Response to reviewer #1

This manuscript explores the ocean biogeochemical consequence of extended emissions scenarios up to 2500 using the MRI Earth system model. The analysis presented is robust, the manuscript is well-written, well referenced and adds an interesting long-term perspective to past publications which have overwhelmingly concentrated on 21st century perturbations. My comments are minor, typically focusing on text which could be clarified and additional references which could enhance the discussion in certain areas. Subject to these minor recommendations, I wholeheartedly support publication.

We thank the reviewer for the positive assessment of our study and for the many constructive suggestions.

Several comments highlighted the need for clearer explanations of the mechanisms underlying long-term changes in alkalinity, buffering capacity, and carbon uptake. In response, we have revised the manuscript to clarify the respective roles of physical and biogeochemical processes, particularly regarding alkalinity decline, nutrient redistribution, and carbonate chemistry changes.

We have also expanded the discussion by incorporating the suggested literature (e.g., Koven et al., 2022; Wilson et al., 2022; Terhaar et al., 2022), and by explicitly discussing model limitations associated with the absence of sediment carbonate feedbacks and ice-sheet freshwater forcing.

In addition, we improved figure references, clarified terminology (e.g., surface ocean vs. ocean), and revised several passages where the causal interpretation was unclear.

We believe these revisions have strengthened both the mechanistic interpretation and the broader context of our findings.

L15 Would be nice to mention the driver(s) of alkalinity declines.

We agree that briefly identifying the drivers of the simulated surface-ocean alkalinity decline would improve the clarity of the Abstract. We will therefore revise the text to indicate that enhanced upper-ocean stratification and a slowdown of the deep overturning circulation reduce the resupply of alkalinity-rich subsurface waters, leading to the simulated decline in surface-ocean alkalinity. The revised text will read:

"In parallel, enhanced upper-ocean stratification and a slowdown of the deep overturning circulation reduce the resupply of alkalinity-rich subsurface waters, leading to a decline in surface-ocean alkalinity that further enhances the influence of alkalinity on surface pCO2 during this period."

L65 I would rephrase this to make it clear the stratification and not the reduced buffering capacity that reduces alkalinity.

We have revised the sentence to clarify that enhanced upper-ocean stratification, rather than reduced buffering capacity, causes the decline in surface-ocean alkalinity. The revised sentence will read:

"Subsequent work shows that enhanced upper-ocean stratification reduces the upward transport of alkalinity-rich waters, leading to declining surface-ocean alkalinity and a subsequent reduction in carbon-system buffering capacity, thereby slowing CO_2 uptake in later centuries."

L68 Maybe explain why this is the case (computing costs) and that EMICs often do run this far but with potentially less biogeochemical realism.

We agree that providing additional context on the limited availability of simulations extending to 2500 would improve the Introduction. We will therefore revise the text to explain that such simulations remain relatively uncommon with comprehensive Earth system models because of their high computational cost, while Earth system models of intermediate complexity (EMICs) can often be integrated over these timescales but generally employ less realistic representations of marine biogeochemical processes. The revised text will read:

"Yet most studies do not go beyond 2100 because multi-centennial simulations with comprehensive Earth system models remain computationally expensive, and the mechanisms shaping centennial-to-millennial responses (2100–2500) therefore remain poorly constrained. Although Earth system models of intermediate complexity (EMICs) have been integrated over such timescales, they generally employ simplified representations of marine biogeochemical processes. "

L102-104 Perhaps complement this with the model TCRE if known.

We agree that reporting the transient climate response to cumulative car bon emissions (TCRE) would provide additional context for interpreting the climate response of MIROC-ES2L. We will therefore revise the text to include the model's TCRE together with a brief comparison with the likel y range assessed by the IPCC. The revised text will read:

"The equilibrium climate sensitivity (ECS) of this version of the model is 2.66 K, diagnosed using Gregory's method (Gregory et al., 2004) and prev iously reported (Tsutsui et al., 2020; Ohgaito et al. 2021). The transient cli mate response to cumulative carbon emissions (TCRE) of MIROC-ES2L is 1.3 °C per 1000 PgC (MacDougall et al., 2020; Hajima et al., 2020), which lie s within the likely range assessed by the IPCC. Relative to the ECS range of the CMIP6 models, the ECS of MIROC-ES2L is relatively modest."

L122-124 I think a citation or reference to a figure is needed to support th is statement.

We agree that this general statement should be supported by appropriat e references. We will therefore revise the text to cite previous review stu dies on climate tipping elements and irreversible climate-system respon ses. The revised text will read:

"Additionally, changes in GMST levels are associated with non-linear and potentially irreversible responses in the climate system, such as tipping points in sea-ice extent, abrupt changes in El Niño–Southern Oscillation (ENSO) behavior, cloud regimes, and polar climate dynamics, as well as ir reversibility in the Atlantic Meridional Overturning Circulation (AMOC) (Lenton et al., 2008; Weijer et al. 2019); Armstrong McKay et al., 2022.)"

