Response to reviewer #1

This manuscript explores the ocean biogeochemical consequence of exte nded emissions scenarios up to 2500 using the MRI Earth system model. The analysis presented is robust, the manuscript is well-written, well ref erenced and adds an interesting long-term perspective to past publicatio ns which have overwhelmingly concentrated on 21st century perturbatio ns. My comments are minor, typically focusing on text which could be cla rified and additional references which could enhance the discussion in c ertain areas. Subject to these minor recommendations, I wholeheartedly support publication.

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

Several comments highlighted the need for clearer explanations of the m echanisms underlying long-term changes in alkalinity, buffering capacit y, and carbon uptake. In response, we have revised the manuscript to clar ify the respective roles of physical and biogeochemical processes, partic ularly regarding alkalinity decline, nutrient redistribution, and carbonate chemistry changes.

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

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

We believe these revisions have strengthened both the mechanistic inter pretation 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-oc ean alkalinity decline would improve the clarity of the Abstract. We will t herefore revise the text to indicate that enhanced upper-ocean stratifica tion and a slowdown of the deep overturning circulation reduce the resu pply of alkalinity-rich subsurface waters, leading to the simulated declin e in surface-ocean alkalinity. The revised text will read:

"In parallel, enhanced upper-ocean stratification and a slowdown of the d eep overturning circulation reduce the resupply of alkalinity-rich subsu rface 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 re duced buffering capacity that reduces alkalinity.

We have revised the sentence to clarify that enhanced upper-ocean strati fication, rather than reduced buffering capacity, causes the decline in sur face-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 CO2 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 int erplay 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 w eakening of the AMOC (Fig. A5) reduces the upward resupply of nutrient s to the North Atlantic surface ocean, leading to lower surface macronut rient concentrations (Fig. A4) and a suppression of detritus export (Fig. xxx)"

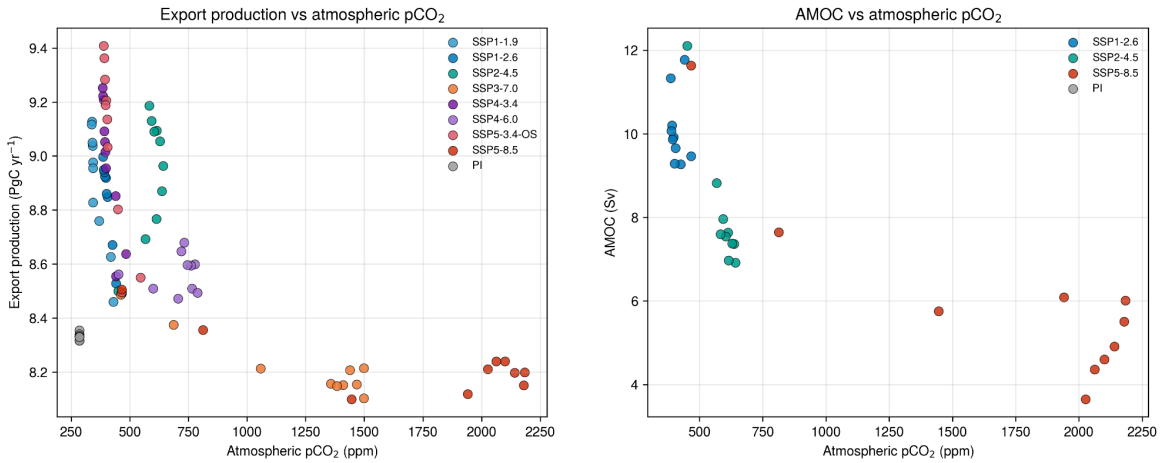


Figure R1. Scatter plots showing the relationships between atmospheric CO2 concentration and (a) global export production (Pg C yr^{-1}) for all emi ssion scenarios analyzed in this study, and (b) Atlantic Meridional Overt urning Circulation (AMOC) strength (Sv) under the SSP1–2.6, SSP2–4.5, an d SSP5–8.5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

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

We confirmed that the simulated air-sea CO2 fluxes are consistent with t he interpretation based on (Δ pCO2), showing an intensification of ocean CO2 uptake until approximately 2080 followed by a subsequent decline. We will revise the manuscript to explicitly state this and add a figure sho wing the simulated air–sea CO2 fluxes. We will also examine changes in 1 0 m wind speed to clarify the possible contribution of gas-exchange chan ges. The revised text will read:

"This interpretation is supported by the simulated air–sea CO2 fluxes, w hich show that ocean CO2 uptake intensifies until approximately 2080 b efore gradually weakening thereafter. Although regional changes in 10 m wind speed occur, particularly over the Southern Ocean, the temporal ev olution of the area-mean air–sea CO2 flux is consistent with that inferre d from Δ pCO2."

