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Peer Review File

Organic-inorganic carbon coupling shapes carbon dioxide fluxes in seagrass ecosystems

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Summary:

In their article on “Revisiting Blue Carbon”, Chou and co-authors suggest that an organic carbon-centric approach to evaluating net carbon storage in seagrass meadows fails to take into the account role of inorganic carbon cycling in carbon flows through these coastal ecosystems. In particular, net community calcification and both inorganic and organic matter sources (allochthonous vs. autochthonous) have important ramifications for CO2 source vs. sink estimates in seagrass meadows. The authors go on to propose a metric (the ratio of net community production to net community calcification, or NCP/NCC) that links inorganic and carbon cycling in these ecosystems, and a more qualitative and conceptual framework for considering how different combinations of allochthonous/autochthonous inorganic/organic carbon would impact net CO2 flows.

Major comments:

The major justification for this paper is that carbon accounting in blue carbon/seagrass ecosystems disproportionately focuses on NCP and carbon burial in sediments. This paper is largely about incorporating shifts in carbonate chemistry into the framework, which makes the discussion quite novel in itself. The introduction (Sect. 1) sets this up well; it is coherent and easy to follow, making it accessible for a variety of readers who do research, policy or decision-making in these coastal ecosystems. I can see it being influential to the field if revised and published, and comprehensive enough to be incorporated into future (coastal) oceanography textbooks.

I found three major conceptual and/or structural aspects of the paper that need to be strengthened going forward. First, the justification that NCP alone is not “enough” for accurately quantifying carbon sources/sinks in seagrass meadows needs some further clarification in Sect. 1. Over the open ocean, NCP can be decoupled from sediment burial due to different carbon flux integration depths. In this paper, what is the quantitative relationship between NCP and OC burial in sediments? Since seagrass meadows tend to be shallow, is NCP integrated over the entire water column such that it is tightly coupled to OC burial?

Relatedly, it seems that NCP and NCC must operate on similar time scales for the authors to propose a usable NCP/NCC metric in Sect. 2. Rates of seawater equilibration with atmospheric CO2 can differ from the rates of carbon burial in sediments. Rates of alkalinity production from respiration at the sediment-water interface can differ from organic matter production at the surface. Perhaps, again, these rates may be similar enough in a shallow seagrass meadow, but the discussion in lines 213-227 also suggest otherwise. Could the authors address time scale more explicitly in Sect. 1?

Second, the suggestion of the NCP/NCC metric is both practical and novel. But, because the NCP/NCC metric only works in scenarios with purely autochthonous carbon production, its usefulness seems undermined by sections 3-4, which largely focus on seagrass meadows with mixed autochthonous and allochthonous carbon flows. The fact that the case study does not directly utilize the NCP/NCC metric seems to be at odds with the concluding statement in Sect. 5, lines 355-357. To amplify the importance of NCP/NCC in this paper, perhaps there is a way to provide a case study that directly utilizes

NCP/NCC, or suggest how scenarios B-D in Sect. 3 would shift the NCP/NCC ratio relative to the compensation threshold (perhaps through a new panel e in Figure 2)?

Finally, the discussion in Sect. 3 is rich and has potential to grow into a full literature review alone. But, because it is being written in shorter form here, the authors have focused on specific mechanisms and scenarios, and it is not always clear why. For example, Scenarios A and D seem to be end-members in this conceptual model - are Scenarios B and C "end-members" too? It otherwise seems unrealistic to assume that autochthonous $CaCO_3$ production is absent from scenario B and autochthonous OC production is absent from scenario C. In addition, why is only sulfate reduction discussed when other mechanisms of anaerobic OC respiration are possible in coastal sediments?

Minor, line-by-line comments:

Lines 127-137, 138-149: For consistency, it would help to explain if NCP/NCC >0.63 or <0.63 (the 400µatm iso-pCO_2 threshold) in each bullet point, i-iii. This emphasizes the utility of the NCP/NCC calculation, which is otherwise quite clear earlier in the section.

Figure 2: What is the shading in the backgrounds of panels c and d? If there is no color bar or quantitative meaning behind the coloring, is it necessary? Also, it would help to divide panel d into six regions that match the six bullet points in lines 137-149 (e.g., regions I-VI that could then be referenced in those specific lines/bullet points)?

Sect. 3: Initially, I was confused by the reaction numbers that popped up in parentheses in this section. It would be helpful to cite Figure 3 when reaction 1 is first referenced in line 204 so readers know to link reaction number to that figure.

Figure 3: I appreciate the work that went into making this figure. But, I think it could actually be more impactful as a table with scenarios A-D listed over different rows. In such a table, the chemical reactions and their impact on TA, DIC and pCO_2 could then be explained over different columns. Reactions could still be numbered to match the text in Sect. 3.

Lines 313 and 344: The term "re-uptaken" reads awkwardly to me. Consider rephrasing.

Reviewer #2

(Remarks to the Author)
The manuscript by Natividad et al. presents a framework for analysing the combined effects of net organic matter production and net calcification to understand the overall carbon balance of seagrass beds. This is an important problem, because as the authors argue, there has been too much emphasis purely on the organic matter accumulation in such Blue Carbon ecosystems without considering the alkalinity dynamics. The framework is presented and explained well, the manuscript is clearly written, and I think it's appropriate for the journal.

The one aspect that I find a bit problematic is the emphasis on autochthonous versus allochthonous OC and $CaCO_3$. I agree with the authors that it is very relevant for the local-scale CO_2 flux within a given seagrass meadow whether these inputs to the sediments are from local production or whether they are advected to the site. However, I think that misses the bigger picture, which is how the overall ocean–atmosphere CO_2 balance is affected. Essentially, as I understand the authors, the authors are saying that a seagrass meadow will be a strong carbon sink if $CaCO_3$ is produced off-site (e.g. by a nearby coral reef), advected to the seagrass meadow, and then dissolves in the seagrass. However, from the point of view of the ocean as a whole this is no different than if the $CaCO_3$ was produced and recycled within the meadow: for the ocean as a whole, the same quantity of $CaCO_3$ has been precipitated and then redissolved. The only difference is that a seagrass meadow that dissolved externally supplied $CaCO_3$ will have lower pCO_2 within the meadow compared to one where $CaCO_3$ is produced and recycled locally. It is just than in the former case, the effect of alkalinity precipitation on pCO_2 happens elsewhere and is then not accounted for in the local carbon balance of the meadow. So I think the relevant question is not so much about the geographical origin of the OC and the $CaCO_3$, but rather whether a given seagrass meadow leads to more alkalinity production within the ocean as a whole than without the meadow. The same logic also applies, I think, to OC remineralisation: I think that whether a seagrass meadow leads to remineralization or burial of allochthonous OC versus autochthonous OC is less important than whether overall the seagrass leads to more or less net burial of OC, regardless of where it is produced. For example, a given seagrass meadow might receive a lot of allochthonous OC that is then respired in the seagrass meadow, but that allochthonous OC might be ultimately respired anyway even without the seagrass meadow. Possibly, the presence of the seagrass might actually help a larger fraction of the allochthonous OC to get buried thanks to the anoxic sediments.

I would therefore recommend that the authors consider taking a larger view of the overall carbon balance beyond just the immediate boundary of a given meadow, and clarify that the important consideration should be whether maintaining / restoring seagrass in a given area will lead to beneficial overall changes in NCP and NCC, considering the net carbon balance of the seagrass and of the sites where allochthonous inputs originate from. I think this could be done with a relatively moderate amount of rewriting and clarification, because the framework of thinking about NCP vs NCC per se is robust.

Additional minor comments:

Line 122-124: that's true, although reductions in NCC at increasing pCO_2 levels will also ameliorate this effect a bit; maybe that should be acknowledged?

Line 224–227: isn't this just a rather complicated way of explaining that it is the NET production / calcification for the system as a whole that matters?

Reviewer #3

(Remarks to the Author)
This perspective by Natividad et al. nicely presents a novel conceptual framework to assess the CO2 removal potential of seagrass meadows. The theoretical background is elegantly presented and will be useful for the interpretation of carbonate chemistry data of other field studies (especially Figure 2). The framework will also be very useful for the current blue carbon rush, and a complete and nuanced assessment like the one presented here is - in my opinion – long overdue. I have a few comments to further strengthen the framework, and a few inconsistencies that should be addressed, but overall I am supportive of the publication of this article after revisions.

Kind regards
Sebastiaan van de Velde

General comments

I am not sure why you limit yourself to seagrass meadows, you could easily expand this framework to any coastal ecosystem, at the very least other vegetated ecosystems like mangroves and salt marshes.
You will need to clarify upfront what the boundaries of the systems are. Your point of not taking into account allochtonous carbon is valid, but then you do include this in your different scenarios. To be consistent, you will also need to account for lateral transport of seagrass OC/IC out of the system. This of course makes the assessment very complicated, as you will then have to account for transport and hydrodynamics, but it is this level of nuance that we are missing in the current discussions.

Coupling air-sea exchange directly to pCO2 is risky – exchange might happen in well-mixed systems, but aside from equilibration timescales that move the CO2 uptake outside of your boundaries, you could also have subduction that prevent re-equilibration 1. It might be more robust to focus on pCO2 alone and leave the air-sea exchange as 'potential air-sea CO2 uptake'.

The sulfate reduction and TA production is not consistently described throughout the paper – half of the time you seem to suggest that it is always a TA source, other times you correctly add the caveat that this only works if S precipitates and remains in the sediment.

Specific comments

L34: 'blue-carbon sequestration' -> 'carbon sequestration'

L36: I would temper that statement – some meadows like Poseidonia in the Mediterranean might reach those levels, but a lot of temperate meadows do not

L41: aerobic respiration – anaerobic respiration would affect alkalinity differently

L42: clarify that this is only when you define your system as the meadow + seafloor itself – this does not take into account the role of lateral OC transport which makes assessing the role of these meadows more complicated (which is also something that is commonly overlooked and you could highlight here as well)

L47: is this true for all seagrasses or just certain ones?

L60: this statement is not entirely accurate – the production of reduced species like H2S creates TA, but as long as there is no formation of reduced minerals, H2S will be reoxidized and the TA will be lost again. There can be a decoupling in the sediment, but unless the TA consumption due to reoxidation drives CaCO3 dissolution more than the TA production at depth drives CaCO3 production, there will be a net zero effect on CO2 uptake (unless the H2S is captures by Fe to form FeS or FeS2).
Same for Figure1: the reactions there are not correct as you do not account for the reoxidation of H2S (you mention this yourself at L160).

L65: reference 15 has not proven this, this is a (very) poorly constrained model study that suggests this could be the case – which also only considers the benthic environment and not the potential change in calcification in the water column. I would rephrase this sentence, or find a reference that has shown this occurring in real life

L86: is that true? What do you mean by 'geogenic' ? There are studies from the Baltic Sea that show that erosion of carbonate-rich clays lead to IC input that subsequently dissolves 2,3

L122: this is well established from previous work by Frankignouille and the Revelle factor, so I suggest included citations 4,5 and earlier work by Revelle

L127: I guess you mean DIC uptake and not organic carbon?

L141: see my main comment, I would extend your framework to all coastal systems, since coral reefs are arguably not seagrass meadows

L142: it is unclear to me how a seagrass meadow becomes heterotrophic without including the allochtonous contribution? It seems to me that you are not following your own logic?

L164: 'further increasing alkalinity export' – you seem to suggest that sulfate reduction coupled to sulfide reoxidation creates TA, which is not correct. I suggest rephrasing these sentences.

Figure 2: blue/red colours are not very colour blind friendly, suggest to change the colourscheme.
I am sorry to be pedantic, but present day is closer to 425 than to 400 … Also, 25 degrees is more tropical than average temperate conditions

L219: they do exert an immediate effect, as the fact that they persist means there is net consumption of DIC (for OC and IC) and TA (For IC)?

L234: could make the same case for N2 production – which might be important in coastal sediments?

L249: only with S burial …

Figure 3: I don't like reaction 5 -> the deltaTA has to be dependent on the fraction of FeS precipitation, now it suggest it is independent and always +2.
Reaction 4 mentions 'autochtonous IC dissolution', even when your situation describes allochtonous IC

L293: why do you refer to figure 3 here?

L301: is that shown? You refer only to watercolumn measurements and benthic fluxes? It might as well be nitrogen cycling or anything else?

L313: re-uptaken is a funny way of phrasing

L321: That estimate seems low compared to TA fluxes measured globally 6.
It might be worth checking this CaCO3 cycling review to get data-derived estimates of CaCO3 dissolution and TA production in coastal and shelf sediments: 10.22541/essoar.176131953.37180357/v1

L332: it is hardly an unrecognized pathway given the papers you cite and the review mentioned earlier (see also, for example, 7). You do present it nicely in a conceptual framework which is the true novelty of this contribution – since most studies generally look at these pathways individually.