L140-149 I think this excessive detail given this is quite fundamental kno wledge.

We agree that the original description was unnecessarily detailed. We wil l therefore shorten this section by removing the explanatory discussion of the Revelle factor and its typical values, while retaining only the defini tions of the Revelle factor and the buffering-capacity metrics used in the subsequent analysis.

L179-180 The logic here is confusing. Wouldn't declining nutrients in dee p water formation regions mean greater upper ocean nutrient availabilit y (less loss to depth).

We thank the reviewer for identifying this unclear explanation. We agree that the original wording could be interpreted as implying that reduced n utrient transport to the deep ocean would increase nutrient availability i n the upper ocean. Our intention was instead to describe the reduced res upply of nutrients to the surface ocean associated with a weakening of th e AMOC. We will revise the text to clarify this mechanism and add a figur e illustrating the relationship between the AMOC and marine productivit y (NPP and export production) to support this interpretation. The revise d text will read:

"The temporal evolution of detritus export is influenced by a complex interplay of environmental factors, including temperature, light availability, and nutrient concentrations, while the basin-scale distributions of these variables are reshaped by changes in ocean circulation. In particular, a weakening of the AMOC (Fig. A5) reduces the upward resupply of nutrients to the North Atlantic surface ocean, leading to lower surface macronutrient concentrations (Fig. A4) and a suppression of detritus export (Fig. xxx)"

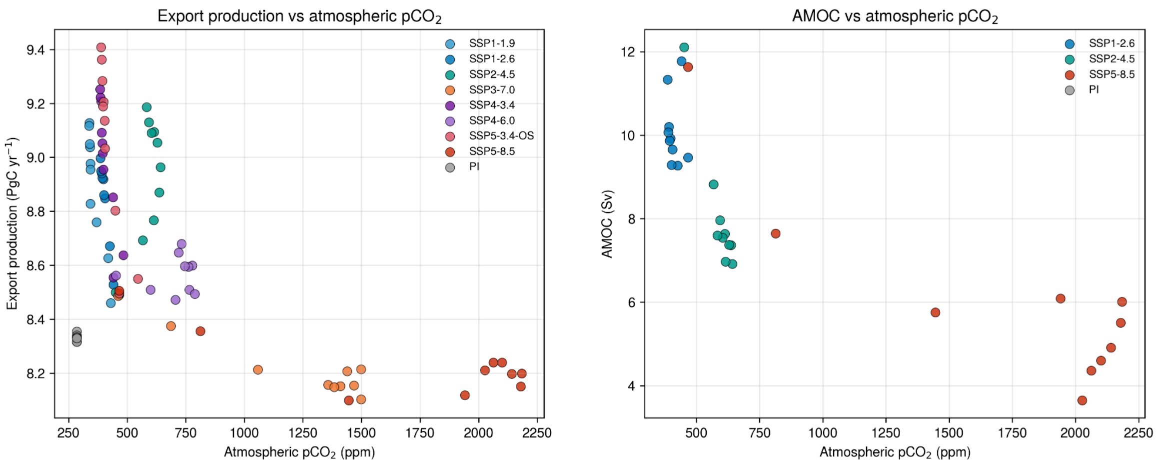

Figure R1. Scatter plots showing the relationships between atmospheric CO2 concentration and (a) global export production (Pg C yr^{-1}) for all emission scenarios analyzed in this study, and (b) Atlantic Meridional Overturning Circulation (AMOC) strength (Sv) under the SSP1–2.6, SSP2–4.5, and SSP5–8.5 scenarios.

L202-203 Is this confirmed by simulated fluxes? Presumably so if changes in winds are minimal.

We confirmed that the simulated air-sea CO_2 fluxes are consistent with the interpretation based on (ΔpCO_2), showing an intensification of ocean CO_2 uptake until approximately 2080 followed by a subsequent decline. We will revise the manuscript to explicitly state this and add a figure showing the simulated air–sea CO_2 fluxes. We will also examine changes in 10 m wind speed to clarify the possible contribution of gas-exchange changes. The revised text will read:

"This interpretation is supported by the simulated air–sea CO_2 fluxes, which show that ocean CO_2 uptake intensifies until approximately 2080 before gradually weakening thereafter. Although regional changes in 10 m wind speed occur, particularly over the Southern Ocean, the temporal evolution of the area-mean air–sea CO_2 flux is consistent with that inferred from ΔpCO_2."

