General response

We appreciate the constructive and thoughtful feedback provided by the reviewers. Their suggestions have improved the clarity of the manuscript. Below, you will find our point-by-point responses to each comment (reviewer comments are presented in **black**, and our responses in **blue**). In revising the manuscript, we have carefully addressed all individual reviewer comments to ensure that their concerns and suggestions have been incorporated.

Before addressing the specific reviewer comments, we made a few minor corrections throughout the manuscript. A detailed list of these general corrections is provided below.

- Correction of the following affiliation:
  - "*Institute for Chemistry and Biology of the **Marine Environment**, Carl von Ossietzky University Oldenburg, Oldenburg, Germany*" (page 1, lines 9–10)
- The percentage increase of 10–33% corresponds to the North Sea and not the entire Northwest European shelf seas (NWES). Thus, we corrected this both in the abstract and the discussion section. This needs to be corrected for the abstract and short summary on the website as well:
  - "*Compared to the reference simulation with fixed Redfield stoichiometry, the variable stoichiometry configurations show an increase in the annual net CO_2 uptake of **10–33% in the North Sea and 9–31% in the entire NWES**, depending on the relative contribution of the two new implementations.*" (page 1, lines 26–29)
  - "*By implementing two pathways for variable C:N:P stoichiometry in the production and respiration of OM (Fig. 3), we demonstrate that variable stoichiometry consistently results in an increased oceanic CO_2 uptake with a range of **10–33% in the North Sea and 9–31% in the entire NWES**.*" (page 34, lines 862–864)
- A hyphen correction has been applied across the whole manuscript: the dash in "*CO_2-uptake*" has been removed to make it "*CO_2 uptake*," and in "*air-sea CO_2-exchange*," only the second dash has been removed, resulting in "*air-sea CO_2 exchange*".
- For the numbering of equations such as "Eq. (1)", the parentheses have been added.
- Number ranges now consistently use an en dash (–).
- The data availability statement has been updated to reference both the datasets used in this study and the code for the model simulations and analysis.
  - "All datasets used for generating the figures and results in this study are openly accessible via Zenodo at https://doi.org/10.5281/zenodo.14916290. The code used for the model simulations and data analysis is available from the corresponding author upon reasonable request."(page 38, lines 960–962)

Response to RC1

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

The authors developed a regional model that considers the variable DOM and POM stoichiometry by incorporating preferential remineralization of organic matter and DOM release to study the impact of these processes on the carbon cycle in the Northwest European shelf seas. The manuscript is well-written, the model results align well with observations, and the discussion of the carbon cycle and the influence of variable organic matter stoichiometry is comprehensive. This study is a good example to show the importance of including variable organic matter stoichiometry in the modeling work. After some minor revisions, the manuscript can be considered for publication.

We thank the reviewer for their appreciation of our work and for recognizing the importance of considering variable organic matter stoichiometry in regional carbon cycle modeling. We value the reviewer's thoughtful questions and constructive suggestions. We have carefully incorporated the reviewer's suggestions into the revised manuscript to strengthen our analysis and resolve any outstanding questions. In the following, we respond to each specific comment in detail.

Specific Comments:

Line 125-130: did the authors consider the river input of DOC into the North Sea in the model?

As the question suggests, the stoichiometry of imported OM and specifically DOM from land plays a relevant role in carbon budgets of shelf seas with respect to the differences in composition of imported and exported DOM. Here, we did not directly consider river inputs of bulk DOC in our model configurations for the following two reasons:

1. ECOSMO II only accounts for the bioavailable or labile fraction of DOC, as all simulated DOM is available for respiration. As such, we would need to derive the respective fraction, which is challenging, as the balance between refractory and labile DOC in river inputs, along with their respective lifetimes, is highly uncertain.
2. Further, including terrestrial DOC with C:N:P ratios that show strong deviations from Redfield ratios would compromise the comparison with the Redfield stoichiometry configuration, as it is important for boundary conditions and forcing to remain consistent.

