Detailed responses to reviewer 2 (reviewer comments are included in black, responses in blue font)

General comments

This is a very interesting and thoughtful paper on a numerical modelling approach to detect and evaluate the effects of OAE. It represents a significant step forward towards realistic simulations of an actual alkalinity release field experiment. I think the paper can be accepted for publication after moderate revisions and clarifications.

There are several questions I hope the authors can clarify.

Response: We appreciate the constructive comments. We will revise the manuscript accordingly and provide point-by-point responses to the reviewer's comments below.

Specific Comments/Questions

Comment:
1. I like the approach to separate the slurry additions into dissolved TA input and particulate form which later dissolve and sink. This is more realistic than the previous modelling approach which adds TA in dissolved form. It also counts for lost TA due to particle sinking onto the seabed. It was interesting to see the result that the maximum CO_2 uptake from this mixture is lower. On the other hand, the more realistic model representation comes at the expense of introducing three additional parameters: the particle dissolution rate, the particle sinking rate and the fraction of slurry particles incorporated onto the sediment, the last of which would be difficult to estimate. In reality, there would be a size spectrum of slurry particles which dissolve and sink. As shown in Fig. S10 in Wang et al. (2025), the sinking velocity varies by two orders of magnitude for alkaline feedstocks of various sizes. It is possible that the dissolution rate may also change with the particle size. Wang et al. (2025) also showed the results are very sensitive to the particle dissolution rate and sink velocity. How does one choose one "representative" particle with a particular size, dissolution rate and sinking velocity? I understand the need to keep the model manageable, but these are model assumptions that could be discussed. There are approaches to model a size spectrum of bubbles generated by breaking waves (Garrett et al., 2000). The bubbles are injected into the upper ocean, rise due to buoyancy and dissolve under partial pressure differences. Maybe some of these modelling approaches could be discussed. Sediment transport modelling has to deal with a spectrum of particle sizes too and it is well known the settling site of sediment depends critically on the particle size.

Response: Given the high spatial model resolution, simulating a particle size spectrum is not feasible. It would be computationally too expensive. Detailed models of size-spectra, like the one for bubbles and those used for sediment transport either use simplified physical models (e.g. 1-dimensional) or are run only for a few weeks (in the case of sediment transport coupled to a 3-dimensional ROMS model). Here we used a highly resolved physical model of the Harbour and had to compromise on using a single parameter for dissolution and sinking rates to represent the bulk of particle sizes. This is a limitation of the model. To clarify this point, we will add text in Section 5.2 to justify our choice. We will also mention how the dissolution and sinking parameters used for the experiments with particulate feedstock were calculated. Finally, we will discuss this limitation in Section 6.4 (Current limitations and future development).

Comment:
2. I am also curious about the author's approach to use a high-resolution hydrodynamic model but a simplified biogeochemical model. I can understand the need for high resolution hydrodynamic to resolve the near-field transport and dispersion of added slurries in the inner model domains but do not quite understand the use of a simplified biogeochemical model. Was it due to the high computational cost of the full biogeochemical model? I thought the biogeochemical model can be run very efficiently if done on an offline mode. It would be good to discuss why the authors took this modelling approach. It will be instructive to other modelers.

Response: As mentioned in Section 4, the reduced complexity biogeochemical model was designed to improve computational efficiency. In our experience, offline models do not match well the more accurate results from online simulations which is why we don't use them. The simplified biogeochemical model has an intermediate level of complexity that we deem to be fit-for-purpose. If we were to run the full biogeochemical model, there would be no direct interaction between the nitrogen and phosphorus species in that model and the carbonate system. The simplified model delivers what we need in terms of describing the seasonal cycle of background DIC. The comparison with observations at the monitoring station in the Bedford Basin shows that despite the simplification the model can simulate the background carbonate system appropriately. Further complexity is therefore not needed in the context of OAE. We will expand the justification for using a reduced complexity biogeochemical model in Section 4 of the revised manuscript.

Comment:
3. There is also this broad question how we can validate the model results and document the OAE effects. The authors did a lot of model validation without OAE but none for the model results with OAE. How can the model help the documentation and verification of alkalinity addition? The latter is a nagging issue facing all OAE studies, due to a combination of large natural variability in the carbonate system and the policy restriction/regulation on exposure impacts.

Response: The manuscript describes the OAE model and provides insight on the effects of release locations and feedstock type in the Halifax Harbour. We did not intent to carry out MRV with these experiments, but the reviewer makes a good point about the need of validating the model with OAE in the case of MRV. This will be the focus of a follow-up study, as mentioned in the Conclusions (L593). In this context, a set of observations needs to be collected directly at and near the dosing location.