Response to RC2

Reviewer comments are provided in black, with responses highlighted in blue for clarity.

Summary:

This paper provides a thorough and innovative investigation into the role of variable organic matter (OM) stoichiometry in marine carbon cycling, with a focus on the Northwest European shelf seas (NWES). Variations in the elemental ratios of carbon, nitrogen, and phosphorus are integrated into a high-resolution coupled 3D physical-biogeochemical model to explore their impact on the efficiency of carbon sequestration. The authors demonstrate that variable stoichiometry, implemented through carbon-enriched dissolved OM under nutrient limitation and preferential remineralization of organic nitrogen and phosphorus, enhances net CO_2 uptake by 10–33% in the NWES compared to fixed Redfield stoichiometry. This paper is very well-written, with clear organization and a logical flow that makes its complex subject matter accessible. The figures are well-crafted and effectively support the key findings, reflecting the substantial effort and attention to detail invested in this work. The study represents a significant contribution to marine biogeochemistry by addressing a poorly understood yet critical aspect of carbon cycling. The results are highly relevant to advancing global modeling efforts and improving our understanding of the coastal carbon cycle. I recommend this paper with **minor revisions** to clarify a few methodological and interpretive details. Below are my specific comments.

We thank the reviewer for their positive evaluation of our work and their insightful comments. We appreciate the constructive feedback and suggestions for improving clarity. We will incorporate the reviewer's suggestions into the final version of the manuscript to further clarify the methodology and strengthen our analysis. Below, we address each specific comment in detail.

A general comment on the terminology used for changes in air-sea CO_2 flux between two model configurations:

In this study, it appears that positive values of air-sea CO_2 flux represent CO_2 uptake by the ocean, while negative values indicate CO_2 outgassing to the atmosphere. The manuscript describes negative changes in air-sea CO_2 flux between two model experiments as "decreased air-sea CO_2 exchange." However, this phrasing can be ambiguous because air-sea CO_2 exchange can have positive (uptake) or negative (outgassing) values. For example, in cases where CO_2 uptake occurs in the RS experiment (positive flux), a "reduction in air-sea CO_2 exchange" in the ER experiment means less CO_2 uptake, which aligns with the terminology. However, if CO_2 outgassing occurs in the RS experiment (negative flux), a "reduction in air-sea CO_2 exchange" in the ER experiment corresponds to more CO_2 outgassing, which actually implies an increase in exchange.

To avoid confusion, I suggest using more explicit terms, such as:

- "CO_2 uptake is stronger/weaker"
- "CO_2 outgassing is stronger/weaker."

For example, in Line 718, instead of "air-sea CO_2 exchange is decreased by xx%," you could write, "CO_2 uptake by the ocean is reduced by xx%" or "CO_2 outgassing to the atmosphere is enhanced by xx%," depending on the context. Please consider checking this throughout the manuscript. This more straightforward phrasing would improve clarity and ensure that readers can unambiguously interpret the results.

We thank the reviewer for highlighting the potential ambiguity in our terminology regarding the air-sea CO_2 flux. We agree that the phrase "reduction in air-sea CO_2 exchange" could be misinterpreted as a reduction in the amplitude rather than a reduction of the downward flux. Our initial intent was to describe both a decrease in CO_2 uptake and an increase in CO_2 outgassing with a single term. However, we recognize that this phrasing does not clearly communicate the distinction between the two processes. We will revise the manuscript to use more explicit terms, as suggested, such as "CO_2 uptake is reduced" or "CO_2 outgassing is enhanced," depending on the context. This will ensure clarity and prevent potential confusion for readers.

Detailed comments:

1. **Line 176**: Does this imply that the growth of the other two phytoplankton groups is not temperature-dependent? What might be the downsides of making such assumptions in the model?

Yes, only cyanobacteria growth is temperature-dependent in the model, while diatoms and flagellates are indeed only limited by nutrients and light. Since in the model cyanobacteria growth is restricted to low-salinity conditions in the Baltic Sea, their competition with diatoms and flagellates is not a significant factor for our assessment of the Northwest European shelf seas. We acknowledge that there is likely some temperature dependence in the growth of diatoms and flagellates. However, we assume that within the typical seasonal temperature variability in this temperate shelf sea region, these groups are primarily limited by nutrient and light availability (e.g., Fernández-González et al., 2022). Therefore, we consider this simplification reasonable for our model's focus on the carbon cycle in the Northwest European shelf seas. For global studies and future climate scenarios, this may be of greater importance.

2. **Line 181**: Why are POM and DOM productions partitioned into 60% and 40%? What is the reference for this assumption?

