Detailed responses to reviewer 3 (reviewer comments are included in black, responses in blue font)

Overview

The manuscript describes a novel BGC model that is run within a physical model (ROMS) which itself can be run at 3 described resolutions (ROMS-H1, ROMS-H2, ROMS-H3). ROMS-H2 is the workhorse resolution here. The novel BGC model here is a cut-down version of a more complete BGC model (not clearly named here) that essentially simplifies the BGC to DIC, TA and O2, with major missing processes parameterised and described here. This model is then used in a series of OAE experiments. The manuscript details coupled physics-biogeochemistry model specifically designed for OAE in a nested grid configuration and reduced biogeochemistry with increasing spatial resolution from Scotian Shelf to Halifax harbour. To ensure that the model is suitable for the location, hindcast simulation of the model is validated against observation on the shelf. Alkalinity enhancement experiment is simulated at the inner, mid, and outer harbour with two different feedstocks; fully dissolved and fully particulate. Then the effect of alkalinity addition to the carbonate system is analysed. A major conclusion is around the relative success (69%) of CO2 absorption driven by OAE within the modelled coastal domain.

Overall, the manuscript is an interesting investigation into OAE in a very specific locale, Halifax Harbour. In most places, the manuscript is written quite well, and the model has been generally shown to capture the observations. The manuscript also explores different locations of alkalinity addition and types of feedstocks, which are relevant and can be insightful for field study and MRV. However, while we generally appreciated it, there are a few suggestions that would make the manuscript stronger.

Response: We appreciate the assessment of the manuscript and the feedback. We provide point-by-point responses to the reviewer's comments below

General comments

Comment:
1. A general comment we'd make is that the structure of the manuscript impedes its interpretation. In particular, it merges model description, experiment design and results from the OAE side of the work into a single and lengthy section. There's nothing special in the work that precludes a conventional method-results-discussion structure, so please reformulate the manuscript this way.

Response: The structure of the manuscript is probably a matter of personal preference. Reviewer 1 said that the manuscript was "*well laid out, focused and easy to follow*". We chose to split the model description/results into three parts (physics-shelf BGC, reduced BGC, OAE module) for clarity. Using a more conventional structure would not improve the interpretation because the three parts mentioned above would be mixed, likely introducing confusion for the reader. We feel that the current structure of the manuscript is best to describe the model and present the results of the simulations with OAE.

Comment:
2. Another general comment is that the use of a novel BGC model here introduces the requirement for a lengthy digression into the formulation and skill of this model. Ideally, such a model would be described in a separate manuscript and then used for the problem at hand. As written, it is sometimes unclear whether the paper's primary aim is to describe the novel OBGC model itself or to address OAE research problem, which in our opinion, makes the narrative harder to follow. We would suggest clarifying the manuscript's focus and streamlining the model description, so that this description is largely moved to supplementary and shortened in the main body.

Response: We disagree that the introduction of the model is a lengthy digression. In fact, the description of this model is one of the main points of this paper (i.e. this is the "separate manuscript" describing the model that the reviewers seem to be asking for). The manuscript investigates alkalinity additions in an idealized setup (Section 5). An application of the model to the ongoing OAE field studies in Halifax Harbour will be a separate manuscript. Given the structure of the manuscript, a reader who is not interested in methodological details, can focus on the OAE experiments. We moved most of the validation and additional figures and tables to the supplement to keep the main body clear. We also moved some of the general BGC equations to the supplement. We hope that the manuscript is a good compromise between methodological details and readability for most readers.

Comment:
3. On a related point, because the model used here is novel, it would be extremely beneficial to compare it with its parent model. At present, the validation in Supplementary Material appears to compare / validate the parent and novel models separately rather than together. For example, Figures S3 to S8 appear to use the parent, while Figures S10-S19 appear to use the novel model, but there are none intercomparing the shared properties of the models with observations. Given how different the models appear to be in their state variables and formulation, it is critical for readers to understand how strong the relationship between the models is.

