REVIEWER 2:

Summary

The manuscript presents a coastal OAE study in which ALK is added to European coastal grid cells of a medium resolution model. The model is simulated for two emissions scenarios, and analysis focuses on the seasonal effects on the carbonate system. The main results include that OAE enhances uptake in the winter over the summer and that the North Sea represents a good region for future OAE deployment.

Assessment

Overall, I found the manuscript generally well-written but with a number of omissions that reduce its value to the literature. My major comments are as follows:

1. The manuscript focuses on a regional sea but **does not validate the performance of the model at this scale.** This is particularly important in the case of the coastline release areas, as the manuscript essentially takes the results from these areas at face value, despite potential issues around poor representation due to resolution, missing physical / biogeochemical processes or riverine inputs.
2. Presentation of the manuscript's results is slender with a **focus purely on the 2090s** with nothing about how the system then came into being, arguably absent spatial plots on changes in seasonality (e.g. maps of seasonal amplitudes), and confusing in-line use of important **metrics** that would be much more useful if **organised into a table**.
3. Although the work is performed globally, the analysis is strictly a regional affair, and it omits consideration of factors such as OAE performance (e.g. extra mol DIC per added eq ALK), where this happens, loss (to the region) of added ALK and extra DIC, and impacts on the model's natural ecosystem (NPP, $CaCO_3$ production, etc.). These affect the **assessment of requirements for MRV and the cost-benefit ratio**.
4. Main conclusions, such as the suitability of the North Sea for OAE, fail to note the dependence of this on the specific OAE approach here (i.e. addition of dissolved ALK), while the manuscript's caveats overlook model limitations in shelf seas areas, and potentially **oversell this region**.

My assessment is Major Revisions to address these gaps so that readers can both better understand this study and better assess its findings in the context of the wider OAE literature.

General response by co-authors

We sincerely thank the reviewer for their time and effort dedicated to this revision, which was extremely helpful in reshaping the focus of our analysis, improving the clarity of the text and re-interpreting the results in the context of the wider OAE literature. Each comment highlighted by the reviewer has been addressed extensively, following the vast majority of the suggestions / changes, as we found them essential to improve the manuscript. Multiple aspects have been largely developed, including:

- **Model validation of the region:** the point that was raised on regional model validation prompted us to include a new section that compares our model's data against GLODAPv2 datasets, using surface alkalinity, surface DIC, SST, and surface salinity (ocean pCO2 observations were too sparse for this region). While the model is overall in agreement with the observations, we acknowledge that some biases exist and we include this aspect as a limitation in our manuscript, emphasising on important results that highlight its value.
- **Spatial extent of the analysis and North Sea section:** thanks to the reviewer's comments, we acknowledged that the North Sea analysis was not adding much value to the interpretation of the manuscript's results, and we decided to replace it with a consistent comparison between the 'European region' and the 'coastline region', an approach that is maintained throughout the study, both in the structure and in terminology. Thanks to this new section, a description of the background state of the two regions under investigation helps the reader understand changes in our OAE simulations.
- **Addition of figures relevant to the understanding of described processes:** multiple figures have been added to the text to cover aspects such as vertical distribution of excess alkalinity, seasonal OAE efficiency metrics, carbon sequestration within and outside the region of alkalinity addition. These new figures help the reader visualise the changes described in the text regarding the seasonal cycle reversal of surface alkalinity, or the carbon sequestration potential difference between regions as well as scenarios. In addition, a table was built to summarise the main quantities described in the text to guide the reader throughout the text.
- **Discussion of the results and of their implications for MRV:** the interpretation of the results in the context of regional carbonate dynamics has been substantially improved to identify how and why OAE changes on one variable affect the other(s). More emphasis has been placed on the connection between surface ocean pCO2 change and changes to the air-sea CO2 flux, stressing the outcome that the European region turns from a system of net summer outgassing to a year-round carbon sink. The role of OAE in reversing the seasonal cycle of alkalinity has been highlighted in the context of detectability and biological risks, stressing the need for tailored MRV frameworks. The relevance of the emission scenario is also explained more in depth, drawing from previous studies that showed similar results from the ones described in this manuscript.
- **Relevance of this study and its place within the wider OAE literature:** The value of this study has been highlighted in the introduction, pointing to previous studies that only touched upon OAE-driven seasonal changes, though with a different setting from our implementation. The Discussion section now includes paragraphs of comparison between our study and previous research, which helps understand the new insights offered in our manuscript and its role in the wider OAE literature, highlighting that much of the results remain dependent on features like modes, rates, locations of alkalinity addition.