We chose the 60% POM and 40% DOM partitioning based on extensive model validation, particularly against observed summer DOC and POC concentrations (Figures 15 and 16). This partitioning ratio was evaluated and found to produce the best overall agreement with these observations. Alternative ratios, such as 80% POM and 20% DOM, were considered but resulted in poorer model performance. For example, higher POM fractions, combined with POM dissolution to DOM, led to an unrealistic distribution of DOM throughout the water column and further decreased the already underestimated surface DIC uptake in the reference configuration.

While the partitioning ratio is indeed a simplification, we believe that the agreement between observed and simulated DIC uptake depth ranges (Figure 14) supports the accuracy of the chosen ratio for representing OM production in the surface layer. The partitioning has implications for vertical export fluxes, as POM has a constant sinking rate, whereas DOM does not. However, since the concentrations of organic matter are well-represented in the model, this suggests that the chosen partitioning is appropriate and robust in this context.

It is also important to note that other factors, such as remineralization rates and grazing, play significant roles in controlling OM concentrations in the model. These parameters contribute to a complex, interconnected system, making it a challenging inverse problem to arrive at the right balance. The chosen partitioning is part of this broader parameterization, which we believe on average represents the organic matter dynamics well. Since RC1 included a similar question, we will provide additional clarification in the Methods section to further explain the rationale behind this partitioning choice and its relationship to other key model parameters, such as remineralization and grazing rates.

3. **Line 225**: If I understand correctly, the extracellular release of DOM should balance with the consumption of nutrients and DIC from a mass balance perspective (without altering phytoplankton biomass). Could the authors clarify this in the Methods section? I noticed a description of DIC consumption in line 279 but found no description to nutrient (N and P) consumption.

That is correct. The extracellular release of DOM, as adapted from Neumann et al. (2022), directly incorporates inorganic carbon and nutrients into DOM without affecting phytoplankton biomass. Since this manuscript focuses specifically on organic and inorganic carbon dynamics, we limited the equations in the main text to those related to carbon. However, in Section 2 of the supplementary material, we provide a full updated model description, including the equations for nutrients, where the corresponding terms for DOM production appear as negative terms, representing the uptake of nutrients.

We recognize that this detail is missing from the main text and may cause confusion regarding the mass balance. To address this, we will add a description of the nutrient consumption related to DOM production in the Methods section for clarity and point out that the ecosystem model equations are described in the supplementary material.

4. **Line 225**: Is the scaling factor B_{ER} used as a knob to tune the model? Is this an arbitrary number? Please clarify.

Yes, the scaling factor B_{ER} is indeed used as a tuning parameter to adjust the representation of organic matter concentrations and stoichiometry in the model. This is already described in the Methods section (page 9, lines 209-212). However, we recognize that this explanation may not have been clear enough. To avoid any confusion, we will add further clarification that the tuning parameter B_{ER} is used to control both the concentrations and stoichiometry of organic matter in the model.

5. **Line 242**: If this was the case, I would expect E_{DON} to be set to zero when $\beta_N \leq 0.1$ or $\beta_P \geq 1$ in Equation (2). However, maybe it doesn't matter to include $\beta_P \geq 1$ in Equation (2) because this might not occur in practice.

We thank the reviewer for suggesting clarification regarding the ER implementation. For completeness, it would indeed be correct to include the limit of $\beta_P \geq 1$ in Equation (2). However, as already noted in the comment, the phosphorus limitation, which depends solely on phosphate, is strictly mathematically defined between zero and one. Therefore, the suggested limit is implicitly satisfied within the current formulation. Nonetheless, for future adaptations, we agree that it may be useful to explicitly add this limit to the equation.

The reason we only mention the $\beta_N \geq 1.1$ cases is due to the composite nature of the nitrogen limitation term, which is a function of both nitrate and ammonium limitations, rather than being strictly limited to 1.1. We computed β_N across a wide range of nitrate and ammonium concentrations, and while it can exceed one, it converges around 1.075. As a result, we chose an approximate limit of 1.1 for the nitrogen limitation to constructively use the ER formulation.

Although both the nitrate and ammonium terms are theoretically constrained between zero and one, which would result in a nitrogen limitation limit of two, this value is never even closely reached in practice. It rather stays below 1.1 as mentioned above. Using a limit of two would overly reduce the ER of DON, so we opted to use a practical upper limit of 1.1 for nitrogen limitation. To ensure the formulation remains feasible and does not fall below zero, we introduced additional restrictions as part of the parametrization.

6. **Line 265**: What do the "+60%" and "+100%" in Figure 3 represent?

We thank the reviewer for the question and the opportunity to clarify this point. The "+60%" and "+100%" in Figure 3 refer to the increased base remineralization rates for DON, DOP, PON, and POP in the PR configuration.

