General response to Reviewer #1: We sincerely appreciate the reviewer's recognition of our work and the thoughtful suggestions for improvement. In response to the main recommendation, we have added further details on the validation of the model simulation and the maximum attainable CDR efficiency in North Sea waters in the main text. In addition, we have carefully addressed all points raised. Please find below our detailed responses. The original text of the review is presented in **black,** while our responses are presented in **blue**.

Reviewer#1' comments:

Liu et al present a study of ocean alkalinity addition (OAE) in the North Sea using a regional ocean model. They vary the location of OAE deployment and explore the impacts on alkalinity transport, ocean carbon uptake and long-term carbon storage. This was an enjoyable read and is a really nice addition to the growing OAE literature that provides a number of interesting and often counterintuitive findings which are robustly explored. These findings include:

1. The North Sea is a relatively efficient OAE deployment region despite its shallow depth and short flushing time.

2. Small differences in the location of North Sea OAE deployment can lead to considerable differences in alkalinity dispersion, ocean carbon uptake enhancement and long-term carbon storage.

3. Loss of alkalinity to the deep ocean does not necessarily equate to inefficient CDR as although the total carbon flux enhancement is reduced, a greater proportion of this carbon may be transported below the permanent pycnocline.

The manuscript is clear and well-written and I recommend publication subject to a few minor corrections. My main suggestion is that the authors provide a little additional detail on the validation of simulated carbonate chemistry (even though this is published in greater detail elsewhere). Alongside this, it would be good to provide some detail on the maximum attainable CDR efficiency possible in North Sea waters based on mean present-day carbonate chemistry conditions.

Minor comments

1. L90 "efficiently transported" I recommend phrasing this differently.

Answer: Thank you for the kind suggestion. Indeed, "efficiently transported" might be unclear or overly broad. To improve the clarity, we have revised the sentence as follows:

"An optimal location would *facilitate the movement of seawater with elevated alkalinity, ensuring adequate atmospheric contact before it is transported into the ocean's interior,* thereby maximizing OAE efficiency while minimizing potential impacts on the local carbonate system and ecology."

The change has been made in the main text accordingly. See **lines 90-91.**

2. L101 Deployment areas appear stippled not "hatched".

Answer: Thank you for pointing out this mistake. 'hatched' has been changed to 'stippled'. See **line 102** in the main text.

3. L161. I see that carbonate chemistry evaluation is published elsewhere. Nonetheless I think some comment on the ability of the model to simulate DIC/TA/pCO2 in the North Sea is required here.

Answer: Thanks for the reviewer's comments. Indeed, some comments on the ability of the model to simulate the key features of the North Sea carbonate chemistry is necessary to demonstrate whether the model is capable of being used in OAE studies. We therefore added a short summary of the model performance on simulating DIC/TA/pCO2/air-sea CO_2 flux in the main text. Please see **lines 195-202.**

It reads as: *A comprehensive validation of the model's physical dynamics, biological processes (e.g., primary production), and carbonate chemistry has been conducted by Kossack et al. (2023, 2024). The modelled DIC and TA exhibit quantitative agreement with observational data, although both simulated variables exhibit a slight overestimation. The model effectively captures the seasonality and spatial variability of surface pCO_2 and air-sea CO_2 fluxes on the NWES, despite some discrepancies between simulations and observations due to inherent model limitations. Nevertheless, the SCHISM-ECOSMO-CO_2 model system successfully reproduces key features of the marine carbon cycle on the NWES, both qualitatively and with reasonable quantitative skill (Kossack et al., 2024). Therefore, the model serves as a reliable tool for OAE studies.*

4. L176. Can you clarify here whether there are sediment fluxes of alk and DIC and whether these fluxes are influenced by benthic carbonate chemistry and OAE?

Answer: Yes, our model includes sediment fluxes of alkalinity and DIC arising from sedimentary processes such as remineralization, nitrification, and denitrification. However, benthic carbonate chemistry is not explicitly represented in the current model. Those sediment fluxes are thus not directly influenced by benthic carbonate chemistry or OAE.

There are indeed studies suggesting that OAE could influence the natural production of alkalinity, particularly through sedimentary processes. However, these feedback mechanisms remain poorly understood, as only a few studies have explored this topic. At present, incorporating such processes into the model remains challenging due to the limited knowledge. Currently, we are actively working on this aspect by integrating laboratory and field findings into our model developments. This work is still undergoing and is not presented in the current manuscript.