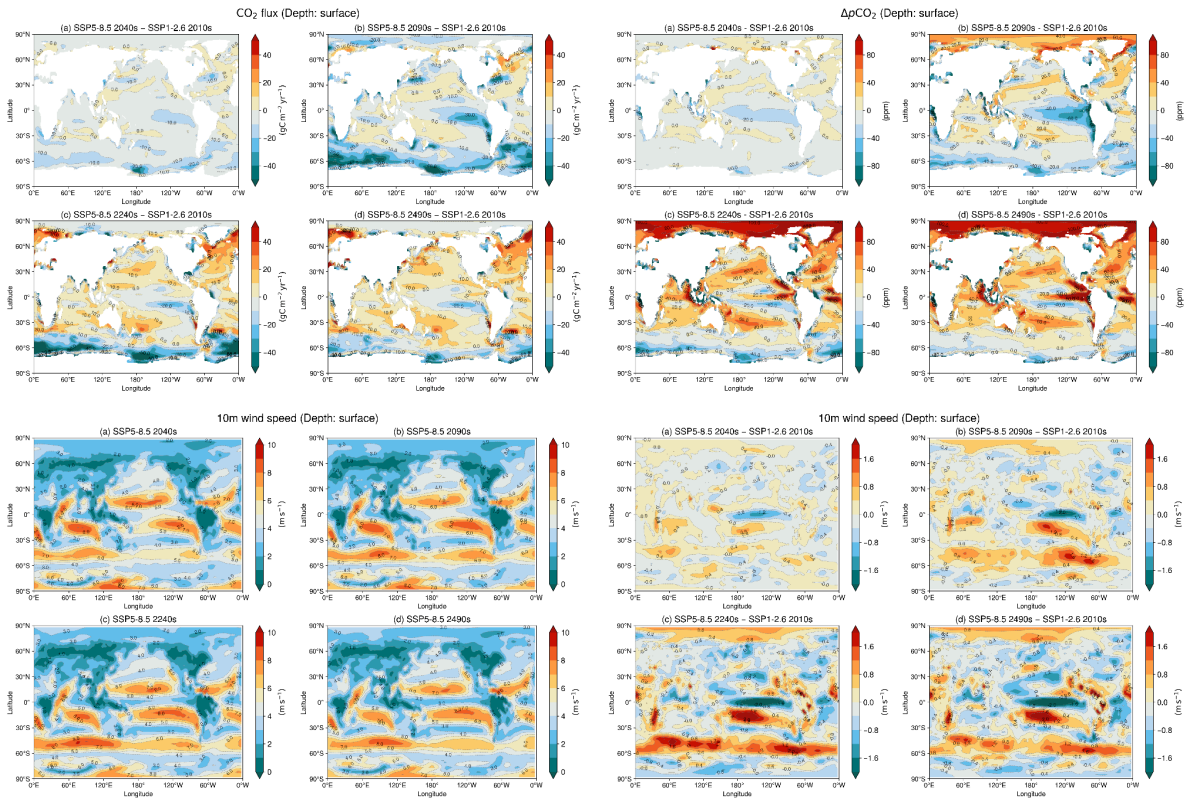


Figure R2. Sea-surface distributions of (a) CO2 flux and (b) Δ pCO2 for th e 2040s, 2090s, 2240s, and 2490s under the SSP5-8.5 scenario, shown as differences relative to the 2010s under the SSP1-2.6 scenario. Panels (c) s how the 10 m wind speed under the SSP5-8.5 scenario for the same four p eriods, and panels (d) show the corresponding differences in 10 m wind s peed relative to the 2010s under the SSP1-2.6 scenario.

L210 Does the Southern Ocean therefore remain a sink? Interestingly un der concentration-driven overshoot scenarios the Southern Ocean tends to switch form sink to source in multi-model assessments (Koven et al., 2 022). This difference in model behavior should probably be discussed.

We thank the reviewer for drawing our attention to Koven et al. (2022). W e compared our results with the multi-model assessment presented in th at study and found that our simulated Southern Ocean response is broad ly 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 atmospher ic CO2. We will therefore revise the manuscript to clarify this distinction and discuss how differences in the temporal evolution of atmospheric C O2 concentrations between overshoot and continuously increasing forci ng scenarios influence the long-term Southern Ocean carbon balance. Th e revised text will read:

"Despite these widespread increases in Δ pCO2 elsewhere, the Southern Ocean (south of 30°S) consistently maintains negative Δ pCO2 until the 2

5th century under SSP5-8.5. Accordingly, the Southern Ocean continues t o act as a carbon sink throughout the simulation. This behavior is broadl y 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 un der concentration-driven overshoot scenarios, whereas the Southern Oc ean generally remained a carbon sink under high-emission scenarios wit hout declining atmospheric CO2. This suggests that the long-term South ern Ocean carbon balance depends not only on the magnitude of anthrop ogenic forcing but also on the temporal evolution of atmospheric CO2 co ncentrations."

L230 Should the p in pCO2 be capitalized here to represent potential pCO 2?

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

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 acidi ty 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 accumulati on of CO2 in the atmosphere."

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

We agree that the original wording was not strictly correct because the b uffering coefficient retains physical units and is therefore not mathemat ically equivalent to the inverse of the Revelle factor. We will revise the te xt 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 (∂ ln pCO2ocn / ∂ l n DIC), a widely used indicator of the ocean's resistance to changes in CO 2. We focus primarily, however, on the buffering coefficient (∂ DIC / ∂ pC O2ocn), which is closely related to the Revelle factor and provides a more direct measure of the ocean's capacity to absorb additional CO2."

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

We agree that the distinction was not always sufficiently clear. We will re vise the manuscript throughout to explicitly refer to the surface ocean w here 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 wi ll read:

"...This indicates a state in which carbonate-system buffering is strongly diminished as continued accumulation of anthropogenic carbon drives b uffering 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 ho mogeneous 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 reducti ons in PIC export? i.e. is physics dominating biotic effects? It is worth me ntioning here or later in the discussion on model limitations whether sed iment carbonate dissolution is possible (presumably not). On these times cales, one might expect a non-negligible benthic alkalinity flux associate d with such dissolution.

We agree that the relative roles of physical and biogeochemical processe s in driving the simulated surface-ocean alkalinity decline should be clari fied. We will therefore revise the Results section to explicitly note that, al though calcium carbonate export decreases in the simulations, surface-o cean alkalinity nevertheless declines because enhanced upper-ocean str atification and a weakening of the overturning circulation reduce the res

upply of alkalinity-rich subsurface waters to the surface ocean, overwhelming the alkalinity-conserving effect of reduced carbonate export. The revised text will read:

"After 2200, however, the influence of reductions in surface alkalinity, together with thermal effects, becomes increasingly important in driving further increases in surface pCO2. This influence is associated with enhanced upper-ocean stratification and a weakening of the meridional overturning circulation thereafter. Although calcium carbonate export decreases during this period, surface-ocean alkalinity nevertheless declines, indicating that these physical changes dominate over the alkalinity-conserving effect associated with reduced carbonate export."