References:
1. Hurd, C. L. et al. Forensic carbon accounting: Assessing the role of seaweeds for carbon sequestration. Journal of Phycology 58, 347–363 (2022).
2. Scholz, F., Börker, J., Vogt, C., Hartmann, J. & Wallmann, K. Natural ocean alkalinization through erosion of glacial till and weathering at the seafloor. Commun Earth Environ 6, 974 (2025).
3. Wallmann, K. et al. Erosion of carbonate-bearing sedimentary rocks may close the alkalinity budget of the Baltic Sea and support atmospheric CO2 uptake in coastal seas. Front. Mar. Sci. 9, 968069 (2022).
4. Frankignoulle, M. A complete set of buffer factors for acid/base CO2 system in seawater. Journal of Marine Systems 5, 111–118 (1994).
5. Frankignoulle, M., Canon, C. & Gattuso, J.-P. Marine calcification as a source of carbon dioxide: Positive feedback of increasing atmospheric CO2. Limnology and Oceanography 39, 458–462 (1994).
6. van de Velde, S. J., Hylén, A. & Meysman, F. J. R. Ocean alkalinity destruction by anthropogenic seafloor disturbances generates a hidden CO2 emission. Science Advances 11, eadp9112 (2025).
7. Reithmaier, G. M. S. et al. Alkalinity Production Coupled to Pyrite Formation Represents an Unaccounted Blue Carbon Sink. Global Biogeochemical Cycles 35, e2020GB006785 (2021).

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
General overview

The revised version of "Revisiting Blue Carbon: How organic-inorganic coupling shapes CO2 fluxes in seagrass ecosystems" continues to display the strengths of the first submission: an integrated framework that considers how NCP and

NCC counterbalance each other in the context of air-sea CO2 exchange in seagrass ecosystems. The revisions have been mostly sufficient in terms of addressing initial reviewer concerns and suggestions. Some of my own concerns have been alleviated by the authors' clarification of how the NCP/NCC compensation ratio ought to be interpreted and how cycling of organic carbon (OC) and inorganic carbon (IC) from allochthonous sources shifts this NCP/NCC ratio mechanistically. Table S1 is helpful, as well.

For these reasons, I still think that this review paper would be a valuable contribution to scientists, scholars and students studying/learning about carbon cycling/accounting in shallow, coastal ecosystems. However, I do have some lingering comments that will hopefully be addressed before publication.

My first comment is about timescale again and is related to section 1. The paper clarifies that long term sediment burial is not the focus – shorter-term air-sea CO2 exchange is (as stated in lines 91-100). But, I think that the introduction needs to acknowledge the different (shorter-than-OC-burial) timescales in which water column metabolism and sedimentary metabolism operate, and the extent to which these two sites of DIC and TA removal/production are coupled from each other in time. This temporal coupling or decoupling seems to matter a lot for the interpretations of scenarios in section 3, and is referred to frequently in Section 3. For example, lines 252-255 state that sedimentary processes "do not directly modify seawater carbonate chemistry on short timescales unless coupled to exchange with the overlying water column". In the Dongsha Island Inner Lagoon, it appears that this temporal coupling is indeed present and enables the ecosystem to be a CO2 sink (exhibiting consistently depressed pCO2 values). Something in the introduction – perhaps in the existing time scale paragraph over lines 91 to 100 – that establishes the importance/lack/possibility of temporal coupling between water column metabolism and sedimentary metabolism would better prepare the readers for sections 3 and 4.

My second comment is related to section 3. I think it's important to compare and contrast the different mechanisms driving DIC and TA exchange through Scenarios A to D of Section 3; this is a major contribution of the paper. But, there seems to be a disconnect between the scenario A and B descriptions and their big picture implication about CO2 sources vs. sinks. At least for Scenarios A and B, it seems like the source vs. sink outcome depends on the quantitative balance (and temporal coupling) among all of the reactions listed for each scenario.

As a result, I do not see how the implied CO2 balance is always near neutral for scenario A, which is what the text and Table S1 imply. Lines 242-243 suggest that this balance should be set by the NCP/NCC compensation ratio, which fits the arguments in Section 2. In addition, Table S1 and lines 267-269 imply that a CO2 sink could be possible if enough of a TA surplus is generated by sulfate reduction in the sediments. If this sediment-generated TA surplus was closely coupled to water column chemistry, would it not be possible for Scenario A to lead to a CO2 sink?

Likewise, lines 274-276 at the end of the Scenario B discussion seem to imply that whether Scenario B yields a CO2 source or sink is also context dependent (i.e., how much TA surplus is generated from allochthonous IC sources). This is at odds with the conclusion in Table S1 – which is that Scenario B always yields a strong CO2 sink.

Smaller comments:

Line 211: What does "their" refer to in the phrase "their coupling"? Does it refer to the sedimentary carbon pools?

Line 212: What is the meaning of "active" in this sentence – the water column processes?

Line 303: What does "baseline conditions" mean in this context?

Figure 2: In the caption, it is worth defining what the solid arrows extending from bottom left to top right mean in panel c.

Reviewer #2

(Remarks to the Author)
Thank you for revising the manuscript so thoroughly. The new version is a lot clearer in terms of the different IC and OC sources and cycling pathways. The changes have largely addressed my previous comment about the relevance of allochthonous vs autochthonous sources and the importance of the overall ocean carbon budget. I also like the revised colour scheme of the figures. I think that the paper makes an important argument that really needs to be heard more loudly, so I fully support the publication.

I do have a minor comment about the paragraph starting in Line 249, which I find a little bit confusing. The overall argument is that recycling of purely autochthonous OC + IC generates no net change in pCO2, but that often only part of the autochthonous OC + IC are actually fully recycled. This means that POC and PIC (or their remineralization products, i.e. DIC + TA) can accumulate in sediments without being recycled back to the water column. My confusion is that in Line 253, the authors then write that such accumulation does not modify seawater carbonate chemistry on short timescales, unless the remineralization products are returned to the overlying seawater. But if a system is continually accumulating sedimentary pools of OC and IC that don't get remineralized and returned to the water column, then that does of course change seawater pCO2. The seawater pCO2 will only not show net changes if the autochthonous OC and IC are fully recycled and returned to the water column, as is stated in the following sentence (Line 255). I think this paragraph can probably be made much simpler by just clearly explaining the fact that in Scenario A there is only no net change in seawater carbonate chemistry if all OC + IC are indeed fully recycled and returned to the water column, but that in reality some fraction of this production might accumulate in the sediment, and depending on how large this fraction is, there would then be a greater or lesser net

change in seawater pCO2.

Reviewer #3

(Remarks to the Author)
I am happy with the changes made by the authors, I have just two comments I would hope they could address before acceptance:

Their framework implicitly assumes that any organic carbon that is transported outside of the meadow (lateral export) contributes to sequestration. This is not always the case, and much of that would be remineralised elsewhere. I hope that they could acknowledge this upfront and state that this is an assumption and their assessment almost always will overestimate the CO2 sequestration potential of seagrass meadows.

Some small comments:

L34: rephrase to 'particulary rich in carbonates'
L62: 'in the ground' -> 'in the seafloor'
L91: 'strongly' -> 'much'

Dear Dr Nezha MEJJAD,

We sincerely thank you for your careful evaluation of our manuscript, *'Revisiting Blue Carbon: How Organic–Inorganic Coupling Shapes CO_2 Fluxes in Seagrass Ecosystems*,' and for your constructive and insightful guidance. We also appreciate your time and effort in handling this manuscript.

We greatly appreciate the opportunity to revise and resubmit our work to Communications Earth & Environment. Below, we provide a point-by-point response to the editorial comments and suggestions, such as using Boxes, as well as detailed responses to the comments from the two anonymous reviewers and Dr. Sebastiaan van de Velde.

Editorial comment: *We expect perspective to be a focused and broadly accessible article that discusses recent literature on a rapidly-evolving topic: about at least half of the references should be from the past few years, and total word count (for the main text) should remain under 5,000 words.*

Response:
We have revised the manuscript to improve focus, clarity, and accessibility for a broad readership. The narrative has been streamlined to emphasize the key conceptual advances, and the reference list has been updated to incorporate recent literature, with approximately half of the cited studies drawn from the past decade (47 of 72 references from 2017–2026). This reflects the rapidly evolving nature of the field and aligns with recent developments in coastal carbon cycling (e.g., refs. 2, 6, 7–9, 13–15 and references therein). The manuscript has also been carefully edited to remain within the recommended word limit (5,000 words including figure captions).

Editorial comment: *We recommend that you condense mechanistic sections (e.g., the derivation of the NCP ratio).*

Response:
We have substantially condensed the mechanistic content by removing detailed derivations from the main text and relocating them to Box 1. This restructuring shifts the emphasis of the main narrative toward conceptual interpretation and broader implications, while retaining the essential mathematical framework in a concise and accessible format.

Editorial comment: *We suggest including boxes for accessibility and readability. For instance, you could highlight your novel proposal into boxes: the NCP compensation ratio and the source-dependent scenarios (A–D), which would also shorten the piece.*

Response:
Following this recommendation, we have introduced two conceptual boxes to improve clarity and highlight the core contributions of the study. Box 1 presents the NCP/NCC compensation ratio as a conceptual diagnostic framework, supported by a simplified mathematical expression. Box 2 outlines the source-dependent scenarios (A–D), which structure our framework for interpreting organic–inorganic carbon coupling. These additions enhance accessibility, reduce redundancy in the main text, and more clearly emphasize the novelty and generality of our approach.

Reviewer #1 (Remarks to the Author):

Summary:

In their article on "Revisiting Blue Carbon", Chou and co-authors suggest that an organic carbon-centric approach to evaluating net carbon storage in seagrass meadows fails to take into the account role of inorganic carbon cycling in carbon flows through these coastal ecosystems. In particular, net community calcification and both inorganic and organic matter sources (allochthonous vs. autochthonous) have important ramifications for CO2 source vs. sink estimates in seagrass meadows. The authors go on to propose a metric (the ratio of net community production to net community calcification, or NCP/NCC) that links inorganic and carbon cycling in these ecosystems, and a more qualitative and conceptual framework for considering how different combinations of allochthonous/autochthonous inorganic/organic carbon would impact net CO2 flows.

Response:

We sincerely thank the reviewer for the careful evaluation of our manuscript, *"Revisiting Blue Carbon: How Organic–Inorganic Coupling Shapes CO_2 Fluxes in Seagrass Ecosystems,"* and for the clear and insightful summary of our work. We appreciate the recognition that incorporating inorganic carbon cycling and carbon source dynamics is essential for accurately resolving CO_2 source–sink behavior in seagrass ecosystems. We fully agree with this perspective and emphasize that our study addresses this gap by integrating organic and inorganic carbon processes within a unified, process-based framework.

Below, we provide a point-by-point response to the reviewer's comments.

Major comments:

The major justification for this paper is that carbon accounting in blue carbon/seagrass ecosystems disproportionately focuses on NCP and carbon burial in sediments. This paper is largely about incorporating shifts in carbonate chemistry into the framework, which makes the discussion quite novel in itself. The introduction (Sect. 1) sets this up well; it is coherent and easy to follow, making it accessible for a variety of readers who do research, policy or decision-making in these coastal ecosystems. I can see it being influential to the field if revised and published, and comprehensive enough to be incorporated into future (coastal) oceanography textbooks.

Response:

We sincerely thank the reviewer for this positive and thoughtful assessment. We are encouraged that the manuscript's central motivation–addressing the limitations of organic carbon–centric blue carbon frameworks–is clearly recognized. A key contribution of this work is the explicit integration of carbonate chemistry and inorganic carbon cycling into blue carbon assessments, by linking NCP and NCC and incorporating carbon-source dependencies, thereby providing a more complete, process-based evaluation of CO_2 fluxes. We also appreciate your kind comments on the clarity and accessibility of the Introduction. In the revised manuscript, we have further sharpen the broader implications for carbon accounting and climate mitigation.

I found three major conceptual and/or structural aspects of the paper that need to be strengthened going forward. First, the justification that NCP alone is not "enough" for accurately quantifying carbon

sources/sinks in seagrass meadows needs some further clarification in Sect. 1. Over the open ocean, NCP can be decoupled from sediment burial due to different carbon flux integration depths. In this paper, what is the quantitative relationship between NCP and OC burial in sediments? Since seagrass meadows tend to be shallow, is NCP integrated over the entire water column such that it is tightly coupled to OC burial?

Relatedly, it seems that NCP and NCC must operate on similar time scales for the authors to propose a usable NCP/NCC metric in Sect. 2. Rates of seawater equilibration with atmospheric CO2 can differ from the rates of carbon burial in sediments. Rates of alkalinity production from respiration at the sediment-water interface can differ from organic matter production at the surface. Perhaps, again, these rates may be similar enough in a shallow seagrass meadow, but the discussion in lines 213-227 also suggest otherwise. Could the authors address time scale more explicitly in Sect. 1?

Response:

We thank the reviewer for this insightful comment. We agree that clarifying the relationship between NCP and sedimentary OC burial, as well as the role of process timescales, strengthens the conceptual framework of the manuscript. We have revised Section 1 (Lines 43–52 and 91–100) accordingly.

First, we now clarify that NCP, as is considered in the context of this paper, reflects short-term metabolic balance and does not directly quantify long-term sedimentary OC burial, though it indicates the potential for long-term preservation. As revised (Lines 43–52), only a fraction of organic production is ultimately preserved in sediments, with the remainder subject to respiration, export, and recycling. Although seagrass meadows are shallow and exhibit strong benthic–pelagic coupling**,** this does not imply a fixed or quantitative equivalence between NCP and burial. Rather, NCP sets an upper bound on potential carbon sequestration, while realized burial depends on additional processes/controls operating on a longer timescale, including remineralization efficiency, lateral transport, and allochthonous inputs. This clarification addresses the apparent coupling between water-column production and sedimentary storage.

