prominent are considered. Could you justify this choice?

Also, are the spatial pattern of EOF1-FCO2 and the FCO2 pattern revealed from composites (positive minus negative) consistent? (consider mentioning this)

We use all months of each sign rather than applying a threshold. The composited quantity is already low-pass filtered at a 7-year cutoff, so it varies slowly and sign changes are not noise-driven; thresholding would mainly shorten the sample without changing which regimes are contrasted. Using all months also keeps each composite contiguous in time, which is what allows them to be read as a decadal regime shift rather than a selection of extreme events. We have added this justification.

They are consistent. The spatial correlation between the EOF1-FCO_2 pattern and the positive-minus-negative composite of ΔfCO_2' is r = 0.82 across the global ocean. We have added a sentence stating this.

Line 181: consider using the wording "annual climatology" rather than "multiyear mean"?

We retain "multiyear mean". An annual climatology conventionally denotes a twelve-month seasonal cycle, whereas Fig. 1A shows a single time-mean field over 1959-2024. We will clarify the caption so the distinction is explicit.

Results:
Line 198: Why using "While"? The two parts of the sentence do not appear to be opposing each other.

Rephrased.

Line 208-209: since you are showing three panels on FCO2-EOF2, I think it would be good to spend a few more words on it, also because it explains quite some part of the variability (19%). Is it connected to some other variability mode? Interestingly, it involves an east-west dipole in the tropical Pacific, and reinforces the Southern Ocean outgassing. PC2-FCO2 shows large positive values around 2000, when an overall global weakening of the ocean carbon sink was detected, most prominently in the Southern Ocean. Some thoughts on this?

We have expanded the text on EOF2, describing its east-west dipole in the tropical Pacific, its same-sign Southern Ocean loading, and the large positive PC2-FCO_2 values around 2000 that the reviewer notes. We are cautious about attributing EOF2 to a specific climate mode: it explains 19% of the variance with a less distinct spatial structure, and we have not carried out a formal test of its separability from the higher modes. We will therefore describe the pattern and its timing but flag its attribution as a direction for future work.

Lines 223-224 and 258: I don't understand what is meant with the 1.5 cycles.

We have removed the "1.5 cycles" phrasing. We had meant that PC1-FCO_2 completes one full negative-to-positive oscillation within the record and part of a second, but the wording is unclear and the point is better made by naming the phases explicitly.

Line 238: since here you are mostly focussing on the low-frequency variability, is it appropriate to talk about la Niña, which is a higher-frequency event? Maybe rephrase?

We have adjusted the terminology so that decadal-scale behaviour is described as "La Niña-like conditions" rather than as discrete events.

Line 240: in several parts of the subpolar Southern Ocean (where most of your positive FCO2 anomalies are located), the ocean is actually a source of CO2 (Fig. A1A, right).

The reviewer is correct. In the multiyear mean the subpolar Southern Ocean is a CO_2 source: south of 60°S the area-weighted mean ΔfCO_2 is about +7 µatm, with roughly 80% of the area having $\Delta fCO_2 > 0$, whereas between 40 and 50°S it is about -18 µatm. The region-wide mean we quoted (-17.1 µatm) therefore masks a sign change with latitude. We have corrected the text and distinguish the two cases: where the mean state is a source, a positive composite change in $\Delta fCO_2'$ represents intensified outgassing rather than a weakened sink, although in both cases net uptake is reduced.

Line 242: In the Pacific sector, it looks like an equatorward shift

The reviewer is right, and the composites support this. In the Pacific sector the zonal wind strengthens on the equatorward flank of the westerly belt between 40 and 60°S and weakens on the poleward flank south of 60°S, with the ORAS5 zonal wind stress showing the same dipole. We now describe this as an equatorward shift of the westerlies in this sector rather than a uniform change in their strength.

We will also recast the "except in parts of the South Pacific" clause in that sentence: what strengthens there is the zonal wind component and the zonal wind stress, not the scalar wind speed.