Instead of prescribing riverine DOC loads, we assume that 10% of the estimated DOC loads are bioavailable, rapidly remineralized and therefore add them directly to DIC river loads. This approximation neglects potential variations in the C:N:P ratios of terrestrial DOM, allowing us to focus on variations in the production and degradation of labile DOM within the marine environment. This is a useful simplification for this experiment, maintaining consistency between the configurations.

As already mentioned in the original manuscript, the methods section states:

"*Riverine DOC is assumed to be only 10% bioavailable as suggested by Kühn et al. (2010). This fraction is considered to be remineralized and hence is directly added to DIC.*" (page 13, lines 324–325).

Additionally, we already clarified in the discussion of the original manuscript that the composition of imported organic matter from land is not considered but presents potential for further investigation:

"*Other effects on OM composition that are beyond the scope of this study include the quality of imported OM from land and the effect of variable stoichiometry on higher trophic levels.*" (page 36, lines 911–912).

Line 181: the authors assumed a 60% POM and 40% DOM separation for the fate of new detritus (this percentage can vary a lot in the ocean). Have other percentages been tested? It would be helpful to discuss whether varying this ratio might influence the conclusions of the study.

As the reviewer correctly pointed out, the partitioning between new POM and DOM can vary regionally and temporally in the global ocean. This partitioning is a strong simplification in our model, where we chose a 60% POM and 40% DOM separation. This ratio was tested during the validation of summer DOC and POC concentrations (Fig. 15 and 16), along with other key ecosystem parameters such as remineralization rates. Alternative ratios were considered, but they worsened the model's performance. For example, the 80% POM and 20% DOM partitioning, combined with POM dissolution to DOM, led to DOM being distributed throughout the water column and further decreased the already underestimated surface DIC uptake in the reference configuration. In our view, the agreement between the depth ranges of observed and simulated DIC uptake (Fig. 14) supports the accuracy of the model's representation of OM production in the surface layer.

Regarding the influence of this ratio on the study's conclusions, we acknowledge that variations in partitioning could affect vertical export fluxes due to differences in sinking rates (POM has a constant sinking rate, while DOM does not sink). However, since the concentrations of organic matter are already well-represented in the model, they implicitly validate the chosen parameter set to a certain extent. While different parameter combinations could reproduce similar concentrations, given similar concentrations, the impact on the carbon cycle would be expected to remain consistent.

We have now added this aspect in the discussion of the revised manuscript along with other uncertainties that may influence the conclusions:

"*Additionally, spatiotemporal variations in ecosystem processes, such as the partitioning of new detritus into POM and DOM, grazing rates, and temperature-dependent remineralization rates, could introduce regional and interannual variability not explicitly accounted for in this study. While these processes contribute to organic matter cycling and could influence our findings, the model's accurate representation of OM concentrations and stoichiometry ensures that the broader conclusions regarding regional carbon fluxes remain robust.*" (page 36, lines 926–930)

Line 261 and Fig 3: In addition to preferential remineralization and DOC release, DON or DOP direct uptake by phytoplankton is another potential pathway contributing to deviations from the Redfield Ratio. Can this pathway (DON or DOP direct uptake) be incorporated into the model? Or at least, please acknowledge this pathway and explain why it is not included in the model.

We thank the reviewer for suggesting the direct uptake of DON and DOP by phytoplankton as another potential pathway contributing to deviations from the Redfield ratio. As pointed out in the original manuscript, the release of carbon-enriched DOM in our model includes DOC, DON, and DOP, with the composition depending on nutrient availability. However, the direct uptake of DON and DOP by phytoplankton (as for example described by Fitzsimons et al., 2020) is not yet considered in our current model version. The current representation of the release of carbon-enriched DOM in our model, which directly converts nutrients to DOM, was chosen as a compromise between an accurate representation of OM cycling and a feasible model complexity. This has already been described in the original manuscript version:

"*In accordance with Neumann et al. (2022), we assume this provides a reasonable simplification, as healthy phytoplankton cells only marginally deviate from Redfield ratios compared to DOM and POM (Ho et al., 2003), while also limiting computational cost with reduced model complexity*." (page 8, lines 199–201)

Incorporating direct pathways for phytoplankton uptake of DON and DOP would require the introduction of independent C, N, and P state variables for all phytoplankton groups, as well as for both zooplankton groups that graze on them. These additional variables would add up to ten more variables, which would substantially increase the model complexity. As an avenue for future model development, the introduction of independent C, N, and P variables could enable the inclusion of additional pathways for variable OM stoichiometry. This could allow for the explicit consideration of pathways such as the microbial loop, including the direct uptake of DON and DOP by

phytoplankton, as well as a more detailed representation of microbial respiration. However, this is beyond the scope of this study.

We have now modified the following sentences in the discussion of the revised manuscript to address this aspect and acknowledge additional pathways for variable OM stoichiometry that are not yet included in our model:

"*Other effects on OM composition that are beyond the scope of this study include the quality of imported OM from land (Painter et al., 2018), the effect of variable stoichiometry in prey on higher trophic levels (Schindler and Eby, 1997), and pathways for variable stoichiometry in phyto- and zooplankton (Moreno and Martiny, 2018). The latter pathways include the direct uptake of DON and DOP by phytoplankton (Fitzsimons et al., 2020), as well as variable C:N:P ratios in zooplankton grazing and excretion (Anderson et al., 2005; Elser and Urabe, 1999).*" (page 36, lines 919–923).

Line 296: why in the fourth configuration (EP&PR), the contributions have been reduced?

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 focusing on 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 (Fig. 4 and 5), we conducted sensitivity tests to estimate the appropriate remineralization ratios and the scaling factor B_{ER} for the ER parametrization. We assume that both the 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 be much higher than those observed and in the other configurations, making the comparison across configurations less meaningful.

For clarifying this reasoning behind these parameter settings, we have now added it explicitly to the revised manuscript and updated the following sentences:

"*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. Accordingly, we reduced the contribution of each mechanism in the ER&PR configuration to ensure consistency across all configurations. We found that using the parameter settings from the individual configurations in the combined ER&PR configuration would result in C:N and C:P ratios significantly exceeding those observed and in the other configurations, making their comparison less meaningful.*" (page 14, lines 377–382)

Line 334: for costal DOC, DON and DOP concentration data, it might be worth looking at CoastDOM v1 database. It collects many coastal DOM concentration data.

Lønborg, C., Carreira, C., Abril, G., Agustí, S., Amaral, V., Andersson, A., ... & Álvarez-Salgado, X. A. (2024). A global database of dissolved organic matter (DOM) concentration measurements in coastal waters (CoastDOM v1). Earth System Science Data, 16(2), 1107-1119.

We thank the reviewer for pointing out the CoastDOM v1 database, which indeed provides valuable data for coastal DOM concentrations. This database will serve as an excellent reference for future analyses and model validation of bulk DOM concentrations and stoichiometry. However, we would like to emphasize that the simulated DOM in our model only includes the labile fraction. As such, it would not be appropriate to use the CoastDOM v1 data as a reference for the range of variations shown in Figure 4, as the data encompass both labile and refractory DOM fractions.

We have incorporated this reference into the introduction of the revised manuscript to better highlight the relevance of coastal DOM studies, alongside several other references that demonstrate variations in OM stoichiometry:

"*Despite observational evidence for large global and regional variations of elemental C:N:P ratios in dissolved (DOM) and particulate (POM) organic matter (Hopkinson and Vallino, 2005; Liang et al., 2023; Loh and Bauer, 2000; Lønborg et al., 2024; Martiny et al., 2013; Tanioka et al., 2022b), many biogeochemical models assume a constant Redfield stoichiometry of C:N:P=106:16:1 or similar, originally derived as an average ratio for the entire global ocean (Redfield, 1963).*" (page 2, lines 50–53)

Additionally, we have included the median, along with the 25th and 75th percentiles of bulk DOC, DON, and DOP concentrations, as well as bulk DOM stoichiometry, in Supplementary Tables S3 and S7.