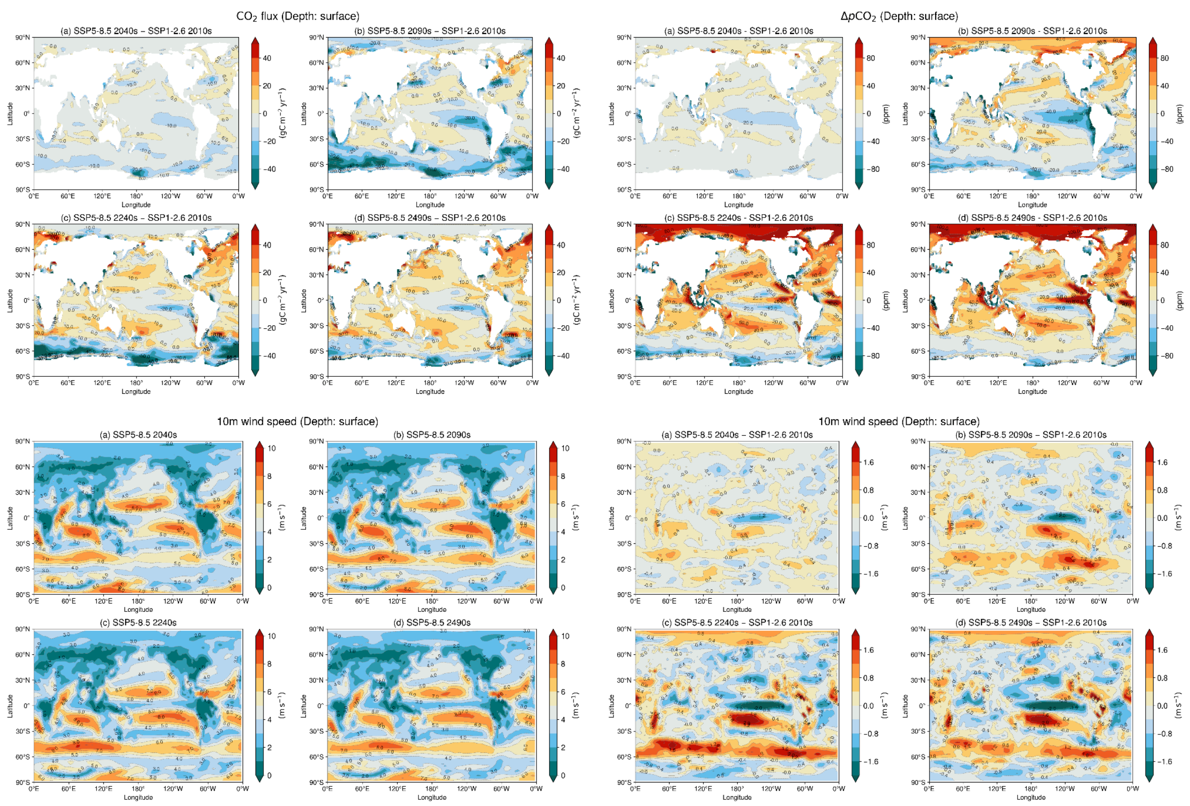

Figure R2. Sea-surface distributions of (a) CO_2 flux and (b) ΔpCO_2 for the 2040s, 2090s, 2240s, and 2490s under the SSP5-8.5 scenario, shown as differences relative to the 2010s under the SSP1-2.6 scenario. Panels (c) show the 10 m wind speed under the SSP5-8.5 scenario for the same four periods, and panels (d) show the corresponding differences in 10 m wind speed relative to the 2010s under the SSP1-2.6 scenario.

L210 Does the Southern Ocean therefore remain a sink? Interestingly under concentration-driven overshoot scenarios the Southern Ocean tends to switch form sink to source in multi-model assessments (Koven et al., 2022). This difference in model behavior should probably be discussed.

We thank the reviewer for drawing our attention to Koven et al. (2022). We compared our results with the multi-model assessment presented in that study and found that our simulated Southern Ocean response is broadly consistent with their results. Specifically, the transition from a carbon sink to a carbon source was primarily reported for concentration-driven overshoot scenarios, whereas the Southern Ocean generally remained a carbon sink under high-emission scenarios without declining atmospheric CO_2. We will therefore revise the manuscript to clarify this distinction and discuss how differences in the temporal evolution of atmospheric CO_2 concentrations between overshoot and continuously increasing forcing scenarios influence the long-term Southern Ocean carbon balance. The revised text will read:

"Despite these widespread increases in ΔpCO_2 elsewhere, the Southern Ocean (south of 30°S) consistently maintains negative ΔpCO_2 until the 2

5th century under SSP5-8.5. Accordingly, the Southern Ocean continues to act as a carbon sink throughout the simulation. This behavior is broadly consistent with the multi-model results of Koven et al. (2022), in which a transition from a carbon sink to a carbon source occurred primarily under concentration-driven overshoot scenarios, whereas the Southern Ocean generally remained a carbon sink under high-emission scenarios without declining atmospheric CO2. This suggests that the long-term Southern Ocean carbon balance depends not only on the magnitude of anthropogenic forcing but also on the temporal evolution of atmospheric CO2 concentrations."

L230 Should the p in pCO2 be capitalized here to represent potential pCO2?

We will revise the notation by changing "pCO2" to "PCO2" where appropriate.

L237 Rephrase "neutralizing increases in DIC". Should DIC be "acidity"?