Response: See also response to Comment 5 by Reviewer 1. ROMS-H1 simulates circulation and biogeochemistry on the shelf, whereas ROMS-H2 and ROMS-H3 simulate background carbonates and OAE in the nearshore and inside the Halifax Harbour. While the later can be compared (e.g., Section 5.2.4, see also Wang et al., 2025), the purpose of ROMS-H1 is to provide accurate boundary conditions to ROMS-H2, which then simulates the dynamics of the Halifax Harbour. We do not feel that comparing ROMS-H1 and ROMS-H2 at the same stations (ST2 and BBMP) would provide much insight since ST2 is located near the open boundary of ROMS-H2 and ROMS-H1 does not resolve the Halifax Harbour circulation well (BBMP).

Comment:
4. We would also suggest that the manuscript would be more complete if the authors also discussed other diagnostics that are relevant for impact assessments. Such as how OAE affects Halifax Harbour's pH and saturation states (e.g. Ω aragonite) over the course of alkalinity addition. Those are liable to be important for natural ecosystems in the region.

Response: pH and saturation states are important variables in the context of OAE, as an indicator for stress but also because of the regulatory limit on pH and the potential for precipitation with higher saturation states. Looking at the effect of alkalinity additions on these variables is important and will be the focus of a follow-up manuscript.

Specific comments

Comment:
5. Abstract: A little light on actual results. I would have expected a statement regarding either the efficacy or challenges encountered during the work.

Response: We will add a statement about the outcome of the OAE simulations at the end of the abstract. Further, we note that one of the purposes of this manuscript is to describe the model (see our response to comment 2).

Comment:
6. Line 90: some models, e.g. Palmieri & Yool (2024), assumes that particulate material is added, and uses the calculated dissolution rate of this to specify the flux. In this specific model, yes, the model only sees a flux of TA, but this is achieved through the temperature-dependent dissolution rate of an implicit particulate source (which is assumed to have already settled on the seafloor).

Response: The sentenced will be modified as follows:

"Most models assume the addition of fully dissolved rather than particulate material (e.g., Kwiatkowski et al., 2023; Nagwekar et al., 2024; Zhou et al., 2025), whereas mineral feedstock particles will dissolve over time (Schulz et al., 2023). Palmiéri and Yool (2024) assumed dissolution but of an inert particulate feedstock at the sediment-water interface."

Comment:
7. Line 96-103: It might have expected to see some articulation of the key research questions here. This seems primarily a breakdown of how the manuscript is organised. (Of which, see below.)

Response: This paragraph will be modified as follows:

"*To fill the gaps described above, we present a high-resolution, coupled physical-biogeochemical-addition-dissolution model that is designed to support OAE research in coastal environments. The model builds on Wang et al. (2025) to include oxygen and carbonate system chemistry in a realistic alkalinity addition setting. The model is validated and tested for Halifax Harbour. The key question that is investigated with this novel model is how feedstock type and dosing location influence alkalinity dispersion and net CO_2 air-sea flux in the Halifax Harbour and surrounding nearshore waters.*

The observations used for the study are described in Section 2. The circulation model is presented in Section 3 and the results of an eight-year simulation (2016-2023) validated against observations on the shelf and in the harbour are presented. The biogeochemical

model is described in Section 4 and validated for 2016-2023. The addition model is then presented and a series of OAE simulations carried out in Section 5. The overall results are discussed in Section 6."

Comment:
8. Methods: There are 6.5 pages of model description before we get to the OAE part. Ideally, the model used would be an existing model, previously described elsewhere. However, we are where we are. We would suggest abbreviating this to a ~1 page summary that cites prior work and moving the more expansive text to an appendix. It's useful – in fact, given this is a novel model, *critical* – to have all of this, but this level of detail tends to distract from the focus of the paper.

Response: Again, when the reviewers say here that we should have used an "existing model" that should have been "previously described elsewhere" they are missing the point that this is the first time it is described. It will be used for more in-depth studies in forthcoming manuscripts. The information included in the main manuscript only covers the novel parts of the model and we feel this is critical to the understanding of the paper. The validation figures that are not essential were included to the supporting material. Also see response to Comment 2 above.

Comment:
9. Ln. 137: It would be helpful if the BGC models here were given names so that they can be clearly identified, and clearly separated from the physical frameworks they are coupled to.