Revised Text (L43 – L52):

While NCP is widely used as a proxy for ecosystem carbon balance, its relationship to long-term sedimentary OC burial is neither direct nor fixed, as much organic production is not retained locally. At the global scale, seagrass net primary production (NPP ≈ 490 Tg C yr^{-1}) is largely remineralized (~50%), with substantial export (~24%) and only ~16% buried within the meadows, highlighting that most production is not preserved as long-term carbon storage[5,6]. In the open ocean, this decoupling is more pronounced, as most organic matter is remineralized during export through the water column[7,8]. By contrast, shallow depths and strong benthic–pelagic coupling in seagrass meadows enhance sediment transfer of organic matter, yet substantial losses still occur through remineralization and lateral export[6,9]. Thus, NCP constrains the potential for carbon sequestration but does not determine its long-term magnitude or direction.

Second, we explicitly clarify the role of timescales (Lines 91–100). NCP and NCC vary over diel to seasonal cycles and regulate seawater carbonate chemistry and pCO_2, whereas air–sea CO_2 equilibration operates over hours (in very shallow systems) to weeks and sedimentary carbon burial integrates over annual to centennial timescales. Although strong benthic–pelagic coupling in shallow systems

facilitates the transmission of metabolic signals through the water column, these processes remain temporally distinct despite spatial coupling.

We further clarify that the NCP/NCC compensation ratio is intended as a diagnostic tool of short-term carbonate-system regulation and CO_2 source–sink behavior, rather than a direct predictor of long-term carbon burial. This distinction resolves the apparent mismatch in process timescales and avoids overinterpretation of the metric.

Revised Text (L91 – L100):

This divergence arises from the distinct yet overlapping timescales governing NCP, NCC, air–sea CO_2 exchange, and sedimentary carbon burial. In shallow seagrass meadows, strong benthic–pelagic coupling efficiently transmits metabolic signals through the water column, linking short-term metabolism to near-surface carbonate chemistry[6,28], with metabolic variability further modulated across environmental gradients[13]. NCP and NCC vary over diel to seasonal cycles[29,30], air–sea CO_2 equilibration occurs over hours (in extremely shallow systems) to weeks[31–33], and sedimentary burial integrates over annual to centennial timescales[2,12]. Thus, these spatially coupled systems are temporally decoupled, such that short-term metabolic CO_2 fluxes may diverge from long-term sequestration. Accordingly, NCP and NCC serve as diagnostics of short-term water-column CO_2 regulation, whereas climate mitigation depends on the fraction of carbon retained beyond remineralization.

Second, the suggestion of the NCP/NCC metric is both practical and novel. But, because the NCP/NCC metric only works in scenarios with purely autochthonous carbon production, its usefulness seems undermined by sections 3-4, which largely focus on seagrass meadows with mixed autochthonous and allochthonous carbon flows. The fact that the case study does not directly utilize the NCP/NCC metric seems to be at odds with the concluding statement in Sect. 5, lines 355-357. To amplify the importance of NCP/NCC in this paper, perhaps there is a way to provide a case study that directly utilizes NCP/NCC, or suggest how scenarios B-D in Sect. 3 would shift the NCP/NCC ratio relative to the compensation threshold (perhaps through a new panel e in Figure 2)?

Response:

We thank the reviewer for this important comment. We agree that the applicability of the NCP/NCC framework in systems influenced by mixed autochthonous and allochthonous carbon sources requires further clarification.

To address this, we have revised Lines 177-194 to explicitly clarify that the NCP/NCC compensation ratio is not restricted to purely autochthonous systems, but instead provides a generalizable, stoichiometric baseline for carbonate-system regulation. We now emphasize that, in natural seagrass ecosystems, allochthonous inputs and sedimentary processes modify DIC and TA, thereby shifting the system's position relative to the compensation threshold rather than invalidating the framework.

Specifically, we added mechanistic examples to illustrate this extension, including (i) dissolution of externally supplied $CaCO_3$, enhancing TA and CO_2 uptake, (ii) remineralization of allochthonous OC increasing DIC and CO_2 release, and (iii) sedimentary redox processes regulating alkalinity retention. These additions clarify that deviations from the compensation threshold reflect the influence of source-dependent and sedimentary processes superimposed on water-column metabolism.

We further revised the text to emphasize that the NCP/NCC ratio should be interpreted as a state-dependent diagnostic, rather than a fixed threshold, and that locally derived values provide more accurate assessments under variable environmental conditions.

Revised Text (Line 177 – Line 194):

Although the framework assumes fully autochthonous, water-column processes, natural seagrass ecosystems are influenced by allochthonous inputs and sedimentary transformations that modify DIC and TA, shifting states relative to the compensation threshold. These effects are pronounced in sediments, where external OC and IC drive coupled biogeochemical feedbacks regulating TA and DIC. For example, dissolution of allochthonous geogenic $CaCO_3$ increases TA and promotes CO_2 uptake, whereas remineralization of allochthonous OC elevates DIC and enhances CO_2 release. Sedimentary redox processes further regulate TA dynamics[48]. Sulfate reduction generates TA, but its net effect depends on sulfur cycling pathways. Sulfide may be buried as pyrite[20–23], preserving TA for export, or re-oxidized under oxygen supply (e.g., radial oxygen loss, bioturbation, tidal flushing)[49–51]. Re-oxidation generates protons that lower porewater TA and pH and can promote $CaCO_3$ dissolution[52]. While this dissolution enhances benthic TA fluxes[19], it may largely reflect internal carbonate recycling rather than a net TA source if the carbonates are modern biogenic.

Together, these externally driven modifications decouple carbonate chemistry from purely metabolic fluxes, such that the observed signal reflects an apparent NCP/NCC balance integrating metabolic, sedimentary, and source-dependent processes. Consequently, deviations from the compensation threshold capture these superimposed influences, with seawater pCO_2 reflecting their integrated effects. This underscores the need to extend the NCP/NCC framework to explicitly incorporate sedimentary dynamics and carbon-source dependencies, as developed in the following section.

Regarding the reviewer's suggestion to directly apply the NCP/NCC metric to the case study, we agree that directly applying the NCP/NCC metric would be particularly useful in systems dominated by purely autochthonous production. However, the Dongsha Inner Lagoon is characterized by strong sedimentary processes that decouple TA and DIC from purely metabolic fluxes. As such, the carbonate system cannot be interpreted within a closed, metabolism-only framework, and a direct calculation of the NCP/NCC ratio would not be mechanistically appropriate. Nonetheless, the Dongsha system clearly corresponds to an autotrophic, net-dissolving regime (NCP > 0; NCC < 0), which falls within the NCP/NCC framework (Sub-region I in the revised Fig. 2d), where a numerical threshold is not required to diagnose CO_2 uptake. Importantly, the case study is intended to provide a natural validation of the combined NCP–NCC compensation and source-dependent frameworks. It exemplifies conditions under which CO_2 uptake is maximized—namely, autotrophy coupled with net $CaCO_3$ dissolution, particularly where high autochthonous OC supply coincides with abundant allochthonous geogenic $CaCO_3$ (Scenario B, Fig. 3).

Revised Text (Line 318 – Line 321):

Within the NCP–NCC compensation and source–dependent control frameworks, a CO_2 sink is expected to be most pronounced in autotrophic systems with net $CaCO_3$ dissolution (NCP > 0; NCC < 0; Metabolic Regime I), particularly where high autochthonous OC supply coincides with abundant allochthonous geogenic $CaCO_3$ in sediments (Scenario B in Fig. 3).

Finally, the discussion in Sect. 3 is rich and has potential to grow into a full literature review alone. But, because it is being written in shorter form here, the authors have focused on specific mechanisms and scenarios, and it is not always clear why. For example, Scenarios A and D seem to be end-members in this conceptual model - are Scenarios B and C "end-members" too? It otherwise seems unrealistic to assume that autochthonous CaCO3 production is absent from scenario B and autochthonous OC production is absent from scenario C. In addition, why is only sulfate reduction discussed when other mechanisms of anaerobic OC respiration are possible in coastal sediments?

Response:

We thank the reviewer for this thoughtful and constructive comment. We agree that the conceptual framing of Section 3, particularly the presentation of the scenarios and the discussion of anaerobic pathways, required clarification.

First, we now explicitly clarify (Lines 229–237) that all four scenarios (A–D) represent idealized end-member configurations. These are designed to isolate the dominant controls of autochthonous versus allochthonous OC and $CaCO_3$ inputs on seawater carbonate chemistry, rather than to represent mutually exclusive or strictly realized conditions in natural systems. We emphasize that seagrass ecosystems span a continuum of overlapping processes, where multiple pathways co-occur but differ in relative importance. Accordingly, the revised text makes clear that the scenarios highlight dominant pathways without implying the absence of others. For example, we now explicitly state that autochthonous $CaCO_3$ production may still occur in Scenario B, and autochthonous OC production may persist in Scenario C.

Revised Text (Line 229 – 237):

These scenarios represent idealized end-members designed to isolate dominant pathways, although natural systems more likely span a continuum in which multiple processes co-occur. Accordingly, each scenario may still include contributions from both autochthonous and allochthonous OC and IC, but is defined by the predominance of a particular combination of inputs.

Across these scenarios, carbon-source configuration controls the pathways of carbonate-system modification, but not necessarily the climate-relevant outcome. Sustained ocean–atmosphere CO_2 removal arises only from processes that generate net TA and/or enhance long-term OC storage beyond baseline conditions. Distinguishing internal recycling from TA production driven by geogenic IC sources is therefore critical for resolving the true mitigation potential of seagrass ecosystems.

Second, regarding anaerobic pathways, we agree that sulfate reduction should not be interpreted as the sole mechanism of anaerobic OC remineralization. We have revised the text (Lines 259–261) to clarify that anaerobic respiration in marine sediments is typically dominated by sulfate reduction, while explicitly acknowledging that other pathways (e.g., denitrification and metal reduction) may also contribute. Sulfate reduction is emphasized because it is generally the quantitatively dominant pathway in marine coastal sediments and has a direct and well-constrained influence on alkalinity generation and carbonate-system dynamics, which is central to the framework developed in this study.

Revised text (Line 259 – 261)

Under oxygen-depleted conditions, sulfate reduction—a dominant form of anaerobic respiration—becomes the primary pathway of OC remineralization in marine sediments[54,58], although other processes (e.g., denitrification and metal reduction) may also contribute[59].

Minor, line-by-line comments:

Lines 127-137, 138-149: For consistency, it would help to explain if NCP/NCC >0.63 or <0.63 (the 400µatm iso-pCO2 threshold) in each bullet point, i-iii. This emphasizes the utility of the NCP/NCC calculation, which is otherwise quite clear earlier in the section.

Response:

We thank the reviewer for this helpful suggestion. We note that the revised manuscript explicitly indicates whether NCP/NCC is greater or less than the compensation threshold (≈0.63 at ~400 µatm) for all metabolic regimes (I–VI). Each case now clearly specifies the corresponding NCP/NCC condition, ensuring consistency and emphasizing the diagnostic utility of the framework. We have also shortened section 2.

Figure 2: What is the shading in the backgrounds of panels c and d? If there is no color bar or quantitative meaning behind the coloring, is it necessary? Also, it would help to divide panel d into six regions that match the six bullet points in lines 200-205 (e.g., regions I-VI that could then be referenced in those specific lines/bullet points)?

Response:

We thank the reviewer for this constructive suggestion. The background shading in panels c and d has now been clarified in the figure caption as a qualitative indicator of pCO_2 tendencies (blue: CO_2 uptake; orange: CO_2 release), rather than a quantitative scale.

Revised Text (Line 199 – L203):

(c) Conceptual framework linking in ΔTA and ΔDIC to the combined effects of organic metabolism (NCP) and carbonate cycling (NCC), and their control on seawater pCO_2. The dashed line represents the ΔTA:ΔDIC ratio of 1.226 required to maintain a constant pCO_2 (~400 µatm). Background color gradients indicate pCO_2 tendencies: stronger CO_2 uptake (blue) and CO_2 release (orange).

In addition, panel d has been explicitly subdivided into six regions (Fig. 2d), corresponding to the six metabolic regimes described in the text. These regions are now directly referenced and consistently aligned with the bullet points, improving the clarity and interpretability of the framework. The previous bullet labels (i–iii) have been replaced with metabolic regime classifications (I–VI) for consistency with the revised conceptual structure.

Revised text (Line 150 – L171)

Within this framework, distinct metabolic regimes emerge relative to the compensation threshold (Fig. 2 c,d). A net pCO_2 decrease occurs when DIC uptake and/or TA generation dominate (sub-regions I–III in Fig. 2d), including:

> *(I) autotrophy with net $CaCO_3$ dissolution (NCP > 0; NCC < 0), typical of rapidly growing seagrass meadows over carbonate sediments[25,42];*

(II) systems where both NCP and NCC are positive (NCP > 0; NCC > 0) but the NCP/NCC ratio exceeds the compensation threshold (NCP/NCC > 0.63), characteristics of highly productive water columns[43,44];

(III) heterotrophic systems (NCP < 0) that remain weak CO_2 sinks when TA generation offsets respiratory CO_2 release (NCC < 0; NCP/NCC < 0.63), as observed in organic-rich carbonate sediments[45].