We agree that the original wording was imprecise. We will revise the text to clarify that seawater carbonate chemistry buffers the increase in acidity associated with elevated DIC, rather than neutralizing the increase in DIC itself. The revised text will read:

"The ocean plays a buffering role in the global carbon cycle by absorbing CO2 and, through seawater carbonate chemistry, buffering the increase i n acidity associated with elevated DIC, thereby mitigating the accumulation of CO_2 in the atmosphere."

L242 Although closely related, I don't think this is exactly the inverse as it has units (umol kg-1/uatm) unlike 1/R.

We agree that the original wording was not strictly correct because the buffering coefficient retains physical units and is therefore not mathematically equivalent to the inverse of the Revelle factor. We will revise the text to describe the buffering coefficient as a closely related metric rather than the inverse of the Revelle factor. The revised text will read:

"To assess the buffering capacity of the surface ocean carbon system, we examine the long-term evolution of the Revelle factor ($\partial \ln pCO_{2ocn} / \partial \ln DIC$), a widely used indicator of the ocean's resistance to changes in CO_2. We focus primarily, however, on the buffering coefficient ($\partial DIC / \partial pCO_{2ocn}$), which is closely related to the Revelle factor and provides a more direct measure of the ocean's capacity to absorb additional CO_2."

L246 Here and elsewhere be explicit when you are referring to the surface ocean.

We agree that the distinction was not always sufficiently clear. We will revise the manuscript throughout to explicitly refer to the surface ocean where appropriate and improve the consistency of the terminology.

L246-255 One wonders what are the respective roles of DIC vs. alkalinity in the differences in buffering capacity recovery. Presumably this is near ly all DIC driven?

We agree that the respective roles of DIC and alkalinity in the recovery o f buffering capacity should be clarified. We will revise the discussion to e xplicitly state that the recovery of buffering capacity is primarily associ ated with the evolution of surface-ocean DIC, while changes in surface-o cean alkalinity provide a secondary contribution. The revised text will al so refer readers to the subsequent analysis, where the relative influence s of DIC and alkalinity on carbonate-system buffering are examined in m ore detail.

"… The carbonate system therefore gradually re-equilibrates. As discusse d in a later section, this recovery is primarily associated with changes in surface-ocean DIC, while changes in surface-ocean alkalinity further mo dulate the recovery."

L255 Maybe mention that there is still some regional variability in buffer ing capacity in SSP126 but this disappears in SSP585.

We agree that the contrasting evolution of the spatial variability in buffe ring capacity between SSP1–2.6 and SSP5–8.5 is an important feature of t he simulations. We will revise the manuscript to explicitly note that regi onal variability persists under SSP1–2.6, whereas under SSP5–8.5 the muc h larger accumulation of anthropogenic carbon causes buffering capacit y to approach its effective minimum across most of the global surface oc ean, thereby largely eliminating regional differences. The revised text will read:

"...This indicates a state in which carbonate-system buffering is strongly diminished as continued accumulation of anthropogenic carbon drives buffering capacity toward its effective minimum across most of the global surface ocean. As buffering capacity approaches this effective minimum, regional contrasts become progressively weaker, resulting in a nearly homogeneous spatial distribution by around 2200."

L270 I think "in contrast" should be "similarly".

We agree that "Similarly" is more appropriate than "In contrast" here.

L268-277 Does this surface ocean alkalinity decline occur despite reductions in PIC export? i.e. is physics dominating biotic effects? It is worth mentioning here or later in the discussion on model limitations whether sediment carbonate dissolution is possible (presumably not). On these timescales, one might expect a non-negligible benthic alkalinity flux associated with such dissolution.

We agree that the relative roles of physical and biogeochemical processes in driving the simulated surface-ocean alkalinity decline should be clarified. We will therefore revise the Results section to explicitly note that, although calcium carbonate export decreases in the simulations, surface-ocean alkalinity nevertheless declines because enhanced upper-ocean stratification and a weakening of the overturning circulation reduce the res

upply of alkalinity-rich subsurface waters to the surface ocean, overwhel
ming the alkalinity-conserving effect of reduced carbonate export. The r
evised text will read:

"After 2200, however, the influence of reductions in surface alkalinity, to
gether with thermal effects, becomes increasingly important in driving f
urther increases in surface pCO_2. This influence is associated with enha
nced upper-ocean stratification and a weakening of the meridional overt
urning circulation thereafter. Although calcium carbonate export decrea
ses during this period, surface-ocean alkalinity nevertheless declines, in
dicating that these physical changes dominate over the alkalinity-conser
ving effect associated with reduced carbonate export."