Lines 242-246: I find the chain of mechanisms difficult to follow. Based on the literature La Niña is associated with a stronger Amundsen Low (e.g. Huguenin et 2024), which would then correspond to stronger -not weaker- winds in the Pacific sector. Also, I don't understand which mechanism should cause the stronger outgassing in the Pacific sector. Since we see concomitant MLD shoaling and cooling (two processes which should not lead to increased outgassing) a process that comes to my mind is increased upwelling. Maybe wind stress curl composites could give some information on this.

The reviewer is right, and our analysis supports their suggestion. Two clarifications are needed. First, the distinction between wind speed and wind stress. The wind speed contains components from the change in the mean winds, which the reviewer refers to, as well as changes in the higher frequency storminess. In the Pacific sector between 40 and 60°S the ERA5 zonal wind component strengthens during the positive PC1-FCO_2 phase while the scalar wind speed is essentially unchanged, so the winds there become more westerly rather than faster. The ORAS5 zonal wind stress composite independently shows the same regional pattern (strengthening there, against weakening at the same latitudes in the Indian and Atlantic sectors). Scalar u_{10}^2 by contrast weakens in every sector and latitude band of the Southern Ocean, with only a small fraction of the region's area showing an increase. This is consistent with a stronger Amundsen Low under La Niña-like conditions (Huguenin et al., 2024), and we now make the distinction explicit, since it reconciles our result with the literature the reviewer cites.

Second, following the reviewer's suggestion we computed wind stress curl composites, using both ORAS5 stress components, together with the Ekman pumping they imply. In the Pacific sector between 50 and 60°S, where the zonal stress strengthens, Ekman upwelling strengthens (of order +3 m yr^{-1}), co-located with the SST cooling and with the largest $\Delta fCO_2'$ anomalies in that sector (roughly +4 µatm); across the Pacific sector north of 60°S the two composite patterns are positively correlated. Increased upwelling is therefore a plausible driver of the concurrent cooling and outgassing in the Pacific sector, as the reviewer proposes.

The mechanism does not extend across the whole region. South of 60°S the composite Ekman pumping weakens (by roughly 3 m yr^{-1} between 60 and 70°S and 5 m yr^{-1} south of 70°S) while $\Delta fCO_2'$ is largest (roughly +6 to +7 µatm), so the outgassing there requires a different explanation. We have revised this passage around the regional contrast rather than presenting a single region-wide chain of mechanisms.

Line 245-246: not clear why higher wind speeds in the Pacific lead to cooling, while stronger winds south of Australia to warming.

The sentence as written attributes both the cooling and the warming to strengthened wind speeds, which is not what the composites show, and we have revised this.

Scalar wind speed weakens almost everywhere in the Southern Ocean in our composite, with only a small fraction of the region's area showing an increase. What strengthens in the Pacific sector is the zonal wind component between 40 and 60°S and, correspondingly, the zonal wind stress; the winds there become more westerly rather than faster. The SST cooling is co-located with that: of order -0.1 °C in the Pacific sector between 50 and 70°S, compared to warming over roughly 60% of the Southern Ocean elsewhere, including south of Australia (about +0.1 °C), where both wind speed and zonal stress weaken.

The revised text therefore contrasts cooling where the zonal wind stress strengthens - consistent with the enhanced Ekman upwelling reported in our response to the comment on L242-246 - against warming where the winds weaken and wind-driven mixing and evaporative heat loss are reduced. This makes the two SST responses part of a single coherent regional pattern rather than an unexplained contrast.

Discussion:
Line 278: why should the damped variability in the 60s and 70s suggest that “the magnitude of the sink in the 1960s and 1970s may actually have been weaker during that period”?

We clarify the reasoning. Damped variability in the reconstruction before 1982 means the amplitude of departures from the record mean is likely under-represented in that period. Since the reconstruction places the 1960s and 1970s on the weak-sink side of the mean, an under-represented amplitude implies the true sink may have been weaker still, and correspondingly that the strengthening around 1980 may have been more pronounced than Fig. 1B suggests.