Response: The full biogeochemical model used on the shelf (ROMS-H1 grid) is referred as "*the explicit biogeochemical model*" throughout the manuscript, whereas the model developed for alkalinity addition and used in H2 and H3 grids was named "*the reduced complexity biogeochemical model*". Given the structure of the manuscript the explicit biological model is first discussed in Section 3 and then the reduced complexity biogeochemical model is discussed in Section 4. We feel that this should avoid confusion. For clarity, we add the following in Section 3 (L150):

"*ROMS-H2 and ROMS-H3 are coupled with the reduced biogeochemical model (see Section 4).*"

Comment:
10. Line 300: Section 5 appears to combine model description with model results. This doesn't seem a helpful way to present the work. We would suggest creating / merging into separate sections to 1. describe the model (part of methods), and 2. describe findings from its use (the results section). Similarly, the validation of the model ahead of its use could either be arranged formally part of the methods (as it kind-of is now) or moved to be part of the results.

Response: See response to Comment 1 above. We feel that merging Section 5 with the previous sections would lead to confusion. The current structure where all alkalinity

addition (model description, experiments, results) is in the same section seems easier to follow.

Comment:
11. Line 301-303: a verbal overview description of the OAE scheme might fit well here. Specifically, that OAE TA is added to the ocean in dissolved TA and particulate TA forms (the latter requiring a new tracer), and that the latter dissolves into the former with time. Also, this whole model description completely overlooks Figure 2 which rather clearly indicates how the model works.

Response: The introductory paragraph will be modified as follows:

"*To simulate alkaline feedstock addition (often in a liquid form, e.g. slurry) and the effect of added alkalinity on the carbonate system, several tracers were added to the biogeochemical model. An overview of this model, including the addition module, is presented in Fig. 2. The alkalinity addition module is described in detail below. It was designed so that a single simulation can be used to calculate the difference between the realistic addition case and counterfactual*"

Comment:
12. Line 306: It could be helpful to explain why new delta tracers were added rather than the more conventional compare-and-contrast with a control simulation. One can imagine what the explanation is, but readers would benefit from understanding this.

Response: Models running on HPC systems are typically not bit-reproducible making the conventional approach to compare-and-contrast with a control simulation not ideal. Additionally, running simulations is essential to MRV but has also a carbon footprint (energy consumption), which is improved with the use of dual tracers.

Comment:
13. Ln. 310-312: This describes the split between dissolved and particulate TA additions but does not clarify what the fraction is or how it may vary. It also seems like this is something that isn't explored elsewhere in the manuscript, e.g. a sensitivity analysis on the importance of this fraction might be expected. In any case, one would at least expect to be told what its value was, or – if this is variable – under what conditions it varies.

Response: The value of θ depends on the characteristics of the feedstock. Since we used either fully dissolved or fully particulate feedstock in the experiments, we only set θ to 0 or 1. Therefore, our results already show the maximum range within which sensitivity experiments for θ would lie.

In the revised manuscript the sentence will be clarified as follows:

"*The allocation of TA_{in} into the dissolved (ΔTA) and particulate (TA_P) pools is set by the parameter $\theta_{P:D}$. This representation of dissolution of the particulate stock builds on the formulation in Wang et al. (2025). The value of $\theta_{P:D}$ varies for each feedstock and needs to be assessed prior to dosing. Simulation results (see below) from experiments with fully dissolved ($\theta_{P:D} = 0$) or fully particulate ($\theta_{P:D} = 1$) indicate the maximum effect of $\theta_{P:D}$.*"

The sentence introducing the experiments (L335) will be changed to:

"... each with two different feedstocks, either fully dissolved ($\theta_{P:D} = 0$) or fully particulate ($\theta_{P:D} = 1$)."

Comment:
14. Eqn. 8-9: We would suggest adding the equation for TA so that the relationships between TAp and delta-TA are easy to understand. Same for DIC.

Response: The equations for TA and DIC are available in the supplementary material (mentioned on L229). Here we focus on the alkalinity addition module. ΔTA and ΔDIC are independent variables from TA and DIC. They are only used in combination to calculate air-sea CO_2 flux.

Comment:
15. Ln. 329: This section is sorely missing a clear non-narrative description of the experiments undertaken and the simulations performed. A table listing the simulations and their roles would make it very easy for readers to understand the work undertaken.

Response: This table will be added to the revised manuscript.

Comment:
16. Ln. 329: Technically, it could be argue that this subsection is methods, but the organisation of subsection 5.2 is compromised by a subsubsection 5.2.1 which is more results than methods. Reorganising into the conventional methods-results-discussion format would greatly improve this manuscript.