We will also revise the "Future challenges and uncertainty" section to ex
plicitly discuss the absence of sediment carbonate dissolution and benth
ic alkalinity feedbacks in the model as a limitation. The added text will re
ad:

"Another limitation of the present model framework is the omission of s
ediment carbonate dissolution and benthic alkalinity feedbacks. Althoug
h these processes are expected to become increasingly important on cen
tennial-to-millennial timescales, they are not represented in the present
model. Sediment carbonate dissolution supplies alkalinity to the ocean a
nd can partially offset ocean acidification and the long-term decline in su
rface-ocean buffering capacity. Consequently, the simulated reduction in
surface-ocean alkalinity and buffering capacity may be somewhat overes
timated on multi-centennial timescales."

L290-291 Does export production decline though, as seen in most CMIP6 models and therefore is it remineralization associated with increasing water mass age which is dominating ? (e.g. Wilson et al., 2022).

We thank the reviewer for this insightful comment and for drawing our attention to Wilson et al. (2022). We agree that biological export generally decreases under the high-emission scenario, consistent with previous CMIP6 studies. To clarify the mechanisms responsible for the increase in interior DIC, we will revise the manuscript to distinguish the contributions of preformed and regenerated carbon. We will also add the temporal evolution of preformed and regenerated carbon to Fig. 2. The revised text will read:

"Although export production generally decreases under SSP5–8.5 (Fig. 5), regenerated carbon nevertheless increases because longer water-mass residence times enhance the accumulation of remineralized carbon in the ocean interior. The increase in interior DIC is, however, dominated by preformed carbon, with regenerated carbon providing an additional contribution (Fig. 2)."

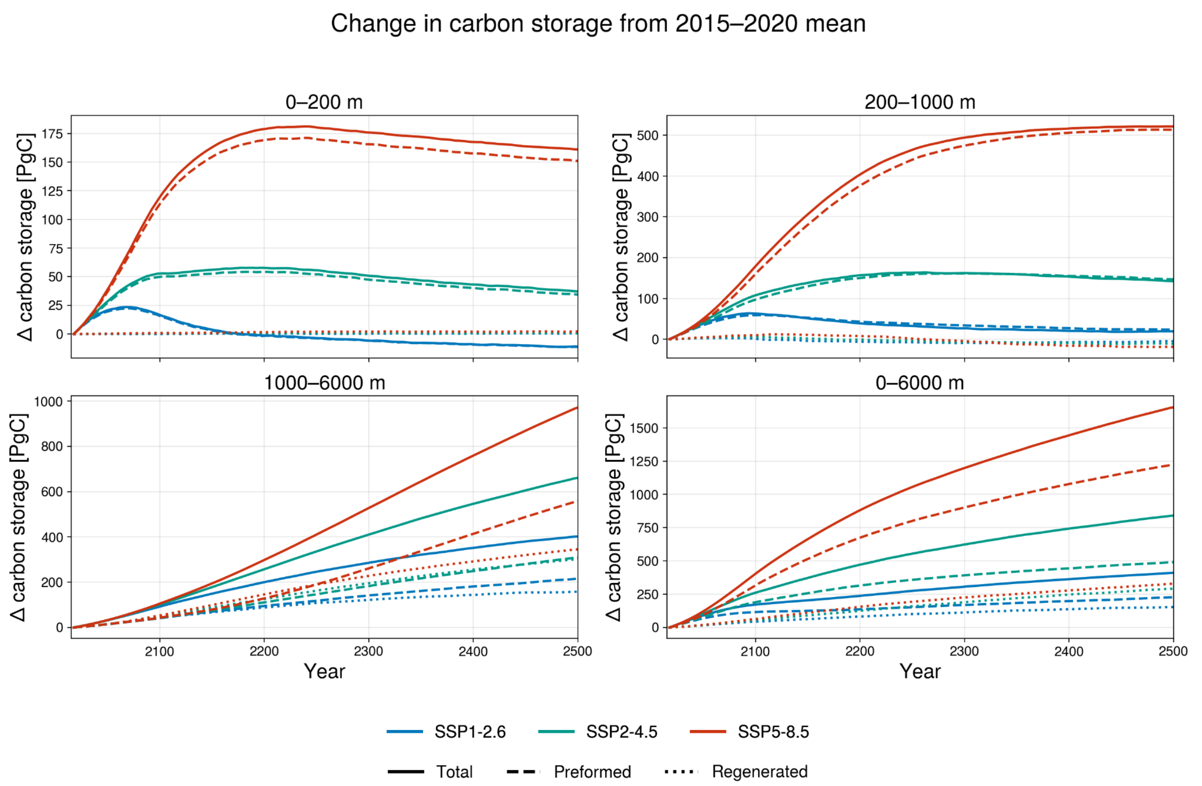

Figure R3. Time evolution of changes in ocean carbon storage (PgC) under the SSP1–2.6, SSP2–4.5, and SSP5–8.5 scenarios for the (a) 0–200 m, (b) 200–1000 m, (c) 1000–6000 m, and (d) 0–6000 m depth ranges. Solid lines denote total carbon storage, dashed lines denote preformed carbon storage, and dotted lines denote regenerated carbon storage. Regenerated carbon was estimated from apparent oxygen utilization, and preformed carbon was obtained as the residual.