In contrast, a net pCO_2 increase arises when respiration and/or calcification dominate (sub-regions IV–VI in Fig. 2d), including:

(IV) heterotrophy with net calcification (NCP < 0; NCC > 0), producing strong CO_2 sources such as in patchy seagrass in seagrass-coral reefs mosaics[46];

(V) systems where TA generation cannot offset respiratory CO_2 inputs (NCP < 0; NCC < 0; NCP/NCC > 0.63), as observed in some subtropical seagrass meadows[47]; and

(VI) autotrophic systems where calcification overwhelms photosynthetic CO_2 uptake (NCP > 0; NCC > 0; NCP/NCC < 0.63), leading to elevated pCO_2 during calcification-dominated conditions in carbonate seagrass meadows[24].

Sect. 3: Initially, I was confused by the reaction numbers that popped up in parentheses in this section. It would be helpful to cite Figure 3 when reaction 1 is first referenced in line 204 so readers know to link reaction number to that figure.

Response:

We thank the reviewer for this helpful suggestion. We have revised Section 3 to explicitly link the reaction numbering to Figure 3 at its first occurrence, improving clarity for the reader. Specifically, in Scenario A, we now state: "In Scenario A (Fig. 3A, see Table S1) (Line 239-240) … (reaction ①)" to make clear that the numbered reactions correspond directly to those illustrated in Figure 3. This clarification has been consistently applied across Scenarios B–D, ensuring that all reaction references are clearly tied to the corresponding figure elements.

Figure 3: I appreciate the work that went into making this figure. But, I think it could actually be more impactful as a table with scenarios A-D listed over different rows. In such a table, the chemical reactions and their impact on TA, DIC and pCO2 could then be explained over different columns. Reactions could still be numbered to match the text in Sect. 3.

Response:

Thank you for this helpful suggestion. We agree that a tabular format provides a clear and structured summary of the reactions and their effects on TA, DIC, and pCO_2.

To address this, we have included a table summarizing Scenarios A–D, with reaction numbering consistent with Section 3, in the Supplementary Information (Table S1).

We retain Fig. 3 in the main text as it provides a visual synthesis of the coupled processes and regime transitions, which is important for conveying the mechanistic framework. The supplementary table serves as a complementary quantitative reference.

Lines 313 and 344: The term “re-uptaken” reads awkwardly to me. Consider rephrasing.

Response:

We thank the reviewer for this helpful suggestion. We have revised the manuscript to replace the term “re-uptaken” with “taken up” at the indicated locations to improve clarity and readability while preserving the intended meaning of repeated biological uptake. We have revised this in Lines 352 and 383.

Reviewer #2 (Remarks to the Author):

The manuscript by Natividad et al. presents a framework for analysing the combined effects of net organic matter production and net calcification to understand the overall carbon balance of seagrass beds. This is an important problem, because as the authors argue, there has been too much emphasis purely on the organic matter accumulation in such Blue Carbon ecosystems without considering the alkalinity dynamics. The framework is presented and explained well, the manuscript is clearly written, and I think it's appropriate for the journal.

Response:

We sincerely thank the reviewer for the careful evaluation of our manuscript and for the positive and encouraging feedback. We appreciate the recognition of the importance of integrating net organic production and net calcification to better resolve the overall carbon balance of seagrass ecosystems.

We fully agree that alkalinity dynamics are a critical yet often overlooked component in Blue Carbon assessments, and we are encouraged that the proposed framework is viewed as a meaningful step toward addressing this gap. We also appreciate the reviewer's comments regarding the clarity and presentation of the manuscript, as well as its suitability for the journal.

Below, we provide a point-by-point response to the reviewer's comments.

The one aspect that I find a bit problematic is the emphasis on autochthonous versus allochthonous OC and $CaCO_3$. I agree with the authors that it is very relevant for the local-scale CO_2 flux within a given seagrass meadow whether these inputs to the sediments are from local production or whether they are advected to the site. However, I think that misses the bigger picture, which is how the overall ocean–atmosphere CO_2 balance is affected. Essentially, as I understand the authors, the authors are saying that a seagrass meadow will be a strong carbon sink if $CaCO_3$ is produced off-site (e.g. by a nearby coral reef), advected to the seagrass meadow, and then dissolves in the seagrass. However, from the point of view of the ocean as a whole this is no different than if the $CaCO_3$ was produced and recycled within the meadow: for the ocean as a whole, the same quantity of $CaCO_3$ has been precipitated and then redissolved. The only difference is that a seagrass meadow that dissolved externally supplied $CaCO_3$ will have lower pCO_2 within the meadow compared to one where $CaCO_3$ is produced and recycled locally. It is just than in the former case, the effect of alkalinity precipitation on pCO_2 happens elsewhere and is then not accounted for in the local carbon balance of the meadow. So I think the relevant question is not so much about the geographical origin of the OC and the $CaCO_3$, but rather whether a given seagrass meadow leads to more alkalinity production within the ocean as a whole than without the meadow. The same logic also applies, I think, to OC remineralisation: I think that whether a seagrass meadow leads to remineralization or burial of allochthonous OC versus autochthonous OC is less important than whether overall the seagrass leads to more or less net burial of OC, regardless of where it is produced. For example, a given seagrass meadow might receive a lot of allochthonous OC that is then respired in the seagrass meadow, but that allochthonous OC might be ultimately respired anyway even without the seagrass meadow. Possibly, the presence of the seagrass might actually help a larger fraction of the allochthonous OC to get buried thanks to the anoxic sediments.

I would therefore recommend that the authors consider taking a larger view of the overall carbon balance beyond just the immediate boundary of a given meadow, and clarify that the important consideration should be whether maintaining / restoring seagrass in a given area will lead to beneficial overall changes in NCP and NCC, considering the net carbon balance of the seagrass and of the sites where allochthonous inputs originate from. I think this could be done with a relatively moderate amount of rewriting and clarification, because the framework of thinking about NCP vs NCC per se is robust.

Response:

We sincerely thank the reviewer for the positive assessment of our NCP/NCC compensation framework and for raising this insightful and important point regarding the broader ocean–atmosphere CO_2 balance. We fully agree with the reviewer's premise that the key question is whether the presence of a seagrass meadow leads to a net increase in alkalinity production and/or carbon sequestration at the scale of the ocean system, relative to baseline conditions without the meadow.

In response, we have revised the manuscript to explicitly clarify that our framework is not intended to assess carbon dynamics solely within the spatial footprint of an individual seagrass meadow. Rather, it is designed to diagnose how seagrass ecosystems modify coupled organic–inorganic carbon cycling relative to baseline conditions at the system scale. Accordingly, we now emphasize that the climate relevance of seagrass ecosystems depends on whether they enhance net alkalinity generation and/or long-term carbon retention in the ocean, rather than simply redistributing processes spatially within the coastal zone.

We also appreciate the opportunity to clarify an important aspect that was not sufficiently explained in the original manuscript. Specifically, we now explicitly distinguish between contemporary biogenic and geogenic (pre-existing) sources of allochthonous $CaCO_3$. When $CaCO_3$ is produced contemporaneously within the coastal ocean (e.g., reef-derived), its subsequent dissolution largely represents internal recycling of alkalinity and dissolved inorganic carbon, as CO_2 uptake during dissolution offsets CO_2 release during calcification. As such, this process results in little to no net addition of alkalinity at the ocean scale, consistent with the reviewer's point.

In contrast, dissolution of geogenic $CaCO_3$ (e.g., coral sands formed over geological timescales) represents the mobilization of a long-term carbonate reservoir that is not part of the contemporary carbonate production cycle. On timescales relevant to climate mitigation (decades to centuries), seagrass-mediated dissolution of such geogenic carbonates can generate a net increase in total alkalinity in the water column, thereby enhancing buffering capacity and promoting atmospheric CO_2 uptake. This distinction is now explicitly incorporated in Section 3 (Lines 208–222) as part of the revised framework.

Furthermore, following the reviewer's suggestion, we have strengthened the manuscript to adopt a broader carbon-budget perspective that extends beyond the meadow boundary to include connected source regions supplying allochthonous inputs. In this context, we now explicitly state that the relevant metric is whether seagrass presence alters net carbon fluxes—through changes in organic production (NCP), calcification (NCC), and alkalinity generation—relative to baseline conditions, rather than the geographical origin of carbon inputs per se.

Revised Text (Line 208 – 222):

Sedimentary sources exert a critical yet underappreciated control on carbon dynamics in seagrass ecosystems. While the NCP–NCC balance governs CO_2 exchange in the seawater column, carbonate chemistry is also strongly shaped by the origin (autochthonous versus allochthonous) and temporal nature (modern biogenic versus geogenic) of sedimentary carbon pools, which determine their coupling to the active carbon cycle and their influence on DIC and TA.

This control is expressed through carbonate mineral composition. Carbonate may derive from modern biogenic production[16,22] or from geogenic sources formed over geological past[34,53], a distinction central to pCO_2 regulation. Dissolution of recently precipitated $CaCO_3$ largely reflects internal recycling, yielding limited net CO_2 uptake under most conditions[30,53]. In contrast, dissolution of geogenic carbonates mobilizes TA from long-term reservoirs, enhancing buffering capacity and promoting CO_2 uptake[25,26,54], consistent with natural ocean alkalinization driven by mineral weathering[55]. Recent evidence shows that carbonate dissolution can be dynamically stimulated under rising atmospheric

CO_2, particularly in shallow shelf environments where strong sediment–water coupling enables rapid TA feedbacks[27]. Thus, resolving both carbon origin and carbonate age is essential for evaluating seagrass climate mitigation potential.

To further address the recommendation for a system-scale perspective, we have revised the manuscript to incorporate recent empirical and modeling evidence demonstrating how seagrass restoration modifies carbon fluxes relative to baseline conditions.

Specifically, we now include evidence showing that restored seagrass meadows can shift benthic systems toward net autotrophy relative to adjacent unvegetated sediments, enhancing carbon uptake despite variability in calcification dynamics (Bandibas-Natividad et al., 2025). We further strengthen this framework by incorporating studies that capture both the magnitude and variability of restoration responses, including increases in net community production (Egea et al., 2023), enhanced sediment organic carbon accumulation (Greiner et al., 2013; Moritsch et al., 2025), and cases in which restored systems remain net heterotrophic under certain environmental conditions (Kindeberg et al., 2024).

These additions clarify that the climate mitigation potential of seagrass ecosystems is not intrinsic, but emerges from net system-scale modification of carbon fluxes relative to baseline conditions, consistent with the reviewer's recommendation.

Specifically, we have added the following statements in Section 3 (Lines 300-311):

In summary, the mitigation potential of seagrass ecosystems is governed not only by the balance between NCP and NCC in the overlying water column, but also by sedimentary carbon-source dynamics. Crucially, the net effect depends on whether seagrass presence enhances TA generation or carbon retention relative to baseline conditions. Recent work shows that restored seagrass meadows can shift benthic systems toward net autotrophy relative to adjacent unvegetated sediments, enhancing carbon uptake despite concurrent calcification, as OC production exceeds CO_2 release[44]. Consistently, NCP in developing meadows can increase by an order of magnitude relative to bare sediments, indicating a pronounced metabolic shift following restoration[60]. Restoration also promotes the accumulation of sediment OC as meadow structure develops, reinforcing long-term carbon storage[61,62]. However, such responses are not universal; restored temperate meadows may remain net heterotrophic, underscoring the role of local biogeochemical conditions and baseline states in determining carbon balance[63]. Climate relevance thus depends on net system-scale modification of carbon fluxes relative to baseline conditions.

Additional minor comments:

Line 122-124: that's true, although reductions in NCC at increasing pCO2 levels will also ameliorate this effect a bit; maybe that should be acknowledged?

Response:

We thank the reviewer for this insightful comment. We agree that reductions in net community calcification (NCC) under elevated pCO_2 conditions may partially ameliorate the increasing organic production required to offset calcification-driven CO_2 release. This moderating effect has now been explicitly incorporated into the revised text in Box 1 L149.

Box 1 (Line 149):

Across 100–600 µatm, the compensation ratio ranges from ~0.37 to 0.71 (Fig. 2b), demonstrating sensitivity to ambient carbonate chemistry. Consequently, greater organic production is required to

offset calcification-driven CO_2 release under elevated pCO_2 conditions, consistent with decreased buffer sensitivity (i.e., increased CO_2 sensitivity; Revelle factor)[39,40], although reduced calcification rates under ocean acidification may partially moderate this effect[41].

Line 224–227: isn't this just a rather complicated way of explaining that it is the NET production / calcification for the system as a whole that matters?

Response:

Thank you for this helpful comment. We agree that, at its core, the carbonate-system response is governed by the net balance between organic production and calcification at the system scale. Our original wording aimed to clarify an additional nuance: that internally recycled carbon does not necessarily translate into an immediate change in pCO_2, whereas processes such as burial or longer-term retention ultimately determine the net effect on the system carbon pool.

To improve clarity and align with the reviewer's suggestion, we have simplified the text to more directly emphasize the system-level balance while retaining this distinction.

Revised text (Lines 255–258):

Accordingly, reaction ④ does not alter pCO_2 at the system scale for the internally recycled fraction, whereas the overall carbonate-system response depends on the extent of recycling versus burial over relevant timescales. Thus, canonical stoichiometries represent theoretical endmembers, while in situ responses reflect this balance.

Reviewer #3 (Remarks to the Author):

This perspective by Natividad et al. nicely presents a novel conceptual framework to assess the CO2 removal potential of seagrass meadows. The theoretical background is elegantly presented and will be useful for the interpretation of carbonate chemistry data of other field studies (especially Figure 2). The framework will also be very useful for the current blue carbon rush, and a complete and nuanced assessment like the one presented here is - in my opinion – long overdue. I have a few comments to further strengthen the framework, and a few inconsistencies that should be addressed, but overall I am supportive of the publication of this article after revisions.