Response: See response to Comment 1 above.

Comment:
17. Ln. 331: superscript typo, 1.29 mol s^{-1}

Response: Done.

Comment:
18. Ln. 363-371 and Ln. 383-403: these sections of text quantify the results found verbally rather than, more obviously, in a table (or tables). This would allow the reader to clearly understand distinctions being drawn between experiments and locations that currently require the reader to hold a lot in their heads at once just to make basic sense of the results.

Response: The information discussed in these sections is shown in Figures 7 and 9, which is why a table was not included. The information will be summarized in a new table in the revised manuscript.

Comment:
19. Ln. 364: The manuscript presents a maximum achievable uptake of 0.89 mol CO2 per mol TA, perhaps we are missing something, but can the authors elaborate more on how

this number came up? Will this number change with seawater condition / location / model resolution?

Response: This number was calculated based on simulated conditions in the Halifax Harbour, as mentioned L365. We will provide more details on this calculation in the revised manuscript.

Comment:
20. Ln. 448: The balance in this manuscript between the base model and the OAE experiments is generally off, and this section (6.1) continues this. Manuscripts like this one generally present just enough information about the base model to satisfy readers that it's an appropriate choice for the experiments in question. Also, and more importantly, it blends in details about the OAE experiments that might better be discussed in the relevant section (6.2, 6.3).

Response: See response to Comment 1 above. Model validation is important, and Section 6.1 discusses both the validation of the current model and the possibility of validating a relocated version of the model. We feel that it is better done in a separate section than blended into Sections 2 and 3.

Comment:
21. Ln. 449-450: Why "not surprisingly"? Also, similar performance to what? Is there maybe a grammatical error in this sentence.

Response: For clarity, we will modify the sentence as follows:

"*The circulation model is modified from the nested model of Wang et al. (2025) using rotated and refined grids. Not surprisingly, the physical model had similar performance to Wang et al.'s version when compared with observations (Table 1).*"

Comment:
22. Ln. 551: "Tufts Cove is the location for alkalinity …" should this be "Tufts Cove is the *ideal* location …"?

Response: The Halifax Harbour is a site for alkalinity addition since 2023. Dosing is done at Tufts Cove. Our results did not show that Tufts Cove is an ideal location for dosing.

Comment:
23. Ln. 591: "These results stress the importance of operational design as well as the use of high-resolution regional models when quantifying additionality." – Can you be clearer on what, specifically, is meant by "operational design" here? Is it geographical / depth choice of TA release, the balance of dissolved / slurry supply of TA, the timing of application of slurry (w.r.t. tides)?

Response: The sentence will be changed to:

"*These results stress the importance of operational design, such as dosing location, type (dissolved, particulate), slurry properties (dissolution rate, sinking speed), as well as the use of high-resolution regional models when quantifying additionality.*"

Comment:
24. Figure 2: This shows pH in a similar way to model state variables. But it's a calculated property rather than a state variable. Maybe alter the line style of the box so that it's clear that it's not a state variable?

Response: The line style will be changed to dashed.

Comment:
25. Figure 3: while I have used Ocean Data View-style palettes like this before in my own work, I now appreciate that they present particular difficulties for readers with colour vision issues. To which end, please convert this figure to a more suitable palette, plus subsequent ones using the same palette.

Response: We will change to a colorblind friendly palette.

Comment:
26. Figure 5-6: This seems to use a different geographical domain to Figure 3 onward. Is there a reason for this? More generally, why do these figures include satellite imagery while the others don't? (Figure 1d also shows slightly different domain limits than in Figure 3.)

Response: The limits of the three model domains are presented in Figure 1c. However, it is not always pertinent to show the entire model domain. For example, enlarging Figures 3 and 7 would only show additional values ~0 (pink) but the relevant areas (Halifax Harbour) would be smaller. This is mentioned in the captions. Showing the entire grid area in the other plots would not add more information. For clarity the following will be added to the captions: "(*offshore areas were excluded from the map)*".

We did not include satellite imagery on Figures 3, 7 and 10 because these figures are compact and satellite imagery did not work well in this context.

Comment:
27. Figure 7: needs rotating.

Response: The figure was rotated to be larger for the review but will not be rotated for final submission.

Comment:
28. Figure 10: needs rotating.

Response: See response to Comment 27 above.