L322 Are undersaturation metrics with respect to the surface ocean only? Clarify this.

Aragonite and calcite undersaturation are calculated for the full ocean, whereas the discussion in this section focuses on changes in the surface ocean because they are most directly relevant to ecosystem stress. We wil

l revise the manuscript to clarify this distinction. The revised text will read:
"This intensification is accompanied by pronounced surface-ocean acidification on a global scale."

L379 Maybe the relative pCO2atm decline with respect to increase should be mentioned here. This relative decline is much greater in SSP126 than SSP585

We agree that the contrasting evolution of atmospheric pCO2 between the two scenarios is important for interpreting the differences in long-term ocean CO2 uptake. We will revise the manuscript to explicitly note that atmospheric pCO2 stabilizes and declines much more rapidly under SSP1–2.6 than under SSP5–8.5, leading to a more rapid reduction in the ocean–atmosphere pCO2 gradient. The revised text will read:

"As pCO2atm stabilizes and declines much more rapidly under SSP1–2.6 than under SSP5–8.5, the negative ocean–atmosphere pCO2 difference progressively decreases, indicating a more rapid reduction in the chemical potential for further ocean CO2 uptake."

L380-381 Would be useful to compare with Koven et al. (2022) here.

We agree that comparison with Koven et al. (2022) provides useful context for interpreting the long-term evolution of the Southern Ocean carbon sink. We will revise the manuscript to compare our results with their multi-model assessment and clarify that the persistence of the Southern O

cean carbon sink in our SSP5–8.5 simulation is consistent with their resul
ts for non-overshoot high-emission scenarios. The revised text will read:

"The Southern Ocean continues to act as a major sink, consistent with the
multi-model assessment of Koven et al. (2022), in which the Southern Oc
ean generally remained a carbon sink under high-emission scenarios wit
hout declining pCO2atm."

*L383 This wording "as expressed as a decline…" doesn't work in light of th
e previous sentence.*

We agree that the original wording was awkward and did not connect pro
perly with the preceding sentence. We will revise the text to improve the
logical flow and explicitly relate the decline in buffering capacity to the i
ncrease in the Revelle factor. The revised text will read:

"As anthropogenic carbon accumulates, the buffering capacity of the car
bonate system declines, particularly under SSP5–8.5. This decline corres
ponds to an increase in the Revelle factor, indicating heightened sensitiv
ity of surface pCO2ocn to additional increases in DIC."

L389 and 391 Can you say "reductions" instead of "changes"?

We agree that "reductions" more accurately describes the behavior discu
ssed in this section. The text has been revised accordingly.

L398-399 It might be worth reelecting on biogeochemical drivers of alkalinity anomalies here.

We agree that the discussion would benefit from considering the biogeochemical drivers of the simulated alkalinity anomalies in addition to the physical mechanisms. We will revise the manuscript to note that changes in calcium carbonate export may also influence surface-ocean alkalinity, but that our results indicate the dominant control is exerted by changes in ocean circulation and upper-ocean stratification. The revised text will read:

"Although changes in calcium carbonate export may also contribute to regional alkalinity anomalies, the present simulations suggest that the dominant driver of the long-term surface-ocean alkalinity decline is the reduced resupply of alkalinity-rich subsurface waters associated with enhanced upper-ocean stratification and changes in overturning circulation. However, the circulation pathways..."