Section 3.1.1: in addition to the discussion of total carbon fixation, it would be interesting to provide a more detailed analysis of the specific changes in primary production among the three modeled species (flagellates, cyanobacteria, and diatoms).

We agree that a more detailed analysis of the specific changes in primary production among the three modeled species (flagellates, diatoms, and cyanobacteria) would offer interesting results. While we could have saved the individual contributions of each species to the DOM release as diagnostic variables during the simulations, we decided not to for the following reasons:

1. To ensure that we accurately represent the contributions, a specific validation would be required. Without sufficient validation, the results would be arbitrary.

2. The current manuscript focuses on assessing the impact of variable organic matter stoichiometry on carbon fluxes. Exploring potential consequences for phytoplankton community composition and phenology should be assessed individually with a sufficient consideration of observational studies on the stoichiometry and community composition of marine primary producers.

As the manuscript already focuses on the broader implications of variable organic matter stoichiometry on regional carbon fluxes, we suggest not including this additional analysis in the present work. It is beyond the scope of the already extensive analysis. However, we view this as an avenue for future research, which would require additional model refinement and validation. We therefore have added a sentence in the revised manuscript to the discussion section, in the paragraph outlining the outlook for future work:

"*Another important factor to consider is the role of community composition in shaping ecosystem stoichiometry* (Kwiatkowski et al., 2018)." (page 36, lines 923–925)

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 have now incorporated the reviewer's suggestions into the revised manuscript version 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 "decrease 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 have now revised 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 ensures clarity and prevent potential confusion for readers. Here is a list of the corresponding changes in the revised manuscript:

- "*In terms of the spatial distribution, the increase in **CO_2 uptake** is not uniform across the shelf, but rather amplifies existing regional differences* (Fig. 11)." (page 26, lines 688–689)
- "*The largest increase in the **CO_2 uptake** is consistently shown in the deeper central shelf areas and parts of the outer shelf regions including the NNS, CNS, NT, SWC, MS, AS, and WI.*" (page 26, lines 693–694)
- "*A persistent increase in **CO_2 outgassing or decrease in CO_2 uptake** is only shown in a narrow band along the Norwegian coast, near the Elbe estuary, and in inner shelf regions including the EC and IS. In the Atlantic Ocean, the **CO_2 uptake** consistently increases.*" (page 26, lines 697–699)
- "*For each season, the smallest **differences in uptake and outgassing** are shown in the PR configuration, while the largest are seen in the ER configuration. In winter, the **CO_2 uptake** is reduced by 0.2 to 0.8 molC $m^{-2}yr^{-1}$ across the entire NWES*." (page 28, lines 725–727)
- "*During the spring and summer months, the **CO_2 uptake** is significantly stronger, with the most pronounced increases in the central North Sea and the Norwegian Trench*." (page 28, line 728–729)
- "*In autumn, the gradient between uptake in the northern and outgassing in the southern parts is enhanced, with an average **decrease in uptake or increase in outgassing** of up to 0.2 molC $m^{-2}yr^{-1}$."* (page 28, lines 730–732)
- "*An increased export from surface waters and subsequent respiration of POC likely contributes to the abovementioned higher drawdown of DIC, and the resulting increase in **CO_2 uptake***." (pages 33–34, lines 848–850)

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. We believe that this is already clearly stated in the original manuscript:

"*Cyanobacteria production is additionally temperature dependent and only possible under sufficient light and low-salinity conditions in the Baltic Sea.*" (page 8, lines 176–177)

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 and grazing rates, 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 now provide additional clarification in the discussion section of the revised manuscript to further explain the rationale behind this partitioning choice and its relationship to other key model parameters, such as remineralization and grazing rates:

"*Additionally, spatiotemporal variations in ecosystem processes, such as the partitioning of new detritus into POM and DOM, grazing rates, and temperature-dependent remineralization rates, could introduce regional and interannual variability not explicitly accounted for in this study. While these processes contribute to organic matter cycling and could influence our findings, the model's accurate representation of OM concentrations and stoichiometry ensures that the broader conclusions regarding regional carbon fluxes remain robust.*" (page 36, lines 926–930)

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 takes up 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 was missing from the main text and may cause confusion regarding the mass balance. To address this, we now mention both the uptake of DIC and nutrients in the methods section of the revised manuscript and point out that the full ecosystem model equations are described in the supplementary material:

"*The OM release results in a corresponding uptake of DIC and nutrients, independent of phytoplankton biomass, which is described as part of the full ecosystem model equations in the Supplementary Material (Section 2).*" (page 9, lines 225–227)

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 of the original manuscript (page 9, lines 209–212). We have revised the following sentence to clarify the use of the parameter:

"*The ER base rate E defined in Eq. 1 is a function of phytoplankton biomass C_{Pj}, the respective maximum uptake rates σ_j, an optional temperature dependence β_{Tj} (here only relevant for cyanobacteria) and a constant scaling factor B_{ER} , which controls the range of stoichiometric variations and organic carbon concentrations to ensure an accurate representation.*" (page 9, lines 210–213)

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 Equations (2) and (6). 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.

As this condition is already implicitly satisfied, we have now added it in Equations (2) and (6) for completeness and comprehensiveness of the mathematical formulation.

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 the methods section, 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 already detailed in the original 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 original manuscript which describes 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.

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. To clarify the reasoning behind this choice, we modified the following sentences in the revised manuscript:

"*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. Accordingly, we reduced the contribution of each mechanism in the ER&PR configuration to ensure consistency across all configurations. We found that using the parameter settings from the individual configurations in the combined ER&PR configuration would result in C:N and C:P ratios significantly exceeding those observed and in the other configurations, making their comparison less meaningful.*" (page 14, lines 377-382)

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

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 already summarized in the methods section of the original manuscript:

"*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.

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 have rephrased the sentence to better clarify the distinction between the North Sea and the entire NWES. This revision ensures it is clear that the numbers refer to rates in different regions, rather than pelagic remineralization and carbon fixation:

"*In the RS configuration, the magnitude of pelagic remineralization is comparable to that of carbon fixation, with annual means of* $4.5\ TmolC\ yr^{-1}$ *in the North Sea and* $9.6\ TmolC\ yr^{-1}$ *across the entire NWES.*" (page 22, lines 586–587)

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. However, in some inner shelf regions that show a CO_2 uptake in the reference configuration, the uptake decreases. For this reason, we decided to mention both an increase in outgassing and a decrease in uptake in this specific case. We agree that the phrasing should be revised to better reflect this, and we have revised it accordingly:

"*A persistent increase in* ***CO_2 outgassing or decrease in CO_2 uptake*** *is only shown in a narrow band along the Norwegian coast, near the Elbe estuary, and in inner shelf regions including the EC and IS. In the Atlantic Ocean, the* ***CO_2 uptake*** *consistently increases.*" (page 26, lines 697–699)

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 have carefully reviewed 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 have now adopted a more explicit terminology in the revised manuscript to better reflect the underlying processes and avoid confusion. Here are the specific revisions in the sentences mentioned in this comment:

- "*For each season, the smallest* ***differences in uptake and outgassing*** *are shown in the PR configuration, while the largest are seen in the ER configuration. In winter, the* ***CO_2 uptake*** *is reduced by 0.2 to 0.8 molC* $m^{-2}yr^{-1}$ *across the entire NWES*." (page 28, lines 725–727)
- "*During the spring and summer months, the* ***CO_2 uptake*** *significantly stronger, with the most pronounced increases in the central North Sea and the Norwegian Trench*." (page 28, line 728–729)

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. 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. We have corrected the titles accordingly and update Supplementary Figure S18.

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.

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 changes 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.

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