As described in Section 2.1.5 of the methods, the remineralization rates are adjusted such that the base rate is increased by 60% for nitrogen and 100% for phosphorus in the PR configuration. In the ER&PR configuration, these rates are reduced to +30% and +50% for nitrogen and phosphorus, respectively.

While the corresponding parameters are provided in the supplementary material, the choice to present these changes as increases in remineralization rates in the figure is intended for clarity. This is further detailed in the manuscript, which states: "The third configuration (PR) includes the preferential remineralization of nitrogen and phosphorus with +60% and +100% higher bioavailability respectively for both POM and DOM. Lastly, the fourth configuration (ER&PR) combines the ER and PR, both with a reduced contribution. This is expressed by a lower ER scaling factor B_{ER}=0.2 and reduced increase in bioavailability of +30% and +50% for nitrogen and phosphorus respectively" (page 12, lines 294-298). We believe this explanation is clearly stated in the manuscript, and we hope this response helps to clarify the matter.

7. **Line 294**: In the PR experiment, was ER turned off?

Yes, in the PR experiment, ER was indeed turned off. We assess the PR and ER separately in the respective configurations, and only in the ER&PR configuration are the two processes combined. We believe this distinction is clearly stated in the manuscript with the goal of assessing the individual and combined impacts: "To assess the individual and combined effects of the release of carbon-enriched DOM and the preferential remineralization of organic nitrogen and phosphorus, we compare four model configurations with identical initial conditions and forcing." (page 12, lines 290-291).

8. **Line 297**: Is there a reason the authors chose a lower ER scaling factor and reduced increase in bioavailability in the ER&PR configuration? As a reader, I'm curious whether the impacts of ER and PR are linearly additive (i.e., using the same settings as in the individual ER and PR configurations). To clarify, I am not requesting the authors to conduct additional model experiments.

We thank the reviewer for raising this question regarding the reduced contributions in the fourth, combined configuration (ER&PR). The reason for this reduction stems from the focus of this study, which prioritizes accurately representing organic carbon concentrations and organic matter stoichiometry, rather than solely reproducing the more constrained DIC concentrations, as in some previous studies.

To ensure that the three configurations—individual and combined effects of the ER and PR mechanisms—accurately represented the observed POM and labile DOM stoichiometry (Figures 4 and 5), we conducted sensitivity tests to estimate the appropriate remineralization ratios and the scaling factor for the ER parametrization. We assume that both preferential remineralization and the release of carbon-enriched DOM contribute to the overall cycling of organic matter, and therefore, we wanted to include a combined configuration to explore potential interactions between these two processes. However, we found that if the same parameters for ER and PR were used in the combined configuration, the resulting C:N and C:P ratios would approximately double and hence be much higher than those observed for labile DOM, making the comparison across configurations less meaningful.

We also appreciate the reviewer's suggestion to assess whether the effects of ER and PR are linearly additive. In fact, the ER&PR configuration, where both processes were combined with 50% of the individual contributions, gives some insight into the interaction between the two processes and provides a comparison to the full individual contributions. This approach is described in section 2.2.2 of the manuscript, where we state: "To ascertain a realistic magnitude of the preferential remineralization and the release of carbon-enriched DOM, we identified parameter settings for which the simulated stoichiometry adheres to observational bounds for LDOM and POM" (page 14, lines 373-375). We hope this satisfies the concern, and we will provide further clarification on this point if needed.

9. **Line 321**: Did the authors use TA at the zero-salinity endmember from TA vs. salinity relationships for those rivers without data?

We thank the reviewer for this question. As described in the methods section, we used TA data directly where available. For rivers without direct data, missing TA values were derived from a salinity-based relationship where applicable. For the remaining rivers, we estimated the TA loads using a regional average. This is summarized in Section 2.1.5 of the methods: "Where possible, missing TA loads are derived from the salinity relation in Hjalmarsson et al. (2008) and Artioli et al. (2012). For the remaining rivers, we use average DIC loads of 2700 µM from Pätsch and Lenhart (2004) and an average TA in the NWES of 2050 µM." (page 13, lines 321-323). This approximation may limit our representation of TA especially in regions of freshwater influence, but we assume that this has no strong impact on the results and conclusions of this study. We hope this clarifies our approach to the model configuration.

10. **Lines 579-580**: These paired numbers refer to rates in the North Sea and the entire NWES. However, on first reading, it seems as if 4.5 and 9.6 refer to pelagic remineralization and carbon fixation, respectively. Could the authors rephrase to avoid this confusion?

We thank the reviewer for pointing out this unclear phrasing. We agree that the original wording could lead to confusion and will rephrase the sentence to better clarify the distinction between the North Sea and the entire NWES. We will ensure that it is clear that the numbers refer to rates in different regions, not specific processes like pelagic remineralization and carbon fixation.

