AC3

General comments:

Overall, the manuscript is well written and clearly structured. The methodology is straightforward, appropriately explained, and well suited to address the key questions. I only have minor comments, mainly regarding the structure and clarity of the results and discussion sections.

We thank the reviewer for their positive feedback and appreciate the valuable suggestions.

Specific Comments:

Line 149: Please add some details on the specific optodes used here.

We refer the reviewer to line 131, which provides details on the specific optodes used.

Lines 152-154: Can you give a range here for the duration of the 'closed' part (e.g., 1-3 days)?

We thank the reviewer for this suggestion. We have added the duration of both incubation periods to the revised manuscript (specifically to line 145), specifying that the closed part lasted around 3 days, and the open part around 7 days.

Line 155: Please state somewhere the volume of seawater enclosed in the chambers and whether any correction for seawater dilution was done for the closed incubations.

We thank the reviewer for pointing this out. We have stated the volume of enclosed seawater in the chambers in section 2.1, and we have clarified in section 2.5 that solute concentrations were corrected for dilution resulting from sample volume replacement in both closed and open incubations.

Line 156: Can you be more specific here; e.g., sampled every time O2 dropped by 10 %?

We have clarified the sampling strategy in the revised manuscript by specifying that sampling occurred after approximately every 8% decrease in O_2 saturation, as determined from the continuous optode measurements.

Line 210: Move definition of ICP-OES to line 171 (first occurrence).

We thank the reviewer for pointing this out. We have moved the definition of ICP-OES to its first occurrence in line 171.

Line 263: I recommend changing to 'in the closed (a, b) and open (c-e) incubations'…

We thank the reviewer for this suggestion. In light of comments from all three reviewers (see responses to similar comments above), we have decided to remove Figure 3 from the revised manuscript.

Figure 3: I find the setup of Figure 3 slightly confusing (e.g., mix between open and closed incubations). Maybe move c) to middle panel and clearly state that upper panel shows closed incubations and middle panel open incubations. I would also recommend reducing the range for ammonium concentrations (y axis in e). Keeping it at the same level as nitrate is somewhat meaningless here. Also, why did you choose to only depict a representative incubation rather than showing the variability as well? Wouldn't mean plus standard deviation be more meaningful here?

We thank the reviewer for the suggestions. As explained in our response to the comment on line 263, we have decided to remove Figure 3 from the revised manuscript.

Figure 4: I find using positive fluxes to indicate both a flux into the overlying water (DIC, alkalinity) as well as a flux into the sediment (oxygen) confusing. Consider changing oxygen fluxes to negative values. Also, consider adding nutrient fluxes to the main figure here rather than the supplements.

We thank the reviewer for this suggestion. We have revised the oxygen flux figure to report oxygen uptake as negative fluxes. We have also considered including nutrient fluxes in the main figure. However, we decided to retain these results in the Supplementary Information because clear nutrient flux signals were only observed during the first week of incubation, whereas fluxes during the remainder of the experiment were generally close to zero.

We therefore believe that including nutrient fluxes in the main figure would add limited additional information compared with the primary focus of the figure on oxygen, DIC, and alkalinity fluxes.

Lines 372-373: This sentence feels out of place and raises more questions than it answers. Please provide a better transition to sections 4.2 and elaborate on these additional alkalinity sources and sinks here or elsewhere in the discussion.

We thank the reviewer for this helpful suggestion. We agree that the original transition was abrupt. We have revised the text to explicitly state that the deviations observed in weeks 3, 11, and 13 indicate the presence of additional alkalinity-generating or alkalinity-consuming processes, and we now refer the reader to the following section where these processes are further discussed.

Lines 406-407: Can you be more specific here, i.e., state specific reactions and/or metabolites.

We thank the reviewer for this suggestion. We have included the relevant reactions to the discussion.

Lines 422-424: Very fine shell material was likely also added to the sediment given that all ground shell material smaller than 0.25mm was retained for the experiment. Can you speculate on potential impacts of clogging of pore space on oxygen penetration and observed solute fluxes? Was the grain size distribution of the ground shell material measured?

We thank the reviewer for raising this important point. The grain size distribution of the crushed shell material was unfortunately not measured separately. We acknowledge that the addition of fine shell particles could potentially influence sediment permeability and pore-water exchange by partially filling pore spaces. However, we expect this effect to have been limited because shell material represented only approximately 8 wt% of the amended sediment layer and was homogenized with the surrounding sediment. Furthermore, the rapid oxygen consumption observed following shell addition, together with the accumulation of ammonium in the overlying water, indicates that enhanced oxygen demand from mineralization of the organic matter associated with the shells was likely the dominant driver of oxygen depletion. This interpretation is further supported by the occurrence of oxygen depletion in the overlying water in the first week, which would not be expected from reduced pore-water exchange alone. Nevertheless, we acknowledge that changes in sediment permeability caused by fine shell particles cannot be completely excluded.

Line 460: Is this a new steady-state? It seems that additional timepoints would be required to show whether this is a new steady-state or if it decreases further to meet control values.

We thank the reviewer for this observation. Although the mean dissolution rates in week 24 (2.2 ± 1.1 mmol m^{-2} d^{-1}) and week 21 (7.0 ± 3.3 mmol m^{-2} d^{-1}) were comparable within their associated uncertainties, we agree that the data do not support describing the dissolution rate at the end of the experiment as a new quasi-steady state, as it was still gradually decreasing at the end of the experiment. We have therefore revised the text to remove the reference to a quasi-steady state. Instead, we now describe the dissolution rates in the treatment chambers as gradually approaching those observed in the control chambers, while remaining slightly elevated at the end of the experiment.