We will also revise the "Future challenges and uncertainty" section to explicitly discuss the absence of sediment carbonate dissolution and benthic alkalinity feedbacks in the model as a limitation. The added text will read:

"Another limitation of the present model framework is the omission of sediment carbonate dissolution and benthic alkalinity feedbacks. Although these processes are expected to become increasingly important on centennial-to-millennial timescales, they are not represented in the present model. Sediment carbonate dissolution supplies alkalinity to the ocean and can partially offset ocean acidification and the long-term decline in surface-ocean buffering capacity. Consequently, the simulated reduction in surface-ocean alkalinity and buffering capacity may be somewhat overestimated 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 w ater mass age which is dominating ? (e.g. Wilson et al., 2022).

We thank the reviewer for this insightful comment and for drawing our a ttention to Wilson et al. (2022). We agree that biological export generally decreases under the high-emission scenario, consistent with previous C MIP6 studies. To clarify the mechanisms responsible for the increase in i nterior DIC, we will revise the manuscript to distinguish the contribution s of preformed and regenerated carbon. We will also add the temporal ev olution 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-mas s residence times enhance the accumulation of remineralized carbon in t he ocean interior. The increase in interior DIC is, however, dominated by preformed carbon, with regenerated carbon providing an additional cont ribution (Fig. 2)."

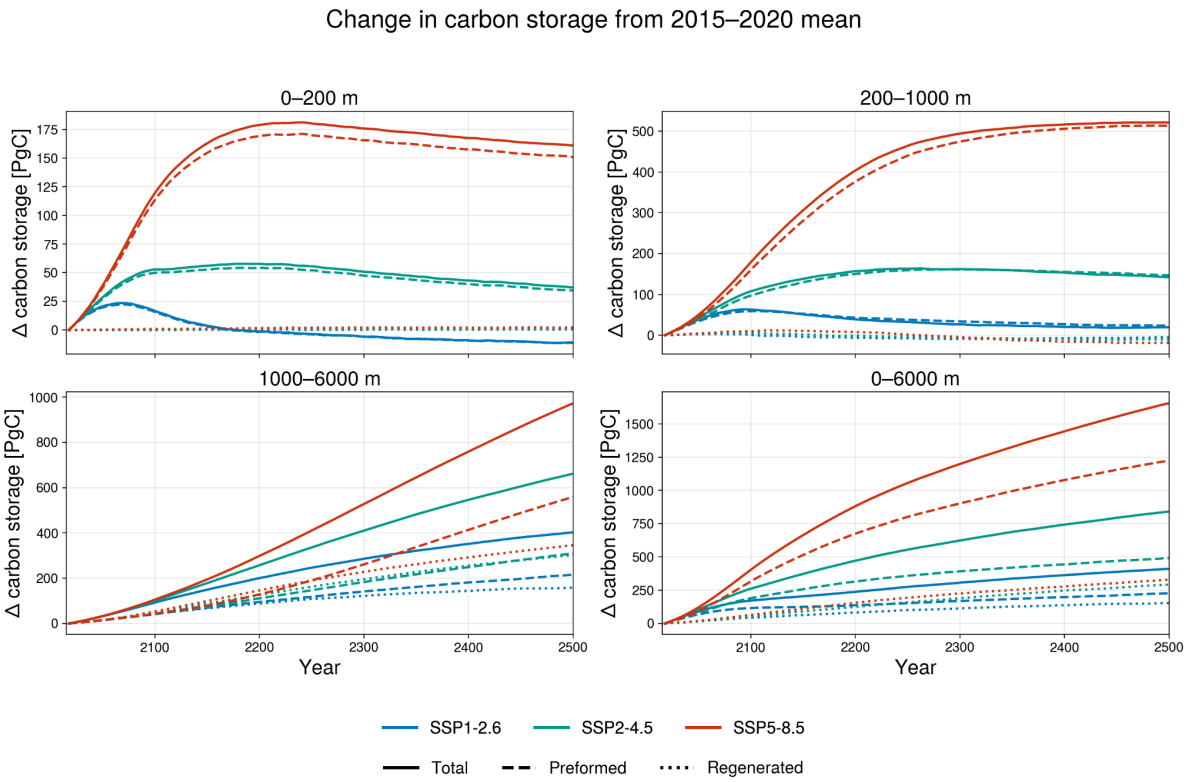


Figure R3. Time evolution of changes in ocean carbon storage (PgC) under the SSP1–2.6, SSP2–4.5, and SSP5–8.5 scenarios for the (a) 0–200 m, (b) 200–1000 m, (c) 1000–6000 m, and (d) 0–6000 m depth ranges. Solid lines denote total carbon storage, dashed lines denote preformed carbon storage, and dotted 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 re ad:

"This intensification is accompanied by pronounced surface-ocean acidif ication on a global scale."

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

We agree that the contrasting evolution of atmospheric pCO2 between th e two scenarios is important for interpreting the differences in long-ter m 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 t han under SSP5–8.5, the negative ocean–atmosphere pCO2 difference pr ogressively 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 conte xt for interpreting the long-term evolution of the Southern Ocean carbo n sink. We will revise the manuscript to compare our results with their m ulti-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 alkal inity anomalies here.

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

"Although changes in calcium carbonate export may also contribute to r egional alkalinity anomalies, the present simulations suggest that the do minant driver of the long-term surface-ocean alkalinity decline is the red uced resupply of alkalinity-rich subsurface waters associated with enha nced upper-ocean stratification and changes in overturning circulation. However, the circulation pathways..."