To improve clarity, we have added a relevant description in the section of 2.1 model description in the main text, see **lines 142-143 and lines 149-151.**

5. L301. Typo "of the" tracer

Answer: Thank you very much for pointing this out. We have corrected the typo, ensuring that the phrase now correctly reads 'of the tracer' in the revised manuscript, see **line 307.**

6. L318-340 Some context on the maximum attainable uptake efficiency in the North Sea would be useful here. Is this around 0.85 given the mean simulated surface ocean DIC and alkalinity fields?

Answer: Given the mean simulated surface ocean DIC and alkalinity, we estimate the maximum attainable CO_2 uptake efficiency in the North Sea is around 0.86. The map is provided below and added to the supplement as **Fig. S6.**

A comparison of the model results with the maximum attainable CO_2 uptake efficiency as well as the related discussion is added to the main text, reading as '*However, this value remains below the maximum*
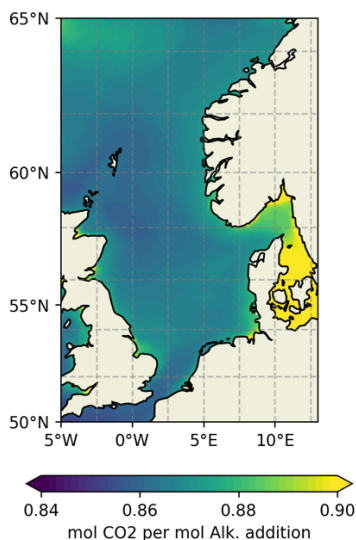
Figure S6. Map of the maximum attainable CO2 uptake efficiency in the North Sea determined by the mean simulated surface ocean DIC and alkalinity fields.

7. Figure 9. It's difficult to distinguish between the thick and thin lines in panel a. I suggest using another line type (e.g. dotted or a different dash type).

Answer: We appreciate the reviewer's recommendation. Using a different line type indeed improves the clarity of the figure. In response, we have changed the thin solid lines in Figure 9a to dashed lines, making them more distinguishable from the thick solid lines. This adjustment enhances readability and makes it easier for readers to differentiate between the two types of DIC inventory. **Figure 9** is displayed below and has been updated in the manuscript, with **the caption of the figure** being changed accordingly.
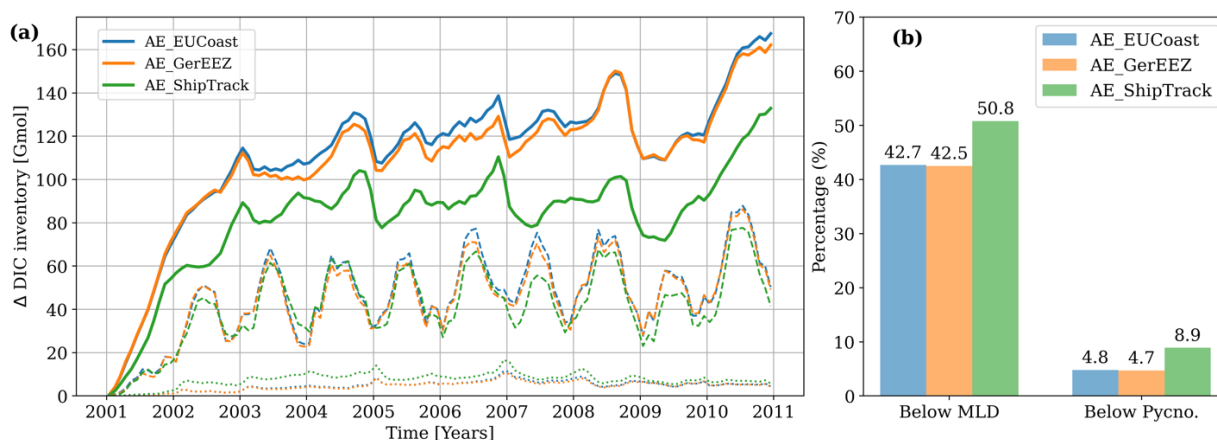
Figure 9: (a) The excess DIC inventory across the entire model domain, integrated over the entire water column (solid lines), below the upper mixed layer (dashed lines) and below the permanent pycnocline (***dotted lines***). (b) the percentage of the total excess DIC inventory below the mixed layer depth (Below MLD) and below the permanent pycnocline (Below Pycno.). The calculation is based on the model results of 2006-2010.