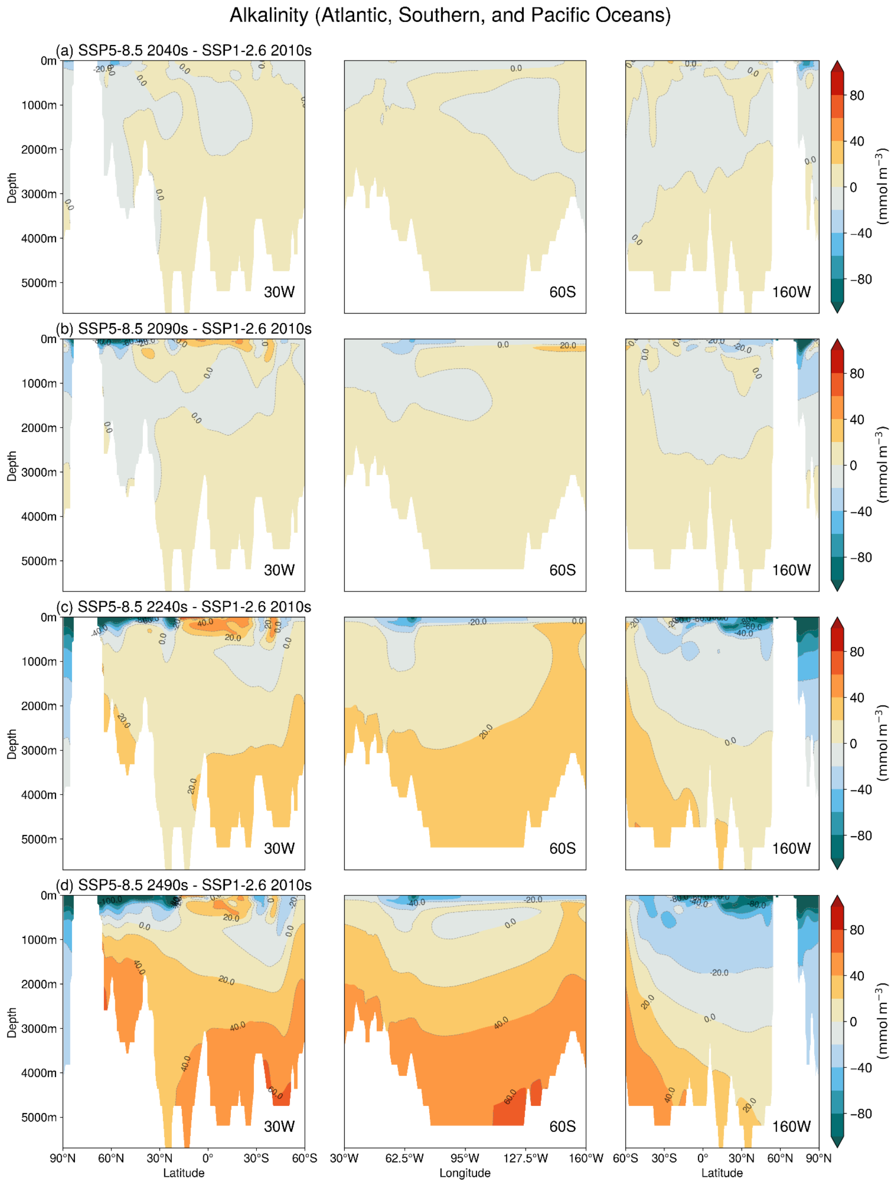

Figure R4. Differences in alkalinity between the SSP5–8.5 scenario (2040s, 2090s, 2240s, and 2490s) and the SSP1–2.6 2010s. Panels show Atlantic meridional sections, Southern Ocean zonal sections, and Pacific meridional sections.

L415 Not sure it makes sense to call the carbonate system CO_2 dominated as CO_2 will still be <<10% of DIC with nearly everything presumably bicarbonate.

We agree that the original wording was imprecise and could be interpreted as implying that dissolved CO_2 becomes the dominant DIC species. We will revise the manuscript to avoid this implication and instead describe the transition in terms of the declining buffering capacity of the carbonate system. The revised text will read:

"This convergence marks a transition toward a weakly buffered carbonate-system regime, in which further increases in DIC generate disproportionately large increases in surface pCO_2ocn. As buffering weakens, ..."

L424 Could also mention the absence of sediment feedbacks here.

We agree that the absence of sediment carbonate dissolution and associated benthic alkalinity feedbacks should also be noted in this context. We will revise the manuscript to explicitly identify this process as an additional limitation of the model and discuss its potential implications for long-term projections. The revised text will read:

"... changes in $CaCO_3$ production and dissolution. In addition, the model does not include sediment carbonate dissolution or associated benthic alkalinity feedbacks, which could partially offset surface-ocean alkalinity declines and modify the long-term evolution of ocean buffering capacity and carbon uptake on multi-centennial timescales. Recent work suggests that ..."

L444-445 This is a little confusing. Please clarify what is meant by this decoupling.

We agree that the term "decoupling" was not sufficiently clear in the original manuscript. We will revise the text to explicitly explain that, despite continued accumulation of carbon in the deep ocean, deep-ocean PCO2 remains well below atmospheric pCO2 because deep-ocean ventilation cannot keep pace with the rapid rise in atmospheric CO2. Consequently, the deep ocean retains substantial capacity for further carbon uptake. The revised text will read:

"This reflects the rapid rise in atmospheric pCO2 outpacing deep-ocean ventilation, such that the deep ocean remains far from chemical equilibrium with the atmosphere despite continued DIC accumulation. Consequently, the deep ocean retains substantial capacity for further CO2 uptake, highlighting that carbon storage and chemical equilibration proceed on different timescales under sustained high emissions."

L447 "Ice sheet derived" freshwater forcing? What follows this is also a bit confusing. Is this a consequence of enhanced remineralization in these older waters?

We agree that the original wording was not sufficiently clear. The freshwater forcing referred to here is specifically freshwater input from ice-sheet melt, which is not included in the present simulations. We will revise the manuscript to make this explicit and clarify that the expected enhancement of deep-ocean carbon storage arises primarily from reduced AABW formation and weakened deep-ocean ventilation, rather than from enhanced remineralization associated with older water masses. The revised text will read:

"Freshwater input from ice-sheet melt, which is not included in the present simulations, is expected to amplify the long-term impact of anthropogenic carbon storage on seawater chemistry by reducing AABW formation and deep-ocean ventilation. The resulting increase in deep-water residence time would prolong the isolation of carbon-rich waters from the atmosphere, reinforcing deep-ocean acidification (Lago and England, 2019; Chen et al., 2023)."