Kind regards

Sebastiaan van de Velde

Response:

We sincerely thank Dr. van de Velde for this positive and encouraging assessment. We are pleased that the novelty, clarity, and timeliness of the proposed framework are recognized, particularly its utility for interpreting carbonate chemistry data and informing blue carbon assessments. We agree that a more complete, process-based framework is needed in this field and appreciate your support for this contribution.

We have carefully addressed your constructive comments and revised the manuscript to improve clarity and resolve the noted inconsistencies. Please see below for our point-by-point responses.

Thank you again for your thoughtful feedback and support.

General comments

I am not sure why you limit yourself to seagrass meadows, you could easily expand this framework to any coastal ecosystem, at the very least other vegetated ecosystems like mangroves and salt marshes.

Response:

Thank you for this insightful comment. We agree that this conceptual framework could, in principle, be extended to other coastal ecosystems such as mangroves and salt marshes, and that similar mechanisms may also operate in these systems. However, in this study, we intentionally focus on seagrass meadows to maintain a clear system definition and mechanistic interpretation, as our discussion and case study are centered on seagrass ecosystems.

To address this point, we have added Section 6 (System-scale considerations and applications) to explicitly discuss the broader applicability of the framework. We now state that the framework is transferable to other coastal systems, while emphasizing that its local expression remains strongly system-dependent, requiring context-specific evaluation of sediment composition, hydrodynamic setting, and carbon-source configuration.

In addition, we retain the focus on seagrass systems while highlighting future research directions, including the need to incorporate lateral outwelling and hydrodynamic connectivity to better constrain net ecosystem carbon balances beyond local system boundaries.

Added Text (Line 423-439):

6. System-Scale Considerations and Applications

Although this framework is developed in the context of seagrass ecosystems, the coupling between organic metabolism, carbonate dynamics, and sedimentary redox processes is broadly applicable across coastal systems, including mangroves, salt marshes, and reef-associated systems. However, the relative importance of these processes varies with sediment composition, hydrodynamic setting, and carbon-source configuration, requiring system-specific evaluation. Future work on seagrass should explicitly resolve lateral carbon transport, an increasingly recognized, currently poorly constrained component of the blue carbon cycle. Emerging evidence suggests that TA outwelling can rival OC burial, revealing a substantial yet overlooked sequestration pathway[72].

Ultimately, advancing blue-carbon science requires a shift from single-process metrics (e.g., OC burial) toward multi-process, system-scale frameworks that resolve the full carbonate system. Integrating hydrodynamic connectivity into carbon accounting is essential to quantify the distal fate of exported TA and DIC, constrain the true atmospheric CO_2 uptake signal, and evaluate whether local sinks translate into regional or global climate benefits. This integration enables a more complete representation of coastal carbon cycling by capturing both organic and inorganic pathways and their interactions across spatial and temporal scales, providing a more mechanistic and scalable basis for evaluating coastal ecosystems in climate mitigation strategies.

You will need to clarify upfront what the boundaries of the systems are. Your point of not taking into account allochtonous carbon is valid, but then you do include this in your different scenarios. To be consistent, you will also need to account for lateral transport of seagrass OC/IC out of the system. This of course makes the assessment very complicated, as you will then have to account for transport and hydrodynamics, but it is this level of nuance that we are missing in the current discussions.

Response:

Thank you for this insightful comment. We agree that explicitly accounting for lateral transport and hydrodynamic processes is essential for a fully resolved system-scale carbon budget. In this study, we adopt a locally defined control volume to isolate and clarify the underlying biogeochemical mechanisms governing coupled organic–inorganic carbon dynamics within seagrass ecosystems. Within this framework, lateral exchanges are not explicitly resolved, and the presented scenarios are intended to represent process-based endmembers rather than complete system budgets.

We have revised the manuscript to clarify this system boundary upfront and to ensure consistency in how allochthonous inputs and lateral fluxes are treated. In addition, we now explicitly acknowledge that lateral transport of both OC and IC—including alkalinity export—can substantially modify the net carbon balance beyond the local system.

Revised Text (Line 428-431):

Future work on seagrass should explicitly resolve lateral carbon transport, an increasingly recognized, currently poorly constrained component of the blue carbon cycle. Emerging evidence suggests that TA outwelling can rival OC burial, revealing a substantial yet overlooked sequestration pathway[72].

Coupling air-sea exchange directly to pCO2 is risky – exchange might happen in well-mixed systems, but aside from equilibration timescales that move the CO2 uptake outside of your boundaries, you could also have subduction that prevent re-equilibration 1. It might be more robust to focus on pCO2 alone and leave the air-sea exchange as 'potential air-sea CO2 uptake'.

Response:

Thank you for this insightful comment. We agree that directly linking air–sea CO_2 exchange to pCO_2 may be uncertain, particularly in systems where equilibration is incomplete or physical processes (e.g., subduction) limit re-equilibration.

In response, we have revised the manuscript to clarify this distinction and now consistently refer to air–sea fluxes as "potential air–sea CO_2 uptake," emphasizing that our framework primarily diagnoses carbonate-system state (pCO_2) rather than realized fluxes. This clarification has been incorporated in Sections 2 and 3.

The sulfate reduction and TA production is not consistently described throughout the paper – half of the time you seem to suggest that it is always a TA source, other times you correctly add the caveat that this only works if S precipitates and remains in the sediment.

Response:

Thank you for this important observation. We agree that the original manuscript did not consistently distinguish between gross alkalinity generation during sulfate reduction and the conditions required for net alkalinity preservation. We have revised the manuscript to clarify that sulfate reduction is a conditional alkalinity source: while it initially generates alkalinity, net alkalinity is only preserved when reduced sulfur is buried (e.g., as FeS or FeS_2). In contrast, sulfide re-oxidation consumes this alkalinity, resulting in no net gain. This distinction is now applied consistently across Sections 1–3, with revised text explicitly clarifying the role of sulfur cycling in regulating net alkalinity and its implications for carbonate dissolution and internal recycling.

In Section 1, we revise the text to emphasize that (Line 77 – 80):

In the deeper sediments, anaerobic respiration, dominated by sulfate reduction and, to a lesser extent, denitrification and iron reduction, produces HCO_3^-, increasing both TA and DIC in porewaters; however, net TA is only preserved when reduced species are buried or escape re-oxidation (e.g., via pyrite formation)[20–23].

In Section 2 (Lines 182–188), we clarify that sedimentary redox processes regulate alkalinity dynamics through sulfur cycling pathways, explicitly distinguishing between alkalinity preservation via sulfide burial and its consumption during re-oxidation, and noting the implications for carbonate dissolution and internal alkalinity recycling.

Sedimentary redox processes further regulate TA dynamics[48]*. Sulfate reduction generates TA, but its net effect depends on sulfur cycling pathways. Sulfide may be buried as pyrite*[20–23]*, preserving TA for export, or re-oxidized under oxygen supply (e.g., radial oxygen loss, bioturbation, tidal flushing)*[49–51]*. Re-oxidation generates protons that lower porewater TA and pH and can promote $CaCO_3$ dissolution*[52]*. While this dissolution enhances benthic TA fluxes*[19]*, it may largely reflect internal carbonate recycling rather than a net TA source if the carbonates are modern biogenic.*

In Section 3 (Lines 259–269), we reinforce this consistency by explicitly stating:

Under oxygen-depleted conditions, sulfate reduction—a dominant form of anaerobic respiration—becomes the primary pathway of OC remineralization in marine sediments[54,58], although other processes (e.g., denitrification and metal reduction) may also contribute[59]. When fueled by autochthonous OC (reaction ①), sulfate reduction initially increases both DIC and TA. Crucially, this TA gain is only preserved if reduced sulfur is buried (e.g., as FeS or FeS_2). If H_2S is instead re-oxidized in near-surface sediments or overlying water, this consumes the produced TA and promotes carbonate dissolution, with the net effect depending on carbonate source[48,49,51,52]. In contrast, the stable burial of reduced sulfur (e.g., via reaction with Fe^{2+}) locks in the TA increase, thereby enhancing the system's CO_2 uptake potential[22,23] (reaction ⑤). Consequently, Scenario A results in a nearly balanced state where $\Delta DIC \approx 0$ and $\Delta TA \approx 0$ for the fully recycled fraction, whereas a net TA surplus arises only when reduced-sulfur burial is sustained.

Specific comments

L34: 'blue-carbon sequestration' -> 'carbon sequestration'

Response:

Thank you for this helpful suggestion. We have revised the text accordingly and replaced "blue-carbon sequestration" with "carbon sequestration" on Line 37.

L36: I would temper that statement – some meadows like Poseidonia in the Mediterranean might reach those levels, but a lot of temperate meadows do not

Response:

Thank you for pointing out this comment. We have revised the statement to avoid overgeneralization and to better reflect variability among seagrass systems.

Revised Text (Line 37-40):

"Despite occupying <0.1% of the global ocean, they account for ~10–25% of coastal organic carbon (OC) burial (27–44 Tg C yr^{-1}) and accumulate sedimentary OC at rates comparable to, and in some high-biomass systems exceeding, those of terrestrial forests[3,4]."

L41: aerobic respiration – anaerobic respiration would affect alkalinity differently

Response:

Thank you for pointing this out. The text has been revised and now reads (Line 56-58):

The same effect on TA also occurs during aerobic respiration, which releases CO_2 and increases both DIC and pCO_2[10,11]. In contrast, anaerobic pathways in the ground can exert additional control on TA.

L42: clarify that this is only when you define your system as the meadow + seafloor itself – this does not take into account the role of lateral OC transport which makes assessing the role of these meadows

more complicated (which is also something that is commonly overlooked and you could highlight here as well)

Response:

Thank you for this important comment. We agree that the interpretation of NCP as an indicator of carbon source or sink behavior depends on the definition of system boundaries. In the revised manuscript, we now explicitly clarify that this interpretation applies to a locally defined meadow–seafloor system and does not account for lateral transport of organic carbon. We further highlight that such lateral exchanges can substantially influence the net coastal carbon balance but are often overlooked in organic-centric blue carbon frameworks.

Revised Text (Line 58 – 64):

Meadows function as carbon sinks when photosynthesis exceeds respiration (NCP > 0) and as carbon sources when respiration dominates (NCP < 0), with neutral metabolic balance when NCP ≈ 0 (Fig. 1)[12,13]. However, this interpretation assumes a local meadow–sediment system boundary and does not account for lateral OC transport, which can significantly modify the net coastal carbon balance. As such, organic-centric framing underpins most blue-carbon assessments—yet it implicitly assumes that carbonate chemistry responds passively to metabolism, despite evidence that it is regulated by multiple interacting processes[1,14].

L47: is this true for all seagrasses or just certain ones?

Response:

Thank you for this helpful comment. We have revised the statement to clarify that substantial inorganic carbon (IC) reservoirs are not universal across all seagrass systems, but are most pronounced in tropical, carbonate-rich environments. The text (Lines 65–67) has been updated accordingly to reflect this context.

Revised Text (Line 65-67):

Seagrass ecosystems also host large reservoirs of inorganic carbon (IC) in the form of calcium carbonate stored within sediments, particularly in tropical carbonate-rich environments, and these pools can exert a first-order control on air–sea CO_2 exchange[15].

L60: this statement is not entirely accurate – the production of reduced species like H2S creates TA, but as long as there is no formation of reduced minerals, H2S will be reoxidized and the TA will be lost again. There can be a decoupling in the sediment, but unless the TA consumption due to reoxidation drives CaCO3 dissolution more than the TA production at depth drives CaCO3 production, there will be a net zero effect on CO2 uptake (unless the H2S is captures by Fe to form FeS or FeS2).

Same for Figure1: the reactions there are not correct as you do not account for the reoxidation of H2S (you mention this yourself at L160).

Response:

Thank you for this important clarification. We agree that alkalinity generated via sulfate reduction does not necessarily represent a net alkalinity source, as it may be transient if reduced species such as H_2S

are subsequently re-oxidized. Net alkalinity generation is only realized when reduced sulfur is effectively removed from the system, for example, through burial as metal sulfides (e.g., FeS or FeS_2).

In response, we have revised the manuscript to consistently distinguish between gross alkalinity generation and net alkalinity preservation, emphasizing that the net effect depends on the fate of reduced sulfur.

Revised text (Line 77-80):

In the deeper sediments, anaerobic respiration, dominated by sulfate reduction and, to a lesser extent, denitrification and iron reduction, produces HCO_3^-, increasing both TA and DIC in porewaters; however, net TA is only preserved when reduced species are buried or escape re-oxidation (e.g., via pyrite formation)[20–23].

We have also revised Figure 1 and its associated reactions to explicitly account for the re-oxidation of reduced sulfur, clarifying that net alkalinity generation occurs only when reduced species are stabilized or removed from the system.

[figure redacted]

L65: reference 15 has not proven this, this is a (very) poorly constrained model study that suggests this could be the case – which also only considers the benthic environment and not the potential change in calcification in the water column. I would rephrase this sentence, or find a reference that has shown this occurring in real life

Thank you for this important comment. We agree that the previous reference relied on a model-based estimate that remains insufficiently constrained and does not fully account for coupled benthic–pelagic feedbacks, including potential changes in water-column calcification.