We believe that these revisions have strengthened our analysis and enhanced its scientific rigor, and we once again thank the reviewer for their insightful feedback.

LEGEND for the review:

- REVIEWER'S COMMENT
- RESPONSE BY CORRESPONDING AUTHOR
- NEW / UPDATED TEXT

Specific comments

- Ln. 29-30: tweak to "... to lower its sea surface partial pressure" and "durable ocean storage" for clarity

Thank you for your comment. We changed the text to:

Lines 28-30: 'One potentially scalable method to remove CO_2 from the air is ocean alkalinity enhancement (OAE), which works to lower its sea surface partial pressure (pCO_2) and accelerate CO_2 sequestration and durable ocean storage.'

- Ln. 34-35: "OAE enhances CO2 uptake in winter ... reduces ocean pCO2 in summer" is potentially confusing as lowering pCO2 in the summer might also be read as enhancing the conditions for CO2 uptake

Thank you for pointing this out. After in-depth restructuring of the manuscript and analysis, here is a revised text of the key findings in the abstract:

Lines 30-36: 'This study explores how OAE might affect the seasonal carbon cycle, which plays a key role in the ocean's annual CO_2 uptake. By analysing earth system model simulations of OAE implemented continuously at the European coastline under low and high emissions, it was found that: a) due to surface alkalinity retention, OAE reduces ocean pCO_2 most strongly in summer, turning the region into a year-round carbon sink; b) the highest air-sea CO_2 flux change takes place in winter; c) the ocean's carbon sink is increased more strongly in SSP3-7.0 than in SSP1-2.6 due to a lower buffering capacity.'

Additionally, this is how we reformulated the connection between lowest ocean pCO2 and highest CO2 uptake:

In the results:

Lines 369-378: 'Importantly, in the OAE runs in both the European region and coastline region, excess surface alkalinity accumulates during summer, prompting a stronger reduction of surface ocean pCO_2 which, due to fast air-sea exchange, turns net summer outgassing into a year-round carbon sink. OAE drives the largest excess CO_2 uptake in winter, as the seasonal air-sea CO_2 flux remains in phase with the MLD cycle, regardless of seasonal variations in alkalinity (Nagwekar et al., 2024). In the coastline region, a shallow bathymetry favours year-round mixing and stronger CO_2 uptake than in the European region. As for the role of the emission scenario, SSP3-7.0 drives an even stronger carbon uptake in winter, while maintaining summer values similar to SSP1-2.6, which further increases the CO_2 flux seasonal amplitude. As it will be described later, this behaviour is due to the lowering buffering capacity of the ocean under higher emissions.'

In the discussion:

Lines 427-435: 'Secondly, continuous OAE deployment induces surface ocean pCO_2 reduction at all seasons, but most strongly when the associated seasonal driver reduces the ocean CO_2 sink potential, which is summer in the temperature-driven European region. In the coastline region, this phase shift of surface alkalinity drives the reversal of ocean pCO_2 seasonality, though the CO_2 uptake, and the OAE-driven uptake increase, remains largest during winter, as CO_2 seasonality follows MLD seasonality.

This leads to two main conclusions: one the one hand, increasing ocean disequilibrium with the overlying atmosphere turns summer net outgassing into net uptake year-round due to fast air-sea exchange; on the other hand, as the system moves to colder months, SST decreases, the MLD deepens and CO_2 dissolution is favoured.'

- Ln. 36-37: grammar – switch "a region with a shallow bathymetry" to "regions with shallow bathymetry"; also, perhaps change "fast" to "faster" on Ln. 38

Thank you for the suggestion. This sentence has been removed due to changes in the analysis following some of your and other reviewers' comments. This will be argued more extensively in later responses.

- Ln. 130: correct the Chien et al. (2022) citation to the published paper and not the Discussions manuscript:

Chien, C.-T., Durgadoo, J. V., Ehlert, D., Frenger, I., Keller, D. P., Koeve, W., Kriest, I., Landolfi, A., Patara, L., Wahl, S., and Oschlies, A.: FOCI-MOPS v1 – integration of marine biogeochemistry within the Flexible Ocean and Climate Infrastructure version 1 (FOCI 1) Earth system model, Geosci. Model Dev., 15, 5987–6024, https://doi.org/10.5194/gmd-15-5987-2022, 2022.