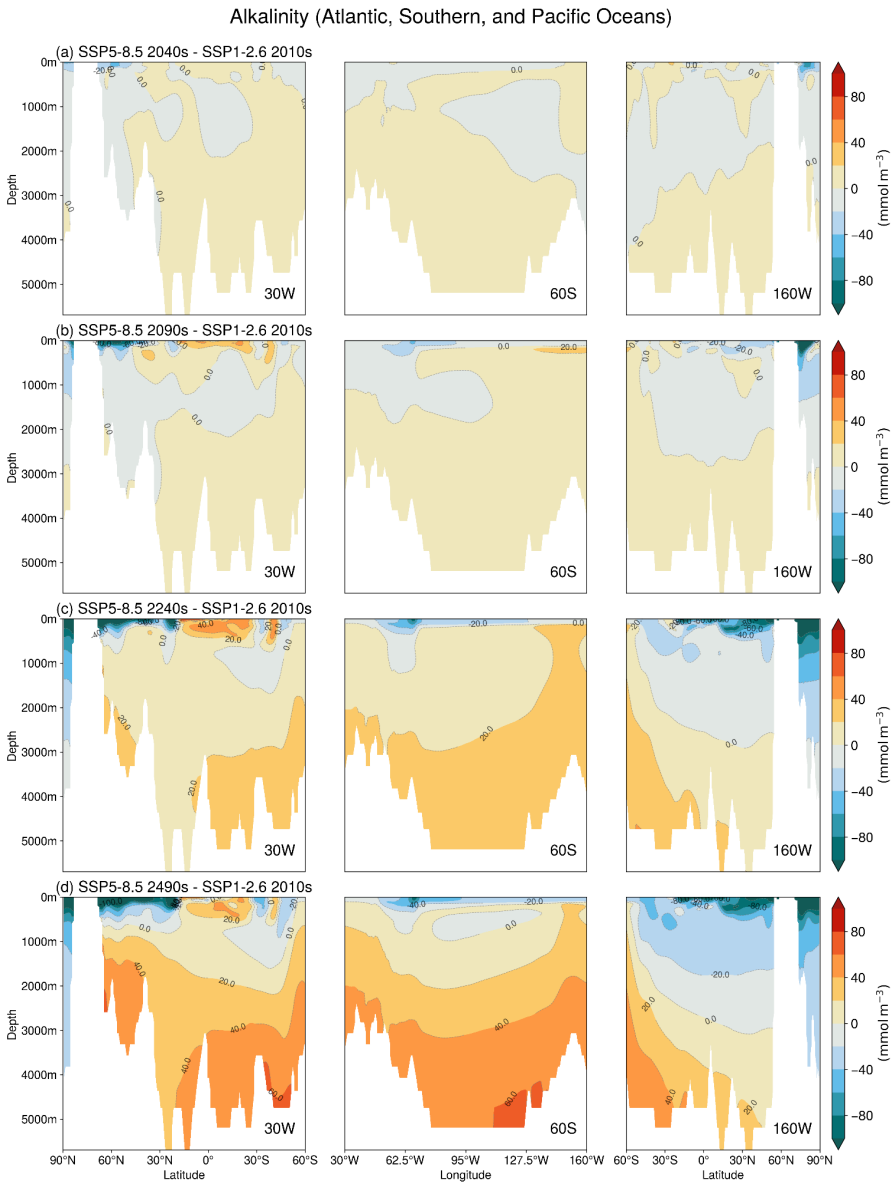


Figure R4. Differences in alkalinity between the SSP5–8.5 scenario (2040 s, 2090s, 2240s, and 2490s) and the SSP1–2.6 2010s. Panels show Atlantic meridional sections, Southern Ocean zonal sections, and Pacific meridion al sections.

L415 Not sure it makes sense to call the carbonate system CO2 dominate d as CO2 will still be <<10% of DIC with nearly everything presumably bic arbonate.

We agree that the original wording was imprecise and could be interpret ed as implying that dissolved CO2 becomes the dominant DIC species. W e will revise the manuscript to avoid this implication and instead describ e the transition in terms of the declining buffering capacity of the carbon ate system. The revised text will read:

"This convergence marks a transition toward a weakly buffered carbonat e-system regime, in which further increases in DIC generate disproporti onately large increases in surface pCO2ocn. As buffering weakens, ..."

L424 Could also mention the absence of sediment feedbacks here.

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

"... changes in CaCO3 production and dissolution. In addition, the model d oes not include sediment carbonate dissolution or associated benthic alk alinity feedbacks, which could partially offset surface-ocean alkalinity de clines and modify the long-term evolution of ocean buffering capacity an d carbon uptake on multi-centennial timescales. Recent work suggests th at ..."

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

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

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

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

We agree that the original wording was not sufficiently clear. The freshw ater forcing referred to here is specifically freshwater input from ice-sh eet melt, which is not included in the present simulations. We will revise t he manuscript to make this explicit and clarify that the expected enhanc ement of deep-ocean carbon storage arises primarily from reduced AAB W formation and weakened deep-ocean ventilation, rather than from enh anced remineralization associated with older water masses. The revised t ext will read:

"Freshwater input from ice-sheet melt, which is not included in the prese nt simulations, is expected to amplify the long-term impact of anthropog enic carbon storage on seawater chemistry by reducing AABW formatio n and deep-ocean ventilation. The resulting increase in deep-water resid ence time would prolong the isolation of carbon-rich waters from the at mosphere, 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 t o Wilson et al. (2022). We agree that the projected increase in export pro duction under the low-emission scenarios differs from the general behav ior reported in previous CMIP6 multi-model assessments, and that the n ovelty of this result should be highlighted more clearly. We will revise th e discussion to emphasize that the long-term simulations reveal a transi ent response that is not apparent from end-of-century analyses alone. In particular, export production responds rapidly during the 21st century a nd closely follows the evolution of the AMOC. The transition from increa sing to decreasing export occurs once atmospheric CO2 exceeds approxi mately three times the pre-industrial level, when the weakening of the A MOC becomes more pronounced under stronger forcing. We are also con sidering adding a figure showing the temporal evolution of export produ

ction together with AMOC strength to illustrate this relationship more cl early. The revised text will read:

"Simulation results for detritus export at the depth of 100 m vary substa ntially among emission scenarios. Detritus export exhibits a long-term i ncrease 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 div ergence develops during the 21st century and closely follows the rapid a djustment of the large-scale ocean circulation. Export production declin es as the weakening of the AMOC becomes more pronounced under stro nger forcing; this transition occurs when atmospheric CO2 exceeds appr oximately three times the pre-industrial level (Fig. xxx). Consequently, t he long-term behavior is largely established by the end of the 21st centur y and remains comparatively stable thereafter, a feature that is difficult t o identify from end-of-century analyses alone. Regionally, detritus expor t decreases across the high-latitude North Atlantic under all scenarios, w ith the largest decline occurring under SSP5–8.5, consistent with the stro ngest 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 sho wn to reduce ocean carbon uptake (e.g. Swingedouw et al., 2007).

We thank the reviewer for this important comment and for drawing our a ttention to Swingedouw et al. (2007). We agree that the sign of the bias in troduced by the omission of ice-sheet freshwater forcing is not straightf orward and may depend on the source of the freshwater perturbation an d the associated circulation response. We will therefore revise the manus cript to avoid implying a one-sided bias and instead emphasize that the o mission of dynamic ice-sheet freshwater forcing represents an importan t source of uncertainty in long-term projections. The revised text will re ad:

"... its influence on buoyancy-driven overturning (Rahmstorf et al., 2002). Recent studies have shown that Antarctic ice sheet melt can weaken AA BW 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 (Sw ingedouw et al., 2007). The omission of dynamic freshwater forcing in th e present study therefore represents an important source of uncertainty in projections of long-term ocean carbon storage and acidification. Futur e modeling efforts ..."

L563-565 This is a feature of carbonate chemistry projections at depth b ut not in the global surface ocean where there is generally high model ag reement for acidification projections with the exception of regions domi nated by sea ice dynamics or riverine fluxes. I therefore suspect the imp act 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 car bon uptake simulations (Terhaar et al., 2022).

We thank the reviewer for this helpful comment and for drawing our atte ntion to Terhaar et al. (2022). We agree that projections of surface-ocean acidification are generally robust across models, except in regions stron gly 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 ca rbonate-system buffering capacity and its implications for long-term oc ean carbon uptake. We will also cite Terhaar et al. (2022), who showed tha t 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 acidi fication are generally robust across Earth system models, substantially g reater uncertainty remains in the timing and magnitude of changes in ca rbonate-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 b iases in projected ocean carbon uptake in CMIP6 models (Terhaar et al., 2 022). At the same time, ocean acidification poses significant risks to mari ne ecosystems, particularly calcifying organisms (Doney et al., 2009). "

Response to Reviewer #2

Overall Assessment and Recommendation

I appreciate the overall topic of this study, which addresses an important and timely question: the multi-centennial response of ocean biogeochemistry and marine ecosystems to anthropogenic CO2 forcing. Long-term projections beyond 2100 remain relatively scarce, and the results presented here have the potential to provide valuable insights into both the future evolution of the ocean carbon sink and the persistence of ecosystem stressors. These are important issues that deserve attention and publication.

However, I find that the current manuscript suffers from significant organizational and conceptual weaknesses. First, the scientific objectives are not clearly defined or sufficiently justified. The Introduction combines several distinct themes—including ocean carbon uptake, low-latitude productivity, nutrient cycling, ocean acidification, and ecosystem stress—without developing a coherent argument that explains why the long-term response (2100–2500) should be investigated and which specific scientific questions are being addressed. As a result, the narrative lacks focus and the reader is left uncertain about the primary goals of the study.

The Results section reflects this lack of focus. It concentrates disproportionately on the evolution of DIC and NPP, while other potentially more impactful aspects of the simulations receive limited attention. More generally, the manuscript attempts to address two major questions—long-term

carbon uptake and long-term ecosystem stressors—without clearly sepa rating them or establishing strong links between them.

In its current form, I do not believe that the manuscript is ready for publi cation in Biogeosciences. I strongly recommend a major reorganization o f the paper. The authors should either clearly separate the carbon uptake and ecosystem stress components into distinct parts of the manuscript, each with dedicated objectives and analyses, or alternatively choose to fo cus on only one of these themes. Such a restructuring would require a su bstantial rewriting of the Introduction, a clearer formulation of the scien tific objectives, and a reconsideration of the results presented in the cent ral sections of the paper. I believe that these changes would significantly improve the clarity, impact, and overall scientific value of the study.

We thank the reviewer for the thoughtful and constructive assessment o f our manuscript. We appreciate the recognition that multi-centennial pr ojections of ocean biogeochemistry remain scarce and that such simulati ons have the potential to provide important insights into the long-term e volution of the ocean carbon sink and marine ecosystems.

We agree that the original manuscript did not sufficiently articulate its c entral scientific objectives or clearly explain the common scientific ques tion linking the different components of the study. Our objective is to un derstand how the long-term adjustment of the ocean to anthropogenic C O2 forcing differs among emission pathways, and how these differences shape the evolution of ocean carbon uptake, carbonate chemistry, biologi cal productivity, and ecosystem stress over multi-centennial timescales. Rather than examining each of these processes in isolation, this study ai

ms to understand how these components co-evolve and interact during t he long-term adjustment of the ocean to anthropogenic CO2 forcing. We respectfully consider that ocean carbon uptake and ecosystem stress are not independent themes, but complementary aspects of the long-term re sponse of the ocean to anthropogenic CO2 forcing.