In response, we have removed the model-based citation and replaced it with recent empirical and synthesis-based evidence. Specifically, we now cite Goossens et al. (2026), which provides an updated, observation-constrained estimate of coastal and shelf $CaCO_3$ dissolution (~11 ± 9 Tmol yr^{-1}), highlighting its non-negligible contribution to ocean alkalinity generation and CO_2 uptake.

Revised text (Line 84-87)

At broader scales, sedimentary $CaCO_3$ dissolution enhances TA and buffering capacity, thereby promoting CO_2 uptake and contributing non-negligibly to the ocean carbon sink, with coastal and shelf dissolution estimated at ~11 ± 9 Tmol yr^{-1} (ref 26).

L86: is that true? What do you mean by 'geogenic' ? There are studies from the Baltic Sea that show that erosion of carbonate-rich clays lead to IC input that subsequently dissolves 2,3

Response:

Thank you for this important comment. We agree that the term *geogenic* required clearer definition and that geogenic carbonate can influence seawater carbonate chemistry through erosion, transport, and subsequent dissolution, as demonstrated in Baltic Sea studies (e.g., Wallmann et al., 2022).

In the revised manuscript, we clarify that *geogenic carbonate* refers to carbonate minerals derived from long-term geological reservoirs (e.g., carbonate-rich sediments, lithogenic particles, or reworked deposits) rather than produced in situ by contemporary biological processes. We now explicitly acknowledge that such material can be remobilized and dissolved, thereby contributing to alkalinity generation and influencing CO_2 dynamics.

These clarifications have been incorporated into the revised manuscript (Lines 112-114, 213-215 in Section 3, Lines 119-123), and the suggested reference has been included.

Revised text (Lines 112 – 114)

Beyond the context-dependent balance of NCP and NCC, CO_2 dynamics are additionally modulated by the origin and timescale of the carbon inputs (autochthonous versus allochthonous; modern versus geogenic—recently formed vs. geologically derived $CaCO_3$).

Revised text (Lines 213-215)

This control is expressed through carbonate mineral composition. Carbonate may derive from modern biogenic production[16,22] or from geogenic sources formed over geological past[34,53], a distinction central to pCO_2 regulation.

Revised text (Lines 119-123)

Importantly, burial of geogenic carbonate is biogeochemically distinct because its eventual dissolution releases TA derived from long-term geological reservoirs, altering seawater chemistry and TA fluxes[34]. Including such carbonate in carbon stock assessments can therefore bias CO_2 flux estimates by conflating geologically sourced TA inputs with biologically mediated carbon emissions[20].

L122: this is well established from previous work by Frankignouille and the Revelle factor, so I suggest included citations 4,5 and earlier work by Revelle

Response:

Thank you for this helpful suggestion. We agree that the dependence of CO_2 sensitivity on carbonate-system buffering is well established, particularly through the Revelle factor and earlier work by Frankignoulle and colleagues (Citations 39,40). We have revised the manuscript to explicitly acknowledge this foundation and have incorporated the suggested references.

Box 1 (Line 149):

Across 100–600 µatm, the compensation ratio ranges from ~0.37 to 0.71 (Fig. 2b), demonstrating sensitivity to ambient carbonate chemistry. Consequently, greater organic production is required to offset calcification-driven CO_2 release under elevated pCO_2 conditions, consistent with decreased buffer sensitivity (i.e., increased CO_2 sensitivity; Revelle factor)[39,40]*, although reduced calcification rates under ocean acidification may partially moderate this effect*[41].

L127: I guess you mean DIC uptake and not organic carbon?

Response:

Thank you for this clarification. We agree that "DIC uptake" is the appropriate term in the context of carbonate-system dynamics. We have revised "organic carbon uptake" to "DIC uptake" for clarity and accuracy.

Revised text (Line 151–152):

A net pCO_2 decrease occurs when DIC uptake and/or alkalinity generation dominate (sub-regions I–III in Fig. 2d), including:

L141: see my main comment, I would extend your framework to all coastal systems, since coral reefs are arguably not seagrass meadows

Response:

Thank you for this important and constructive comment. We agree that coral reefs alone fall outside the primary scope of this Perspective, which is centered on seagrass ecosystems. In response, we have revised the example to focus on seagrass–coral reef mosaic systems, which more appropriately represent mixed carbonate environments where seagrass is present and actively contributes to carbon dynamics. This ensures consistency with the scope of the study while retaining relevance to carbonate-rich settings.

We also agree that the conceptual framework developed here is not limited to seagrass ecosystems. While seagrass meadows provide a tractable system to develop and illustrate the framework, the governing processes—namely the coupling between organic metabolism (NCP) and carbonate cycling (NCC), together with sedimentary redox dynamics—are broadly applicable across coastal environments.

To address this, we have added a dedicated section (Section 6: System-Scale Considerations and Applications) explicitly stating that this framework can be extended to other coastal ecosystems, including mangroves, salt marshes, and reef-associated systems, while emphasizing that the relative importance of these processes depends on sediment composition, hydrodynamic setting, and carbon-source configuration.

The revised text (Line 163–164) now reads:

(IV) heterotrophy with net calcification (NCP < 0; NCC > 0), producing strong CO_2 sources such as in patchy seagrass in seagrass-coral reefs mosaics[46];

Lines 423-428

6. System-Scale Considerations and Applications

Although this framework is developed in the context of seagrass ecosystems, the coupling between organic metabolism, carbonate dynamics, and sedimentary redox processes is broadly applicable across coastal systems, including mangroves, salt marshes, and reef-associated systems. However, the relative importance of these processes varies with sediment composition, hydrodynamic setting, and carbon-source configuration, requiring system-specific evaluation.

L142: it is unclear to me how a seagrass meadow becomes heterotrophic without including the allochtonous contribution? It seems to me that you are not following your own logic?

Response:

Thank you for this helpful comment. We agree that heterotrophy in natural seagrass systems is often associated with allochthonous organic matter inputs. However, in this framework, heterotrophy is defined strictly as a metabolic state (i.e., NCP < 0), which arises when community respiration exceeds gross primary production within the defined system, regardless of carbon origin.

Importantly, this condition does not require external organic inputs, as heterotrophy can also emerge from the remineralization of internally produced or previously stored autochthonous organic carbon, as well as from temporal imbalances between production and respiration (e.g., net autotrophy during periods of high production followed by heterotrophy when accumulated organic carbon is respired).

In addition, as clarified in the revised manuscript (Lines 236–245), the scenarios represent idealized end-member configurations designed to isolate dominant processes, rather than fully realized natural conditions. Accordingly, while allochthonous inputs commonly contribute to heterotrophy in situ, they are not a prerequisite for defining the metabolic state within this conceptual framework.

Revised text (Line 229–237):

These scenarios represent idealized end-members designed to isolate dominant pathways, although natural systems more likely span a continuum in which multiple processes co-occur. Accordingly, each scenario may still include contributions from both autochthonous and allochthonous OC and IC, but is defined by the predominance of a particular combination of inputs.

Across these scenarios, carbon-source configuration controls the pathways of carbonate-system modification, but not necessarily the climate-relevant outcome. Sustained ocean–atmosphere CO_2 removal arises only from processes that generate net TA and/or enhance long-term OC storage beyond baseline conditions. Distinguishing internal recycling from TA production driven by geogenic IC sources is therefore critical for resolving the true mitigation potential of seagrass ecosystems.

L164: 'further increasing alkalinity export' – you seem to suggest that sulfate reduction coupled to sulfide reoxidation creates TA, which is not correct. I suggest rephrasing these sentences.

Response:

Thank you for this important comment. We agree that the previous wording could be interpreted as implying that sulfate reduction coupled to sulfide re-oxidation generates a net alkalinity source. We have revised the text to clarify that although sulfate reduction initially generates alkalinity, its net expression depends on the fate of reduced sulfur.

We have revised the sentence for clarity as follows (Line 182 – 188):

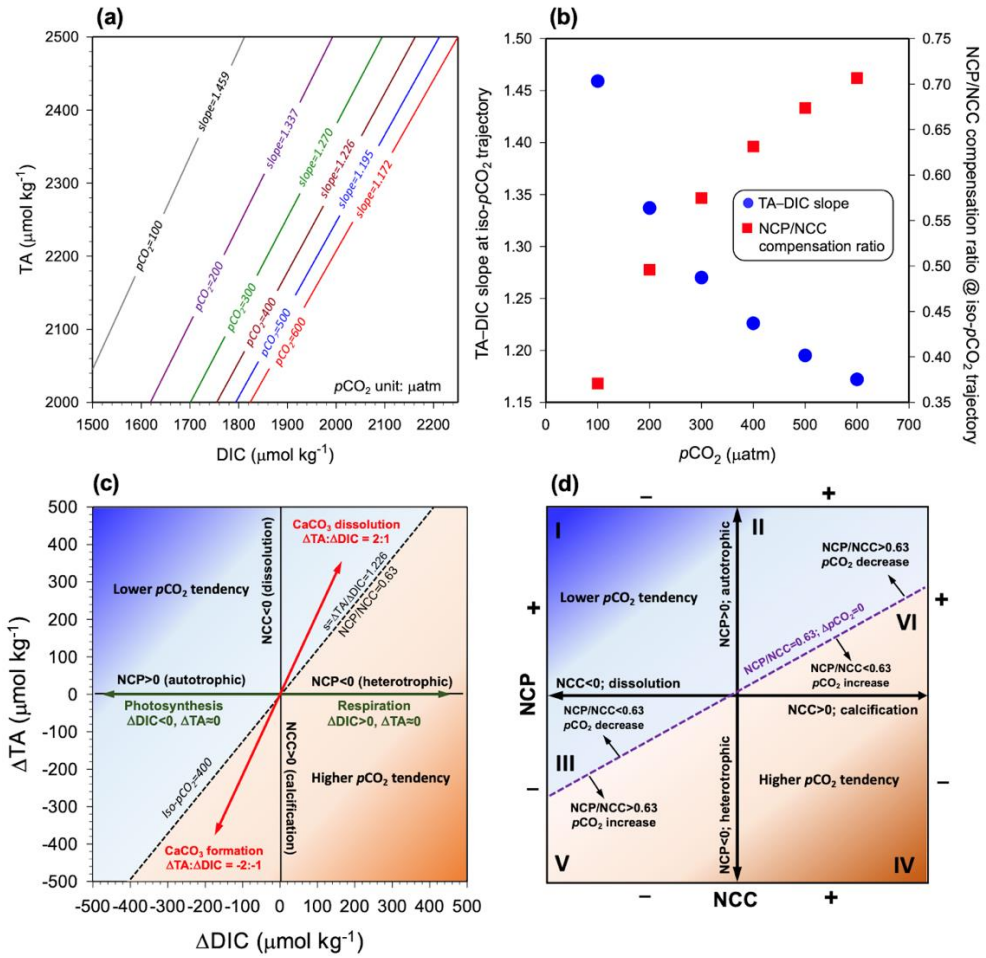
Sedimentary redox processes further regulate TA dynamics[48]. Sulfate reduction generates TA, but its net effect depends on sulfur cycling pathways. Sulfide may be buried as pyrite[20–23], preserving TA for export, or re-oxidized under oxygen supply (e.g., radial oxygen loss, bioturbation, tidal flushing)[49–51]. Re-oxidation generates protons that lower porewater TA and pH and can promote $CaCO_3$ dissolution[52]. While this dissolution enhances benthic TA fluxes[19], it may largely reflect internal carbonate recycling rather than a net TA source if the carbonates are modern biogenic.

Figure 2: blue/red colours are not very colour blind friendly, suggest to change the colourscheme.

Response:

Thank you for this helpful suggestion. We have revised the color scheme of Figure 2 to improve accessibility and ensure compatibility with color vision deficiencies. Specifically, we replaced the original red–blue contrasts with a blue–orange color-blind-friendly palette and adjusted symbol and line contrasts to maintain clear visual distinction across all panels.

Revised figure (Line 195):



I am sorry to be pedantic, but present day is closer to 425 than to 400 … Also, 25 degrees is more tropical than average temperate conditions.

Response:

Thank you for this helpful comment. We evaluated the sensitivity of the threshold to changes in pCO_2 at S = 35 and T = 25 °C (including 425 µatm), and found that it remains essentially unchanged (NCP/NCC ≈ 0.63; s ≈ 1.23). We therefore examined the effect of temperature at present-day pCO_2 (~400 µatm, representative of recent surface ocean pCO_2), where cooling to 20 °C (S = 35) increases the threshold to ≈ 0.66 (corresponding to s ≈ 1.21).

Box 1 (Line 149):

At present-day conditions (~400 µatm), this yields a threshold of NCP/NCC ≈ 0.63, indicating that organic production must exceed ~63% of calcification to maintain constant pCO_2. At present-day conditions (~400 µatm, representative of recent surface ocean pCO_2), this yields a threshold of NCP/NCC ≈ 0.63, indicating that organic production must exceed ~63% of calcification to maintain constant pCO_2. Cooling from 25 °C to 20 °C under temperate ocean conditions (S = 35; pCO_2 = 400 µatm) increases the threshold to ≈ 0.66, corresponding to s ≈ 1.21.

L219: they do exert an immediate effect, as the fact that they persist means there is net consumption of DIC (for OC and IC) and TA (For IC)?