8. L386. Recommend not using "captures"

Answer: Thanks very much for the suggestion. We have revised the sentence and replaced 'captures' with 'represents', which more precisely describes the role of the model in representing the pycnocline depth. The revised sentence reads: *"In this region, our simulation **represents** a permanent pycnocline depth around 200 meters."* See **line 396.**

9. L389-390. Are these values still increasing at the end of the simulations? Does this explain why the deep DIC inventory plateaus within the model domain?

Answer: No, the total excess DIC export below the permanent pycnocline stabilizes from 2003, after which there is no significant increasing trend but only interannual variability. This is evidenced by the yearly export illustrated in Fig. S7, which we have added to the supplement. The information is added in the main text as well, see **lines 398-399.**

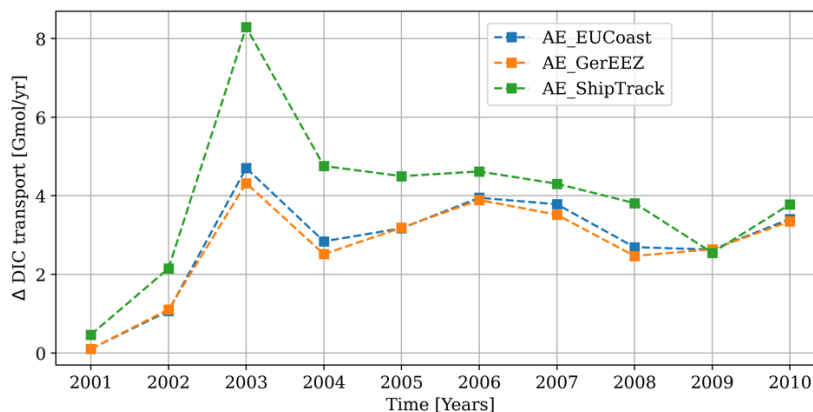This explains why the deep DIC inventory plateaus within the model domain.

Figure S7. The yearly total excess DIC export below the permanent pycnocline across the Norwegian Trench for the three scenarios.

10. L454-456. Can the authors explain this finding here as they do in the supplementary material? How do they know full equilibration has occurred? I recommend phasing this as CO2 system equilibration not alkalinity equilibration.

Answer: Yes, we added a more detailed explanation about how the scenarios are designed and what the results imply to the main text as follows (see **lines 463-470**): '*Injection frequency is another key factor, as demonstrated by three one-time alkalinity addition experiments. In these experiments, a fixed amount of alkalinity is added to the three selected locations only during the first month of a four-year simulation (Supplement S2 and Fig. S2). The extended simulation allows the seawater CO_2 system to gradually re-equilibrate with the atmosphere after alkalinity injection. This is evidenced by the CO_2 uptake efficiency plateauing at 0.57–0.76 mol CO_2 per mol of alkalinity after one year's simulation, indicating that no additional CO_2 uptake occurs beyond this timeframe. However, the CO_2 uptake efficiency is lower than in*

scenarios with continuous alkalinity additions. A possible explanation is that in the one-time addition scenarios, alkalinity is introduced during winter, when strong flushing reduces residence time and thus leads to less effective equilibration.'

As mentioned above, the CO_2 uptake efficiency stabilizes after one year of simulation, indicating that no further CO_2 uptake occurs beyond this point. This suggests that full equilibration has been achieved.

Thanks for correcting the expression regarding the CO2 system equilibration. Indeed, alkalinity itself does not equilibrate but rather the seawater (or its CO2 system) that interacts with the atmosphere. We have thoroughly checked the entire manuscript and the supplementary materials to ensure all related expressions have been corrected accordingly, see **lines 267,342-343** in the manuscript and **Supplement 2.**

In addition, we found that the buffer capacity in the North Sea is relatively homogeneous (Fig. S6) and therefore does not explain the low CO_2 uptake efficiency observed in the one-time alkalinity addition experiments. As a result, we have removed the sentence from the supplement: 'Another possibility is that with continuous alkalinity supply, the added alkalinity disperses over a larger area, where the water has a lower buffer capacity.' This deletion does not impact the overall content of the manuscript.