In response to the reviewer's comments, we will substantially revise the manuscript. We will reorganize the Introduction to better motivate the sc ientific importance of simulations extending beyond 2100, explicitly stat e the scientific questions addressed in this study, and clarify the rational e for investigating long-term ocean biogeochemical responses across m ultiple emission scenarios. We will also strengthen the motivation for inv estigating ecosystem stress by introducing recent studies that use Earth system model projections to assess impacts on higher trophic levels (e. g., FISHMIP), thereby clarifying how long-term changes in ocean biogeoc hemistry provide an important foundation for understanding future mar ine ecosystem responses. In addition, we will expand the discussion of pr evious long-term Earth system simulations and more clearly position the present study within this broader context.

We will also reorganize parts of the Results section to better distinguish responses established during the 21st century from slower multi-centen nial adjustment processes while emphasizing their common physical and biogeochemical drivers. To strengthen the mechanistic interpretation, w e will expand the analyses of biological productivity and ocean carbon st orage. In particular, we will distinguish the contributions of preformed a nd regenerated carbon to long-term carbon sequestration, further exami ne the transient evolution of net primary production and export product

ion in relation to atmospheric CO2 and changes in the Atlantic Meridiona l Overturning Circulation, and clarify the regional characteristics of thes e responses. We will also add new figures, where appropriate, and promo te selected supplementary figures to the main text to better illustrate th ese relationships.

These additional analyses will clarify that many key ocean biogeochemic al responses are established primarily during the 21st century, whereas t heir adjustment continues over subsequent centuries. By comparing mul tiple emission scenarios, the revised manuscript will more clearly demon strate how both the magnitude and persistence of these long-term respo nses depend on the trajectory of anthropogenic forcing, highlighting an aspect that cannot be inferred from conventional end-of-century analyse s alone.

Finally, we will expand the Discussion to clarify the physical and biogeoc hemical mechanisms responsible for the simulated changes, compare ou r results more comprehensively with previous studies, and explicitly dis cuss important model limitations and uncertainties, including alkalinity f eedbacks, sediment processes, freshwater forcing from ice-sheet melt, a nd uncertainties in carbonate-system buffering.

We believe that these planned revisions will substantially improve the cl arity, organization, and scientific focus of the manuscript while more cle arly communicating its central scientific objective of understanding how long-term ocean adjustment differs among emission pathways and the i mplications of these differences for ocean carbon uptake and marine eco systems.

Specific Comments

(1) Introduction

The Introduction requires substantial reorganization. As it stands, it interweaves results and concepts related to the evolution of the ocean carbon sink, primary productivity, and ecosystem stressors (e.g., acidification), without clearly distinguishing these components or explaining why they should be considered together, nor how they potentially interact or feed back on one another. Some statements appear overly general and insufficiently supported by the literature; for instance, the claim that "low-latitude productivity is crucial for the global carbon cycle" is too simplistic and would benefit from a more explicit and better substantiated discussion.

In addition, the paragraph starting at line 59 ("Analyses using CMIP models have also advanced understanding of present-day oceanic uptake of anthropogenic CO2") transitions abruptly from present-day understanding to long-term projections without a clear logical bridge. Given the relatively limited number of studies addressing multi-centennial ocean biogeochemistry, the motivation for focusing on long-term responses needs to be strengthened. In particular, the authors should better justify why centennial-to-millennial timescales are relevant, explicitly discussing ocean adjustment timescales, (ir)reversibility of carbon-cycle changes, and the relevance for net-zero or overshoot scenarios.

We thank the reviewer for this important and constructive comment. We agree that the original Introduction did not sufficiently articulate the ov

erarching motivation of the study or clearly define the scientific questio ns addressed. We also agree that the connections among long-term ocea n carbon uptake, carbonate chemistry, biological productivity, and ecosy stem stress were not adequately explained, and that the motivation for e xtending the analysis beyond 2100 required a stronger justification.

To address these concerns, we will substantially revise the Introduction. We will reorganize the Introduction to more clearly motivate the need fo r simulations extending beyond 2100 and explicitly state the scientific q uestions addressed in this study. We will strengthen the motivation by di scussing the long adjustment timescales of the ocean and their relevance to future mitigation pathways. We will also clarify why ocean carbon upt ake, carbonate chemistry, biological productivity, and ecosystem stress a re investigated together by emphasizing that they represent complemen tary components of the long-term ocean biogeochemical response withi n a common Earth system framework. Furthermore, we will strengthen t he Introduction by incorporating recent studies that link Earth system m odel projections to higher trophic-level ecosystem impacts (e.g., FISHMI P), thereby providing a clearer motivation for examining long-term ecos ystem stress. We will also revise several overly general statements, inclu ding the discussion of low-latitude productivity, to provide a more balan ced description supported by the relevant literature.

The revised Introduction will conclude with the following paragraph:

"Despite growing understanding of 21st-century ocean change, the long-t erm evolution of ocean biogeochemistry after atmospheric CO2 stabiliza tion remains poorly constrained. Because ocean circulation, carbon stora

ge, nutrient redistribution, and ecosystem responses operate on timescales ranging from decades to centuries, substantial adjustments may continue long after atmospheric CO2 concentrations cease increasing. Understanding these delayed responses is particularly relevant for evaluating the long-term consequences of mitigation pathways. In this study, we use extended SSP simulations with MIROC-ES2L through the year 2500 to address three questions: (1) How does ocean carbon uptake evolve after atmospheric CO2 stabilization? (2) What processes control the long-term evolution of ocean carbonate chemistry and buffering capacity? (3) How do biological productivity and ecosystem stress evolve on multi-centennial timescales? By addressing these questions within a single Earth system model framework and across multiple emission scenarios, we identify how long-term ocean adjustment depends on the trajectory of anthropogenic forcing and distinguish responses established during the 21st century from slower adjustment processes that continue for centuries thereafter."

(2) Results structure and metrics

The first results section is relatively short and focuses primarily on dissolved inorganic carbon (DIC) and net primary production (NPP)/export production. This choice is not sufficiently justified. In particular, it is unclear why the evolution of ocean carbon uptake itself is not treated as a central diagnostic, given that it is arguably one of the key variables of the study.