L452-455 I'm not sure I have seen robust increases in export even under high mitigation scenarios in multi-model comparisons (e.g. Wilson et al., 2022). The novelty in these projections should be highlighted.

We thank the reviewer for this insightful comment and for referring us to Wilson et al. (2022). We agree that the projected increase in export production under the low-emission scenarios differs from the general behavior reported in previous CMIP6 multi-model assessments, and that the novelty of this result should be highlighted more clearly. We will revise the discussion to emphasize that the long-term simulations reveal a transient response that is not apparent from end-of-century analyses alone. In particular, export production responds rapidly during the 21st century and closely follows the evolution of the AMOC. The transition from increasing to decreasing export occurs once atmospheric CO_2 exceeds approximately three times the pre-industrial level, when the weakening of the AMOC becomes more pronounced under stronger forcing. We are also considering adding a figure showing the temporal evolution of export produ ction together with AMOC strength to illustrate this relationship more clearly. The revised text will read:

"Simulation results for detritus export at the depth of 100 m vary substantially among emission scenarios. Detritus export exhibits a long-term increase under SSP1–2.6 and SSP2–4.5, in contrast to the decline projected under SSP3–7.0 and SSP5–8.5 (Fig.~\ref{fig:fzd100_PgC}). Most of this divergence develops during the 21st century and closely follows the rapid adjustment of the large-scale ocean circulation. Export production declines as the weakening of the AMOC becomes more pronounced under stronger forcing; this transition occurs when atmospheric CO2 exceeds approximately three times the pre-industrial level (Fig. xxx). Consequently, the long-term behavior is largely established by the end of the 21st century and remains comparatively stable thereafter, a feature that is difficult to identify from end-of-century analyses alone. Regionally, detritus export decreases across the high-latitude North Atlantic under all scenarios, with the largest decline occurring under SSP5–8.5, consistent with the strongest weakening of the AMOC."

L468 "they" typo

We have corrected "they" to "their" in the revised manuscript.

L544 Or an overestimation. Greenland hosing simulations have been shown to reduce ocean carbon uptake (e.g. Swingedouw et al., 2007).

We thank the reviewer for this important comment and for drawing our attention to Swingedouw et al. (2007). We agree that the sign of the bias introduced by the omission of ice-sheet freshwater forcing is not straightforward and may depend on the source of the freshwater perturbation and the associated circulation response. We will therefore revise the manuscript to avoid implying a one-sided bias and instead emphasize that the omission of dynamic ice-sheet freshwater forcing represents an important source of uncertainty in long-term projections. The revised text will read:

"... its influence on buoyancy-driven overturning (Rahmstorf et al., 2002). Recent studies have shown that Antarctic ice sheet melt can weaken AABW formation, alter Southern Ocean circulation, and substantially modify ocean carbon uptake and storage (Bronselaer et al., 2018; 2020), whereas freshwater perturbations from the Greenland Ice Sheet have been shown to reduce ocean carbon uptake through their influence on the AMOC (Swingedouw et al., 2007). The omission of dynamic freshwater forcing in the present study therefore represents an important source of uncertainty in projections of long-term ocean carbon storage and acidification. Future modeling efforts ..."

L563-565 This is a feature of carbonate chemistry projections at depth but not in the global surface ocean where there is generally high model agreement for acidification projections with the exception of regions dominated by sea ice dynamics or riverine fluxes. I therefore suspect the impact on ocean carbon uptake to be minimal. That being said, a general over *estimation of Revelle factor has been shown to low bias CMIP6 ocean carbon uptake simulations (Terhaar et al., 2022).*

We thank the reviewer for this helpful comment and for drawing our attention to Terhaar et al. (2022). We agree that projections of surface-ocean acidification are generally robust across models, except in regions strongly influenced by sea-ice processes or riverine inputs. We will revise the discussion to distinguish the relatively high confidence in surface-ocean acidification projections from the greater uncertainty associated with carbonate-system buffering capacity and its implications for long-term ocean carbon uptake. We will also cite Terhaar et al. (2022), who showed that an overestimation of the Revelle factor can lead to an underestimation of ocean carbon uptake in CMIP6 models. The revised text will read:

"… ocean buffering capacity. Although projections of surface-ocean acidification are generally robust across Earth system models, substantially greater uncertainty remains in the timing and magnitude of changes in carbonate-system buffering capacity and the resulting limits on long-term ocean carbon uptake (Orr et al., 2005; Egleston et al., 2010). In particular, biases in the simulated Revelle factor have been shown to contribute to biases in projected ocean carbon uptake in CMIP6 models (Terhaar et al., 2022). At the same time, ocean acidification poses significant risks to marine ecosystems, particularly calcifying organisms (Doney et al., 2009). "