Line 281-284: I think this is an important discussion point that should be expanded. The decadal variability of the global carbon sink shown in this study is very pronounced,

especially in the Southern Ocean. But this is the region where - because of undersampling- pCO2 products are thought to overestimate the decadal variability of the carbon sink. Could the decadal variability of the LDEO-HPD product be put into the context of other available products?

We agree this deserves expansion. We now quantify the amplitude of Southern Ocean decadal variability in LDEO-HPD against the GOBM ensemble already shown in Figs A2 and A3, and we state the reviewer's caveat explicitly: pCO_2 products are thought to overestimate Southern Ocean decadal variability where sampling is sparse (Gloege et al., 2021; Hauck et al., 2023), by an amount that differs between products (Fay et al., 2025). A full comparison against other observation-based products is beyond the scope of this study.

Line 303: "and then using the index values of years to compute the PC1-FCO2-based composite." Grammar doesn't sound correct

We have corrected this.

Line 305: it is a bit confusing that the text refers to positive PDO phases (which in turn are associated with warmer SSTs in the tropical Pacific), but in Fig. 7 the SST anomalies in the tropical Pacific are negative even though the caption says: "values are regressed onto the PDO index", so shouldn't we be seeing the anomalies associated to positive phases of the PDO?

The figure is correct; the caption is ambiguous, and we rewrite it. The fields are regressed onto the PDO index, but the regression-predicted fields are then composited over the positive-minus-negative PC1-FCO_2 phases, and PC1-FCO_2 is anticorrelated with the PDO. The mean PDO index is -0.17 over the positive PC1 months and +0.73 over the negative PC1 months, so the composite difference corresponds to a PDO change of -0.91, that is, a shift towards the negative PDO phase. The cool tropical Pacific SST anomalies in Fig. 7 are therefore of the expected sign. The revised caption now states that the composite is taken over PC1-FCO_2 phases and corresponds to a shift towards negative PDO.

Lines 328-329: "as wind speeds weakened from the late 20th to early 21st century". This statement is confusing. We know from multiple evidence that Southern Hemispehre westerlies strengthened in the past decades. Could you explain better? Is the trend removed here?

The trend is not removed, and the reviewer has identified an ambiguity in our text. Southern Ocean westerlies do strengthen across the record: area-weighted u_{10}^2 rises from 87.0 $m^2\ s^{-2}$ over 1959-1980 to 96.5 $m^2\ s^{-2}$ over 2018-2024, a trend of +1.9 $m^2\ s^{-2}$ per decade, consistent with the evidence the reviewer cites. The composite in Fig. 6B is a different quantity: because the positive and negative PC1-FCO_2 phases sample different parts of the record, the trend leaves a residue in the composite difference, but a small one. The composite is −2.8 $m^2\ s^{-2}$ raw and −2.4 $m^2\ s^{-2}$ after detrending, so the trend accounts for roughly 15% and the decadal weakening is not a trend artefact.

We will revise the manuscript to discuss the regional structure: in the Pacific sector between 40 and 60°S the zonal wind component strengthens while the scalar speed is flat, so the winds there become more westerly without becoming faster. This difference likely comes from changes in the subseasonal (mostly synoptic) variability. We will correct an inconsistency elsewhere in the Discussion, where the same composite is described as showing a strengthening.

Lines 328-329: Sallee et al., (2021) find a deepening of the summer MLD. Is this relevant in this context?

We added discussion of Sallée et al. (2021) and how the reported summer MLD deepening relates to the composite MLD changes we find. We note that the secular trend in our record is also a deepening (of order +1 m per decade, area-weighted over the Southern Ocean), while the decadal composite is a shoaling, so the two are not in conflict: they describe different components of the variability.