We thank the reviewer for this helpful comment. We agree that the centr al role of ocean carbon uptake was not sufficiently emphasized in the ori ginal presentation of the Results. Our intention was to first describe the l arge-scale redistribution of dissolved inorganic carbon, which provides t he physical and biogeochemical context for the subsequent analyses of o cean carbon uptake, carbonate chemistry, and deep-ocean carbon storag e.

To make this progression clearer, we will revise the first Results section to more explicitly highlight the evolution of ocean carbon uptake. In part icular, we will add the time evolution of the accumulated ocean carbon in ventory for each scenario, thereby providing a more direct diagnostic of long-term ocean carbon uptake and complementing the redistribution of dissolved inorganic carbon. We will also revise the subsection title to bet ter reflect that this section describes both the redistribution of dissolved inorganic carbon and the evolution of ocean carbon uptake.

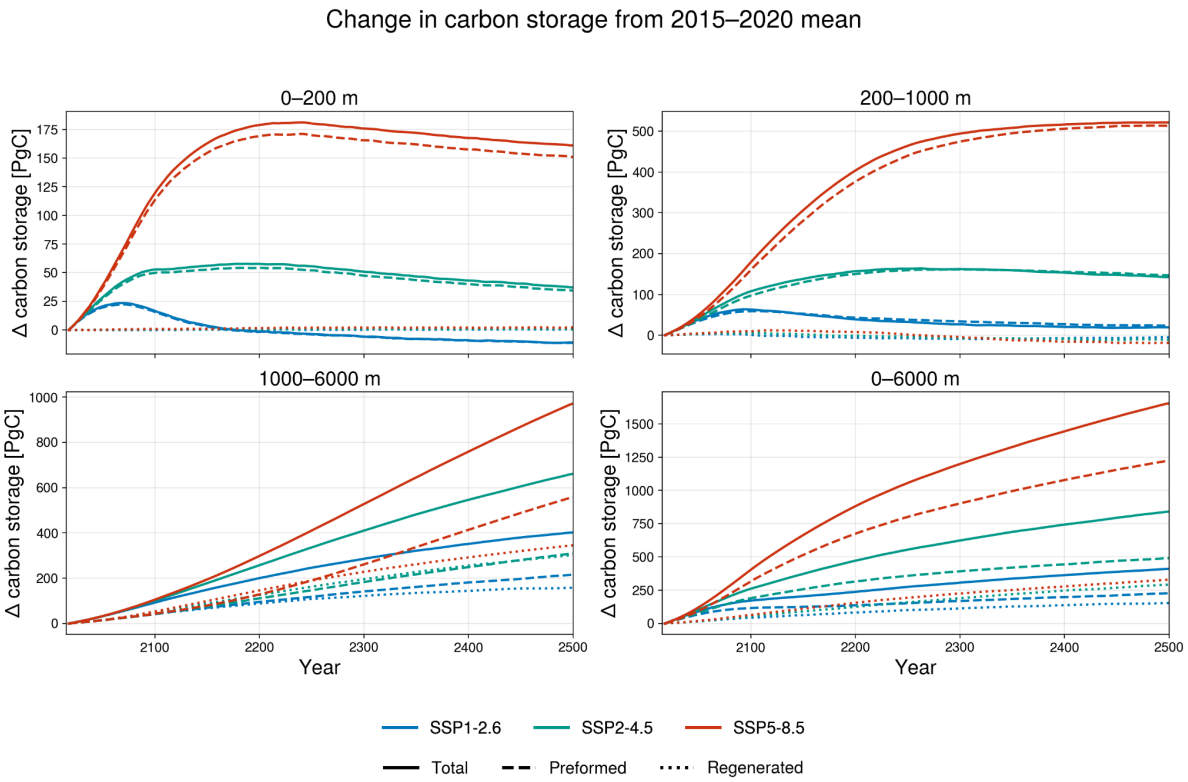


Figure R1. Time evolution of changes in ocean carbon storage (PgC) unde r the SSP1–2.6, SSP2–4.5, and SSP5–8.5 scenarios for the (a) 0–200 m, (b) 2 00–1000 m, (c) 1000–6000 m, and (d) 0–6000 m depth ranges. Solid lines denote total carbon storage, dashed lines denote preformed carbon stora ge, and dotted lines denote regenerated carbon storage. Regenerated car bon was estimated from apparent oxygen utilization, and preformed carb on was obtained as the residual.

(3) NPP response under contrasting scenarios

The evolution of NPP differs markedly between scenarios, with a decreas e under high-emission conditions and an increase under low-emission co nditions. This is a striking and potentially highly significant result. Howe

ver, it is not discussed or explained in any depth, which represents a mis sed opportunity given its implications.

We thank the reviewer for highlighting the importance of this result. We agree that the contrasting responses of net primary production under lo w- and high-emission scenarios are one of the key findings of this study and deserve further discussion.

In response to this comment, we will substantially expand the correspon ding Results and Discussion sections. In particular, we will examine the t emporal evolution of net primary production in relation to atmospheric CO2 and changes in the Atlantic Meridional Overturning Circulation, hig hlighting that much of the divergence among emission scenarios is estab lished during the 21st century and subsequently evolves more gradually over multi-centennial timescales. We will also clarify the regional charac teristics of the simulated responses and discuss their consistency with p revious studies.

At the same time, we recognize that a complete attribution of the underly ing mechanisms would require additional process-oriented diagnostics a nd dedicated sensitivity experiments to separate the respective roles of physical circulation, nutrient redistribution, and biological feedbacks. Su ch analyses would require dedicated sensitivity experiments and additio nal diagnostic calculations to isolate the relative contributions of physic al circulation, nutrient redistribution, and biological feedbacks, and are t herefore beyond the scope of the present study.