Response:

Thank you for this important clarification. We agree that the persistence of unreacted organic matter and $CaCO_3$ implies a net removal of DIC (for both OC and IC) and TA (for IC) from the actively exchanging pool, and therefore represents a real biogeochemical effect.

Our original wording was intended to distinguish between immediate modifications to seawater carbonate chemistry and longer-term carbon storage. We have revised the text to clarify that, while these reservoirs reflect net consumption of DIC and TA at the system scale, they do not directly alter seawater carbonate chemistry on short timescales unless they are actively exchanged with the overlying water.

Revised text (Line 252 – 255):

These reservoirs represent a net removal of DIC (and TA for IC) from the actively exchanging pool, but do not directly modify seawater carbonate chemistry on short timescales unless coupled to exchange with the overlying water column.

L234: could make the same case for N2 production – which might be important in coastal sediments?

Response:

Thank you for this helpful comment. We agree that denitrification, through the loss of N_2, can also contribute to net alkalinity production. However, in many blue carbon ecosystems, denitrification is often limited by nitrogen availability, and sulfate reduction typically represents the dominant anaerobic pathway (Bianchi 2007; Reithmaier et al., 2021). We have added a brief clarification in the manuscript to acknowledge this process.

Reference:

Bianchi, T. S. (2007). Biogeochemistry of estuaries. New York, NY: Oxford University Press on Demand

Reithmaier, Gloria MS, et al. "Alkalinity production coupled to pyrite formation represents an unaccounted blue carbon sink." Global biogeochemical cycles 35.4 (2021): e2020GB006785.

L249: only with S burial …

Response:

Thank you for this comment. We agree that net alkalinity generation from sulfate reduction depends on the burial of reduced sulfur (e.g., as FeS or FeS_2). We have revised the text to clarify that without sulfur burial, the reoxidation of sulfide would offset the alkalinity gain.

Revised text (Line 284 – 286):

Sulfate reduction of external OC further enriches DIC and TA in a 1:1 ratio (reaction ⑤); but yields net TA only when reduced sulfur is buried; otherwise, re-oxidation consumes TA, limiting CO_2 uptake and potentially reinforcing a net CO_2-releasing regime.

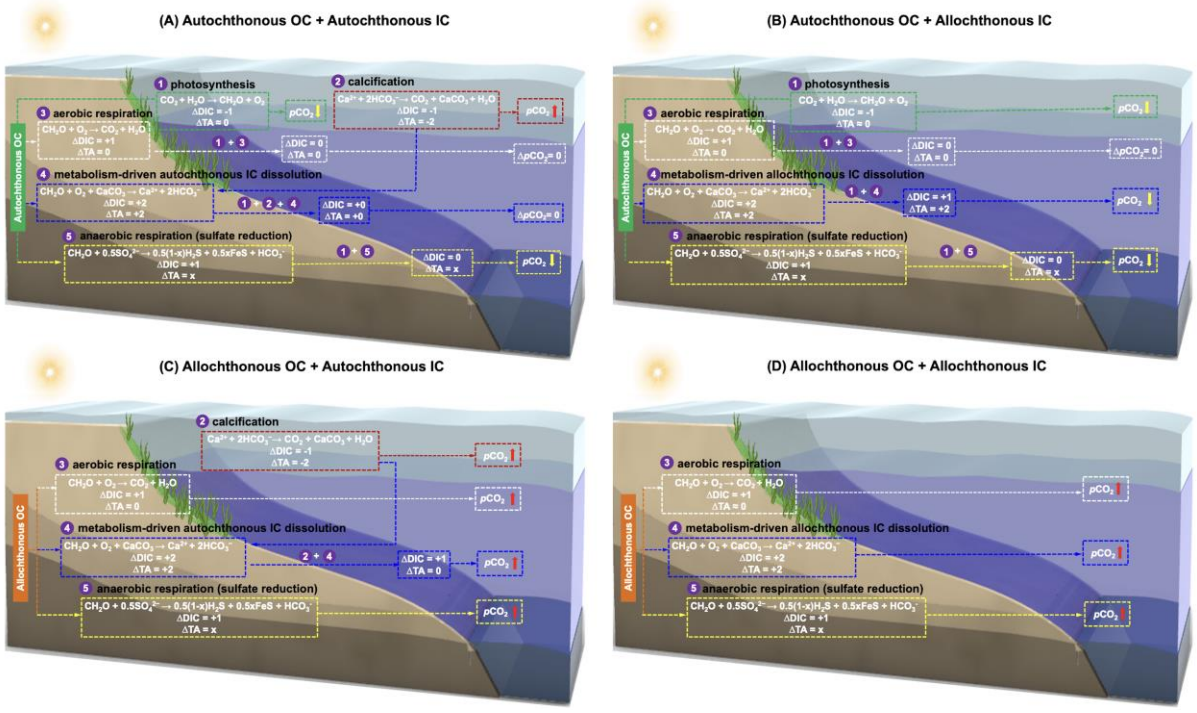
Figure 3: I don't like reaction 5 -> the deltaTA has to be dependent on the fraction of FeS precipitation, now it suggest it is independent and always +2.

Response:

We thank the reviewer for this important comment. We agree that the original formulation of Reaction ⑤ implicitly assumed a fixed alkalinity gain (ΔTA = +2), whereas in reality net alkalinity production depends on the fraction of reduced sulfur that is preserved via FeS (or pyrite) precipitation rather than re-oxidized.

We have therefore revised Reaction ⑤ to include a scaling term (x) representing the fraction of sulfide burial. Accordingly, both ΔDIC and ΔTA are now expressed as ΔDIC = +1 and ΔTA = x, ensuring that net alkalinity generation is contingent on reduced-sulfur preservation. This revision aligns the stoichiometry with established sedimentary redox frameworks and more accurately reflects the controls on alkalinity production.

The updated formulation is shown in Figure 3 (Line 312).



Reaction 4 mentions 'autochtonous IC dissolution', even when your situation describes allochtonous IC

Response:

Thank you for this helpful observation. We have revised Figure 3 accordingly (see Figure 3 above).

L293: why do you refer to figure 3 here?

Response: Thank you for pointing this out. It should be Figure 4.

L301: is that shown? You refer only to watercolumn measurements and benthic fluxes? It might as well be nitrogen cycling or anything else?

Response:

Thank you for this comment. We agree that our description here is conceptual and not directly resolved by the measurements. We have revised the text to clarify that these processes are inferred mechanisms consistent with the observed benthic fluxes. We also briefly acknowledge denitrification as an additional pathway, while maintaining that sulfate reduction coupled with sulfur burial likely represents the dominant control.

Revised text (Line 338 – 341):

Aerobic remineralization near the sediment surface generates CO_2 that promotes carbonate dissolution, while anaerobic pathways—primarily sulfate reduction, with contributions from denitrification and other redox processes—likely dominate at depth.

L313: re-uptaken is a funny way of phrasing

Response:

We thank the reviewer for pointing this out. We have revised the manuscript to replace the term "re-uptaken" with "taken up" at the indicated locations to improve clarity and readability while preserving the intended meaning of repeated biological uptake.

L321: That estimate seems low compared to TA fluxes measured globally 6. It might be worth checking this CaCO3 cycling review to get data-derived estimates of CaCO3 dissolution and TA production in coastal and shelf sediments: 10.22541/essoar.176131953.37180357/v1

Response:

Thank you for this helpful suggestion. We agree that the originally cited model estimate may underestimate the range of benthic alkalinity fluxes observed in coastal environments. Following your recommendation, we have incorporated data-derived constraints from recent syntheses of $CaCO_3$ dissolution in coastal and shelf sediments (Goossens et al., 2026; also see the suggested $CaCO_3$ cycling review). These studies indicate that alkalinity fluxes in seagrass-associated systems are substantially

higher than model-based estimates, with mean values of ~18.3 ± 9.1 mmol m^{-2} d^{-1} derived from global datasets.

We have revised the manuscript accordingly (Lines 367 – 370) to reflect this broader empirical range. Specifically, we now contrast model-based estimates (~5.5 ± 2.2 mmol m^{-2} d^{-1}) with data-derived global synthesis values (~18.3 ± 9.1 mmol m^{-2} d^{-1})[26], while maintaining that the TA fluxes observed at Dongsha IL (72.8 ± 64 mmol m^{-2} d^{-1}) remain substantially elevated.

Revised text (Line 359 – 361):

A recent model predicted that benthic TA fluxes in $CaCO_3$-rich seagrass sediments reach ~5.5 ± 2.2 mmol m^{-2} d^{-1} (ref 68), while global syntheses report ~18.3 ± 9.1 mmol m^{-2} d^{-1} (ref 26). Conversely, measured TA fluxes in the Dongsha IL were substantially higher (72.8 ± 64 mmol m^{-2} d^{-1}).

We have also revised the broader context (Lines 84–87) to reflect the role of carbonate dissolution in regulating ocean carbon uptake:

At broader scales, sedimentary $CaCO_3$ dissolution enhances TA and buffering capacity, thereby promoting CO_2 uptake and contributing non-negligibly to the ocean carbon sink, with coastal and shelf dissolution estimated at ~11 ± 9 Tmol yr^{-1} (ref 26).

In addition, we have incorporated recent evidence from van de Velde *et al.* (2026) in Lines 87-88:

This process can also be anthropogenically accelerated on continental shelves, operating as a fast climate feedback on annual to decadal timescales[27].

L332: it is hardly an unrecognized pathway given the papers you cite and the review mentioned earlier (see also, for example, 7). You do present it nicely in a conceptual framework which is the true novelty of this contribution – since most studies generally look at these pathways individually.

Response:

Thank you for this helpful comment. We agree that these pathways are not unrecognized and have been well documented in previous studies, including those cited and suggested. Our contribution lies in the development of a unified and quantitative conceptual framework that integrates organic metabolism, carbonate cycling, and sedimentary processes in regulating seawater CO_2 dynamics.

Specifically, this work introduces the NCP/NCC compensation ratio and a source-dependent framework, which together provide a mechanistic basis for understanding how these processes interact to determine whether systems function as net CO_2 sinks or sources.

References:

1. Hurd, C. L. et al. Forensic carbon accounting: Assessing the role of seaweeds for carbon sequestration. Journal of Phycology 58, 347–363 (2022).

2. Scholz, F., Börker, J., Vogt, C., Hartmann, J. & Wallmann, K. Natural ocean alkalinization through erosion of glacial till and weathering at the seafloor. Commun Earth Environ 6, 974 (2025).

3. Wallmann, K. et al. Erosion of carbonate-bearing sedimentary rocks may close the alkalinity budget of the Baltic Sea and support atmospheric CO2 uptake in coastal seas. Front. Mar. Sci. 9, 968069 (2022).

4. Frankignoulle, M. A complete set of buffer factors for acid/base CO2 system in seawater. Journal of Marine Systems 5, 111–118 (1994).

5. Frankignoulle, M., Canon, C. & Gattuso, J.-P. Marine calcification as a source of carbon dioxide: Positive feedback of increasing atmospheric CO2. Limnology and Oceanography 39, 458–462 (1994).

6. van de Velde, S. J., Hylén, A. & Meysman, F. J. R. Ocean alkalinity destruction by anthropogenic seafloor disturbances generates a hidden CO2 emission. Science Advances 11, eadp9112 (2025).

7. Reithmaier, G. M. S. et al. Alkalinity Production Coupled to Pyrite Formation Represents an Unaccounted Blue Carbon Sink. Global Biogeochemical Cycles 35, e2020GB006785 (2021).

Dear Dr. Mejjad and Dr. Basu,

We thank you and the reviewers for the careful evaluation of our manuscript and for your constructive comments and suggestions. We have revised the manuscript accordingly and believe that these changes have substantially improved its clarity and scientific presentation.
A detailed, point-by-point response to all comments is provided below. In addition, we have completed the revisions checklist and enclosed both the marked-up and clean versions of the revised manuscript.

****Guideline for Authors (Figures and Legends)****
Please improve the clarity and readability of all figures. Increase font sizes (especially in Figs. 3 and 4) to ensure legibility at publication scale. Revise figure legends to be fully self-contained by clearly defining all abbreviations (e.g., specify "total alkalinity (TA)") and ensure all acronyms used in figures (including Fig. 2) are explicitly explained.

Reply: We thank the Editors for this helpful suggestion. We have revised all figures to improve their clarity and readability. Font sizes have been increased across all figures, particularly in Figures 3 and 4, to ensure legibility at publication scale. Figure labels and annotations were also adjusted where necessary to enhance visual clarity.
In addition, all figure captions have been revised to be fully self-contained. Abbreviations and acronyms used throughout the figures—including total alkalinity (TA), dissolved inorganic carbon (DIC), net community production (NCP), net community calcification (NCC), and partial pressure of carbon dioxide (pCO_2)–are now explicitly defined in the corresponding figure legends. We also incorporated additional explanatory text into the Figure 2 caption to clarify the conceptual framework and the interpretation of the directional arrows.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

General overview

The revised version of "Revisiting Blue Carbon: How organic-inorganic coupling shapes CO2 fluxes in seagrass ecosystems" continues to display the strengths of the first submission: an integrated framework that considers how NCP and NCC counterbalance each other in the context of air-sea CO2 exchange in seagrass ecosystems. The revisions have been mostly sufficient in terms of addressing initial reviewer concerns and suggestions. Some of my own concerns have been alleviated by the authors' clarification of how the NCP/NCC compensation ratio ought to be interpreted and how cycling of organic carbon (OC) and inorganic carbon (IC) from allochthonous sources shifts this NCP/NCC ratio mechanistically. Table S1 is helpful, as well.