11. **Line 690**: I don't necessarily agree with the statement of "decrease in the air-sea CO2-exchange" here. The blue coloration along the Norwegian coast and other regions seems to indicate that outgassing is stronger due to ER, rather than a decrease in air-sea CO_2 exchange. It appears that the effect of ER is to enhance CO_2 uptake in previously uptake-dominant regions and increase CO_2 outgassing in outgassing-dominant regions. While this may not hold everywhere, it seems to be the general pattern in Figure 11. Please correct me if I have misunderstood.

We thank the reviewer for pointing out this specific case in which the phrase "decrease in the air-sea CO_2 exchange" may be misleading. As correctly noted by the reviewer, the effect of ER in this context is to enhance pre-existing gradients, leading to stronger CO_2 uptake in previously uptake-dominant regions and increased CO_2 outgassing in outgassing-dominant regions, as observed in Figure 11. This pattern reflects the biological effects on pCO_2, where ER can amplify the uptake through net carbon fixation or increase outgassing through enhanced net respiration. We agree that the phrasing should be revised to better reflect this, and we will adapt it accordingly in the manuscript.

12. **Lines 718, 720**: Please see my general comment #1. Additionally, the authors may want to check for similar phrasing elsewhere in the manuscript.

We thank the reviewer for pointing this out and for their general comment regarding the phrasing of air-sea CO_2 flux changes. We agree that similar ambiguous phrasing may be present elsewhere in the manuscript, and we will carefully review the text to ensure that all references to changes in air-sea CO_2 exchange are clear and unambiguous. As noted in our previous responses, we will adopt more explicit terminology, such as "CO_2 uptake is stronger/weaker" or "CO_2 outgassing is stronger/weaker," to better reflect the underlying processes and avoid confusion. We will ensure these changes are made consistently throughout the manuscript.

13. **Line 735**: I believe Figure S18 shows the spatial distribution of air-sea CO_2 exchange for four experiments, rather than the differences between two experiments, as suggested by the titles of the last three columns.

We thank the reviewer for pointing out this typo in the individual column titles. As correctly noted, the titles should reflect the names of the configurations (RS, ER, PR, ER&PR) rather than the differences between experiments. We will make this correction and update the figure, as shown in Supplementary Figure S18. The displayed variables are already shown currently in the colormaps and are showing the spatial distribution of air-sea CO_2 exchange for four experiments, rather than the differences.

14. **Line 789**: Why did the authors choose vertically integrated DIC to describe differences between model experiments? I appreciate this approach, but is it because changes in concentrations are small in Figure 14? It's somewhat hard to discern differences in the vertical profiles. Perhaps there are better ways to present the changes in the DIC profile? Just a thought to consider.

We thank the reviewer for the thoughtful suggestion. There are several considerations that influenced our decision to present vertically integrated DIC to describe differences between model experiments, as shown in Figure 14. The use of vertically integrated DIC was a compromise to better highlight the differences between

model experiments, particularly in relation to the seasonal reduction in DIC and its vertical distribution. We wanted to emphasize the overall changes in DIC and compare them with observational profiles, which also consider depth distribution. While we acknowledge that changes in concentration are small at deeper depths (which may make differences harder to discern), the integrated DIC values provide a clearer view of the larger-scale trends across the water column.

In relation to annual carbon fluxes, the vertically integrated values are more informative, as concentrations at larger depths do not vary as significantly. Integrated over the entire water column, however, the additional DIC drawdown is notably larger than the uptake in surface waters. This also suggests that additional organic carbon may be imported from the Baltic Sea and respired in the Norwegian Trench, contributing more DIC than what is observed in the surface waters of the northern North Sea and the Norwegian Trench. This finding aligns with the increased net heterotrophy our model experiments show in these regions.

Alternatively, we also considered showing the differences in concentrations. While this would provide a more direct view of the impact, it would not allow for a meaningful comparison with observational profiles, weakening the argument that we more accurately represent the maximum DIC depletion in summer and match the vertical profiles in terms of the vertical extent, as shown in Figure 14. We hope this clarifies our reasoning for using vertically integrated DIC in this context.

References:

Fernández-González, C., Tarran, G.A., Schuback, N. *et al.* Phytoplankton responses to changing temperature and nutrient availability are consistent across the tropical and subtropical Atlantic. *Commun Biol* **5**, 1035 (2022). https://doi.org/10.1038/s42003-022-03971-z

Neumann, T., Radtke, H., Cahill, B., Schmidt, M., and Rehder, G.: Non-Redfieldian carbon model for the Baltic Sea (ERGOM version 1.2) – implementation and budget estimates, Geosci. Model Dev., 15, 8473–8540, https://doi.org/10.5194/gmd-15-8473-2022, 2022