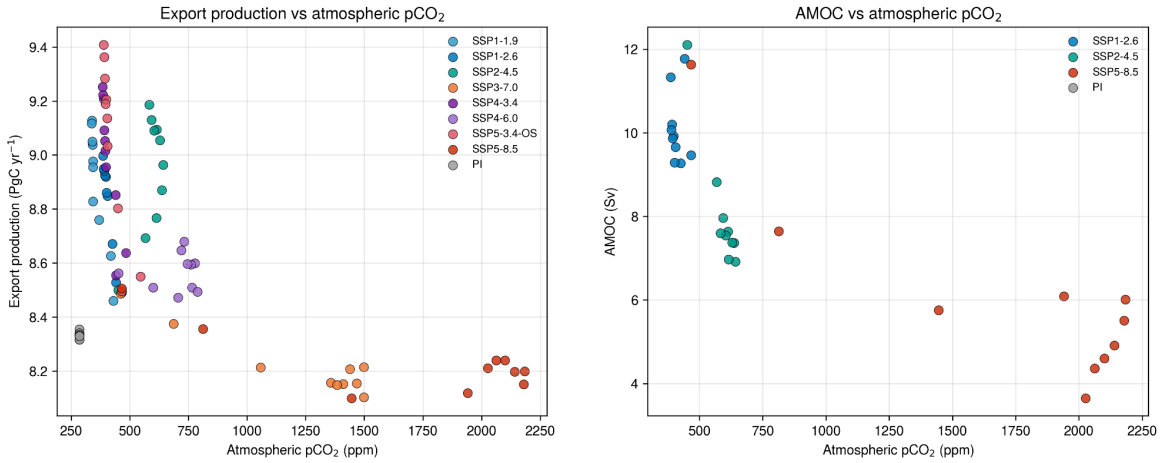


Figure R2. Scatter plots showing the relationships between atmospheric CO2 concentration and (a) global export production (Pg C yr^{-1}) for all emi ssion scenarios analyzed in this study, and (b) Atlantic Meridional Overt urning Circulation (AMOC) strength (Sv) under the SSP1–2.6, SSP2–4.5, an d SSP5–8.5 scenarios.

(4) Carbon uptake drivers and biological pump

The section describing the drivers of carbon uptake is generally well dev eloped and clearly presented. However, it would be particularly interesti ng to further explore the role of the biological pump across these long ti mescales, especially in comparison with the physical pump. In particular, the manuscript could attempt to assess whether biological processes bec ome increasingly important in controlling carbon uptake over centennial to millennial scales.

We thank the reviewer for this valuable suggestion. We agree that the rel ative roles of the biological and physical pumps in regulating long-term ocean carbon uptake deserve further discussion.

In response to this comment, we will expand the Discussion by explicitly considering the complementary roles of physical circulation and biologi cal processes over multi-centennial timescales. Building on the newly ad ded analysis of preformed and regenerated carbon storage, we will discu ss how these diagnostics provide additional insight into the mechanisms controlling long-term carbon sequestration. In particular, we will empha size that regenerated carbon storage continues to increase even as expo rt production declines under the high-emission scenarios, whereas prefo rmed carbon remains the dominant contributor to the long-term increas e in deep-ocean carbon storage. These contrasting responses suggest th at long-term regenerated carbon storage is influenced not only by biolog ical export but also by changes in ocean circulation and water-mass resi dence time.

Accordingly, we will add the following discussion to Section 4.1.3 “Deep-o cean carbon sequestration, circulation pathways, and potential CO2 outg assing”:

"Separating deep-ocean carbon storage into preformed and regenerated components further illustrates the complementary roles of the physical and biological pumps in long-term carbon sequestration. Although expor t production declines under the high-emission scenarios, regenerated ca rbon storage continues to increase. This contrasting behavior suggests t hat long-term regenerated carbon storage is influenced not only by biolo gical export but also by changes in ocean circulation and water-mass res idence time. Nevertheless, preformed carbon remains the dominant cont ributor to the long-term increase in deep-ocean carbon storage. These re sults suggest that changes in ocean circulation remain a primary control

on long-term carbon sequestration, while biological processes continue t o influence the redistribution and storage of carbon within the ocean int erior."

A complete quantitative attribution of the respective contributions of th e physical and biological pumps would require additional process-orient ed diagnostics and dedicated sensitivity experiments, and is therefore be yond the scope of the present study.

(5) Ecosystem focus

The analysis of specific ecosystems (e.g., kelp forests, coral reefs) appear s underdeveloped and only marginally integrated into the overall narrati ve. It is not entirely clear that this level of ecosystem-specific detail is es sential for the scope of the manuscript, and reconsideration of this secti on may help improve focus and coherence.

We thank the reviewer for this helpful suggestion. We agree that the role of the ecosystem-specific examples was not sufficiently clear in the origi nal manuscript.

Our intention is not to provide detailed assessments of individual ecosys tems, but rather to illustrate how long-term changes in ocean biogeoche mistry may translate into persistent ecosystem stress under different e mission pathways. To clarify this objective, we will revise both the Introd uction and the Results. In the Introduction, we will strengthen the motiva tion for including ecosystem stress by discussing recent studies that use Earth system model projections to assess impacts on higher trophic leve

ls (e.g., FISHMIP), thereby placing the ecosystem analyses within a broad er Earth system context. In the Results, we will reduce descriptive details associated with individual ecosystem types and instead emphasize that c oral reefs, kelp forests, and other examples are intended to illustrate bro ader patterns of long-term ecosystem vulnerability rather than to provid e ecosystem-specific assessments.

These revisions will strengthen the connection between the ecosystem a nalyses and the central objective of the manuscript, namely understandi ng how long-term ocean biogeochemical adjustment differs among emis sion pathways and how these differences influence future marine ecosys tem stress.