For these reasons, I still think that this review paper would be a valuable contribution to scientists, scholars and students studying/learning about carbon cycling/accounting in shallow, coastal ecosystems. However, I do have some lingering comments that will hopefully be addressed before publication.

Reply: We thank the reviewer for the thoughtful and constructive comments. We appreciate the positive assessment of the revised manuscript and are grateful for the opportunity to further clarify several aspects of the framework. We have carefully revised the manuscript to address all concerns raised.

My first comment is about timescale again and is related to section 1. The paper clarifies that long term sediment burial is not the focus – shorter-term air-sea CO2 exchange is (as stated in lines 91-100). But, I think that the introduction needs to acknowledge the different (shorter-than-OC-burial) timescales in which water column metabolism and sedimentary metabolism operate, and the extent to which these two sites of DIC and TA removal/production are coupled from each other in time. This temporal coupling or decoupling seems to matter a lot for the interpretations of scenarios in section 3, and is referred to frequently in Section 3. For example, lines 252-255 state that sedimentary processes "do not directly modify seawater carbonate chemistry on short timescales unless coupled to exchange with the overlying water column". In the Dongsha Island Inner Lagoon, it appears that this temporal coupling is indeed present and enables the ecosystem to be a CO2 sink (exhibiting consistently depressed pCO2 values). Something in the introduction – perhaps in the existing time scale paragraph over lines 91 to 100 – that establishes the importance/lack/possibility of temporal coupling between water column metabolism and sedimentary metabolism would better prepare

the readers for sections 3 and 4.

Reply: We agree that the temporal coupling (or decoupling) between sedimentary and water-column processes is critical for interpreting carbonate-system dynamics and the scenarios presented in Section 3. While the timescale distinctions among NCP, NCC, air–sea CO_2 exchange, and sedimentary carbon burial were clarified in the previous revision, we have now expanded this discussion to explicitly address the conditions under which sedimentary processes influence seawater carbonate chemistry. Specifically, we added text clarifying that the influence of sedimentary metabolism depends on its connectivity to overlying waters, and that sediment-derived DIC and TA affect surface-water pCO_2 only when sediment–water exchange and water-residence times are sufficient to transmit benthic signals, whereas rapid hydrodynamic flushing dilutes these benthic inputs before they can significantly alter surface-water chemistry. This addition explicitly introduces the concept of temporal coupling between sedimentary and water-column processes and provides the conceptual basis for interpreting the source–sink dynamics discussed in Sections 3 and 4, including the Dongsha Inner Lagoon example.

Changes in Manuscript (Lines 96–101):
"The influence of sedimentary metabolism on seawater carbonate chemistry also depends on its connectivity to overlying waters. Sediment-derived DIC and TA affect surface-water pCO_2 only when sediment–water exchange and water-residence times are sufficient to transmit benthic signals, whereas rapid hydrodynamic flushing dilutes these benthic inputs before they can significantly alter surface-water chemistry."

My second comment is related to section 3. I think it's important to compare and contrast the different mechanisms driving DIC and TA exchange through Scenarios A to D of Section 3; this is a major contribution of the paper. But, there seems to be a disconnect between the scenario A and B descriptions and their big picture implication about CO2 sources vs. sinks. At least for Scenarios A and B, it seems like the source vs. sink outcome depends on the quantitative balance (and temporal coupling) among all of the reactions listed for each scenario.

As a result, I do not see how the implied CO2 balance is always near neutral for scenario A, which is what the text and Table S1 imply. Lines 242-243 suggest that this balance should be set by the NCP/NCC compensation ratio, which fits the arguments in Section 2. In addition, Table S1 and lines 267-269 imply that a CO2 sink could be possible if enough of a TA surplus is generated by sulfate reduction in the sediments. If this

sediment-generated TA surplus was closely coupled to water column chemistry, would it not be possible for Scenario A to lead to a CO2 sink?

Likewise, lines 274-276 at the end of the Scenario B discussion seem to imply that whether Scenario B yields a CO2 source or sink is also context dependent (i.e., how much TA surplus is generated from allochthonous IC sources). This is at odds with the conclusion in Table S1 – which is that Scenario B always yields a strong CO2 sink.

Reply: We thank the reviewer for highlighting the need to further clarify that the source–sink outcomes of Scenarios A and B are not deterministic and depend on process magnitude and coupling.

To address this concern, we revised both the main text and Supplementary Table S1 to clarify that the source–sink outcomes of Scenarios A and B depend on process magnitude, alkalinity preservation, and sediment–water coupling.

For Scenario A, we now clarify that the apparent near-neutral balance applies only to the fully recycled fraction of internally produced OC and $CaCO_3$. We further state that a net alkalinity surplus may arise when reduced sulfur is preserved through burial and effectively coupled to the overlying water column, potentially shifting the system toward a weak CO_2 sink. Correspondingly, Table S1 has been revised to indicate a "Near-neutral to weak CO_2 sink" outcome and to explicitly note the role of preserved alkalinity generation through sulfur burial.

Changes in Manuscript (Lines 273 – 276)

"Consequently, Scenario A results in a nearly balanced state where $\Delta DIC \approx 0$ and $\Delta TA \approx 0$ for the fully recycled fraction, whereas a net TA surplus arises only when reduced-sulfur burial is sustained and effectively coupled to the overlying water column, potentially leading to a weak CO_2 sink."

For Scenario B, we agree that the magnitude of CO_2 uptake depends on the extent of alkalinity generation and the nature of carbonate dissolution. We therefore revised Table S1 from "Strong CO_2 sink" to "Potentially strong CO_2 sink." We also revised the text to emphasize that Scenario B represents the configuration with the greatest potential for pCO_2 reduction, while noting that its net climate benefit depends on whether dissolution reflects internal carbonate recycling or net alkalinity generation from geogenic carbonate sources.

Changes in Manuscript is in Box 2 (Line 452).

"Scenario B (autochthonous OC coupled with allochthonous geogenic $CaCO_3$) represents the optimal configuration with the greatest potential for pCO_2 reduction, as it enhances alkalinity generation and thereby promotes sustained CO_2 uptake."

More broadly, we now explicitly state that the four scenarios are idealized end-members designed to isolate dominant pathways rather than deterministic predictors of CO_2 source–sink status, and that natural systems likely occupy a continuum where multiple processes co-occur.

Changes in Manuscript (Lines 236 – 238)

"These scenarios represent idealized end-members designed to isolate dominant pathways rather than deterministic predictors of CO_2 source–sink status, although natural systems more likely span a continuum in which multiple processes co-occur."

Smaller comments:

Line 211: What does "their" refer to in the phrase "their coupling"? Does it refer to the sedimentary carbon pools?

Reply: We revised the sentence to clarify that "their" refers to sedimentary carbon pools. The revised text now states that sedimentary carbon pools determine their coupling to contemporary carbon cycling and their influence on DIC and TA.

Changes in Manuscript (Lines 216 – 219)

"While the NCP–NCC balance governs CO_2 exchange in the seawater column, carbonate chemistry is also strongly shaped by the origin (autochthonous versus allochthonous) and temporal nature (modern biogenic versus geogenic) of sedimentary carbon pools, which determine their coupling to contemporary carbon cycling and their influence on DIC and TA."

Line 212: What is the meaning of "active" in this sentence – the water column processes?
Reply: We have replaced this phrase with "contemporary carbon cycling" to improve clarity (**Line 219**).

Line 303: What does "baseline conditions" mean in this context?
Reply: To clarify this terminology, we now define baseline conditions at first use as

"the corresponding unvegetated or pre-restoration state." Subsequent uses of the term retain this defined meaning throughout the manuscript.

Changes in Manuscript (Lines 242 – 244)

"Sustained ocean–atmosphere CO_2 removal arises only from processes that generate net TA and/or enhance long-term OC storage beyond baseline conditions (i.e., the corresponding unvegetated or pre-restoration state)."

Figure 2: In the caption, it is worth defining what the solid arrows extending from bottom left to top right mean in panel c.

Response: We agree with the suggestion to make the definition more precise. We have revised the Figure 2 caption to explicitly define the trajectories and driving processes represented by the solid arrows in panel (c).

Changes in Manuscripts (Lines 206-208)

"The green and red solid arrows in (c) represent net shifts in the carbonate-system state (TA and DIC) driven by organic carbon metabolism (i.e., photosynthesis and respiration) and inorganic carbonate cycling (i.e., $CaCO_3$ formation and dissolution), respectively."

Reviewer #2 (Remarks to the Author):

Thank you for revising the manuscript so thoroughly. The new version is a lot clearer in terms of the different IC and OC sources and cycling pathways. The changes have largely addressed my previous comment about the relevance of allochthonous vs autochthonous sources and the importance of the overall ocean carbon budget. I also like the revised colour scheme of the figures. I think that the paper makes an important argument that really needs to be heard more loudly, so I fully support the publication.
Reply: We thank the reviewer for the positive evaluation, encouraging comments, and strong support for publication.

I do have a minor comment about the paragraph starting in Line 249, which I find a little bit confusing. The overall argument is that recycling of purely autochthonous OC + IC generates no net change in pCO2, but that often only part of the autochthonous OC + IC are actually fully recycled. This means that POC and PIC (or their remineralization products, i.e. DIC + TA) can accumulate in sediments without being recycled back to the water column. My confusion is that in Line 253, the authors then write that such accumulation does not modify seawater carbonate chemistry on short timescales, unless the remineralization products are returned to the overlying seawater. But if a system is continually accumulating sedimentary pools of OC and IC that don't get remineralized and returned to the water column, then that does of course change seawater pCO2. The seawater pCO2 will only not show net changes if the autochthonous OC and IC are fully recycled and returned to the water column, as is stated in the following sentence (Line 255). I think this paragraph can probably be made much simpler by just clearly explaining the fact that in Scenario A there is only no net change in seawater carbonate chemistry if all OC + IC are indeed fully recycled and returned to the water column, but that in reality some fraction of this production might accumulate in the sediment, and depending on how large this fraction is, there would then be a greater or lesser net change in seawater pCO2.

We thank the reviewer for this helpful clarification. We agree that the previous wording could be interpreted as implying that sediment accumulation has no effect on seawater carbonate chemistry. Our intention was to describe the specific case in which autochthonous OC and PIC are fully recycled and their remineralization products are returned to the overlying water column, resulting in no net change in seawater pCO_2.
In accordance with the reviewer's suggestion, we have simplified this section. The revised text clarifies that under Scenario A, a net-zero effect on seawater carbonate chemistry occurs only when all autochthonous OC and IC are completely recycled and

returned to the water column. We further explain that when a fraction of this production accumulates in the sediment and escapes recycling, it results in a net change in seawater pCO_2, the magnitude of which depends on the proportion of OC and IC retained in the sediment.

Changes in Manuscript (Lines 257–264):

"This apparent balance ($\Delta DIC \approx 0$ and $\Delta TA \approx 0$) in Scenario A applies only to the fraction of internally produced CH_2O and $CaCO_3$ that is fully recycled (reaction ④). In practice, remineralization and dissolution are often incomplete and condition-dependent, governed by porewater saturation state and redox conditions, allowing unreacted OC and $CaCO_3$ to persist as sedimentary reservoirs rather than being returned to the water column[56,57]. The removal of this buried fraction from active cycling exerts a corresponding net influence on seawater pCO_2, with the magnitude of the effect depending on the proportion retained in the sediment. Thus, canonical stoichiometries represent theoretical endmembers, whereas in situ carbonate-system responses depend on the balance between recycling and burial."

Reviewer #3 (Remarks to the Author):

I am happy with the changes made by the authors, I have just two comments I would hope they could address before acceptance:
Their framework implicitly assumes that any organic carbon that is transported outside of the meadow (lateral export) contributes to sequestration. This is not always the case, and much of that would be remineralised elsewhere. I hope that they could acknowledge this upfront and state that this is an assumption and their assessment almost always will overestimate the CO2 sequestration potential of seagrass meadows.
Some small comments:
L34: rephrase to 'particulary rich in carbonates'
L62: 'in the ground' -> 'in the seafloor'
L91: 'strongly' -> 'much'

Reply: We sincerely thank the reviewer for the positive feedback and for supporting the publication of our manuscript. We appreciate the constructive comments, which have helped us further refine the nuance and accuracy of our proposed framework.
(1) We agree that the ultimate fate of organic carbon (OC) exported laterally from seagrass meadows–whether it is buried in deep-sea sediments or remineralized in the water column–is a critical factor in determining the true net climate mitigation potential of these ecosystems. In the revised manuscript, we have added a clarifying statement in the Introduction to acknowledge this assumption and its associated limitation.

Changes in Manuscript (Lines 56–59):
"*However, this interpretation assumes a local meadow–sediment system boundary and does not explicitly account for lateral OC transport. Because a fraction of exported OC may be remineralized elsewhere in the ocean rather than being permanently stored, the framework should be regarded as an upper-bound estimate of the net CO_2 sequestration potential of seagrass ecosystems.*"

(2) The manuscript has been revised accordingly. Specifically, the wording in Line 34 was revised during the restructuring of the Abstract in accordance with the Editor's recommendations. In addition, “in the ground” was revised to “in the seafloor” (**Line 53**) and “strongly” was revised to “much” (**Line 71**) to improve clarity and readability.