Lines 330-332: So, the concomitant shoaling of the MLD cannot explain the stronger outgassing, correct? Interesting is that the largest MLD shoaling anomalies and the largest outgassing anomalies in the Pacific sector are not located in the same location (as seen in Fig. A1). Is this important? Did you consider increased upwelling as a possible mechanism causing cooling, MLD shoaling and higher CO2 outgassing? Since the CO2 outgassing anomalies are amplified at high latitudes of the Southern Ocean, did you consider changes in sea ice coverage?

We agree that MLD shoaling alone does not explain the increased outgassing, and we note the spatial offset between the largest shoaling and the largest outgassing anomalies in Fig. A1. Following the reviewer's suggestions we examined both upwelling and sea ice.

Upwelling is part of the answer, but only regionally. In the Pacific sector between 50 and 60°S the zonal wind stress strengthens, Ekman upwelling strengthens (of order +3 m yr^{-1}), SST cools, and $\Delta fCO_2'$ is elevated (roughly +4 µatm), so increased upwelling plausibly contributes to the concurrent cooling, shoaling and outgassing the reviewer identifies. South of 60°S, however, Ekman pumping weakens (by roughly 3 to 5 m yr^{-1}) while $\Delta fCO_2'$ is largest (roughly +6 to +7 µatm), so enhanced upwelling cannot account for the high-latitude outgassing.

On the mixed layer specifically we will be careful not to over-interpret. MLD shoals over roughly 78% of the Southern Ocean in the composite, in every sector and latitude band, including where the winds and the Ekman pumping both weaken. Its spatial pattern correlates only weakly with both the Ekman pumping composite and the u_{10}^2 composite ($|r| < 0.1$ region-wide), so we do not find evidence that the shoaling is a local response to either. This is consistent with Li and England (2020), who attribute Southern Ocean mixed layer variability to a remote Pacific teleconnection rather than to local forcing, and we will frame it that way.

On sea ice: because the ice fraction used in the LDEO-HPD flux calculation enters FCO_2 through the (1-fice) scaling, compositing that field is not an independent test. We repeated the composite with ORAS5 sea ice concentration and obtained the same result (Southern Ocean mean change of +0.004 in both products; spatial correlation 0.78; close agreement between 60 and 70°S). Sea ice does increase at the latitudes where the outgassing anomaly is largest, and we now discuss it as a candidate mechanism, noting that ORAS5 is not fully independent of the Hadley lineage.

Taken together, the high-latitude signal remains unexplained by the physical supply of DIC: both the Ekman upwelling and the mixed layer changes act to reduce it during the phase in which the outgassing increases. We now set this out as an open problem, with reduced biological drawdown and accumulation within a smaller mixed layer volume (Fassbender et al., 2017) as the remaining candidates.

Lines 336-344: the discussion on the role of the biological pump is not clear to me. Could you streamline your message so that it can better help to interpret the results found here?

We have streamlined the biological-pump discussion and restrict its argument to where the evidence supports it. The revised paragraph will make a single point about the high latitudes: south of 60°S, and outside the Pacific sector, the physical supply of DIC to the surface decreases during the phase in which outgassing increases - Ekman upwelling weakens and mixed layers shoal - so the rise in surface fCO_2 there is more plausibly explained either by reduced biological drawdown, itself consistent with reduced nutrient supply through the same weakened pathway, or by accumulation within a smaller mixed layer volume (Fassbender et al., 2017). We will note that this argument does not apply to the Pacific sector between 50 and 60°S, where enhanced Ekman upwelling offers a physical explanation for the outgassing, and we will then state that the absence of observational constraints on decadal productivity changes prevents us from distinguishing the remaining candidates at high latitudes.

Figures:
A1 (right): consider a tighter colobar axis to highlight the colors

We have tightened the color bar range in Fig. A1 (right)

I suggest to indicate in all captions which variables are detrended and which not.

We now indicate in each caption which variables are detrended and which are not. We agree this is important: as the exchange on L328-329 shows, whether u_{10}^2 retains its trend materially changes how the composite should be read.