Thank you for pointing this out. We updated the reference:

Lines 591-593: 'Chien, C.-T., Durgadoo, J. V., Ehlert, D., Frenger, I., Keller, D. P., Koeve, W., Kriest, I., Landolfi, A., Patara, L., Wahl, S., and Oschlies, A.: FOCI-MOPS v1 – integration of marine biogeochemistry within the Flexible Ocean and Climate Infrastructure version 1 (FOCI 1) Earth system model, Geosci. Model Dev., 15, 5987–6024, https://doi.org/10.5194/gmd-15-5987-2022, 2022.'

- Ln. 130: see my comment below too, but the validation here is at a global scale while your analysis is at the regional scale; so it does not especially help with ground-truthing your North Sea study

Many thanks for raising this point. We fully acknowledge that a regional validation of our model was missing. We added a dedicated section to address the validation against GLODAP datasets for alkalinity, DIC, surface salinity and SST, and we also accept this as a limitation in a new Limitations section that describes the caveats of using FOCI-MOPS in this study. See answers to your specific comments below.

- Ln. 141: O'Neill et al. (2016) is the protocol for the ScenarioMIP simulations, not for general guidance on spin-up or Historical simulations; you should also add Eyring et al. (2016)

Thank you for the comment. We added the reference by Eyring et al. (2016)

Lines 133-134: 'All spin-ups, control simulations and forcings adhered to the CMIP6 protocol (O'Neill et al., 2016, Eyring et al., 2016).'

Lines 595-597: 'Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization, *Geoscientific Model Development*, 9, 1937–1958, 2016.'

- Ln. 147: is the addition region really 50km or is it the width of a single grid coastal cell?; and if the latter, you may find that the width of the addition area changes with latitude because of grid distortion; I assume that, where the amounts of alkalinity added are quantified later that proper account is taken of varying grid cell areas

Thank you for pointing this out. Indeed, the alkalinity is applied to a single grid coastal cell which, as you suggested, implies grid distortion with latitude. We applied the grid cell area weighting to the analysis but we didn't make this clear enough in the text. We explained this process in depth in the text:

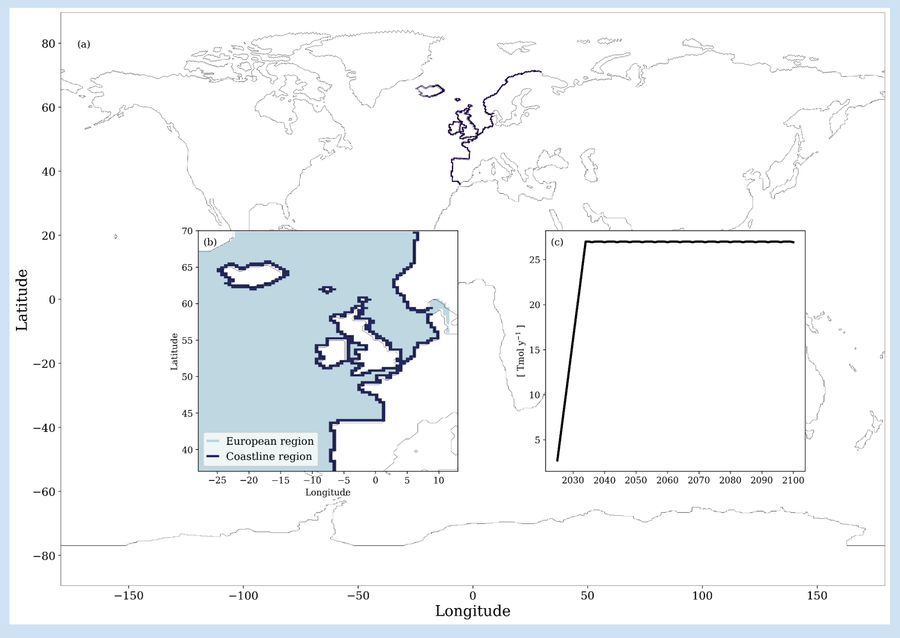
Lines 143-148: 'In the OAE runs, alkalinity is introduced continuously and evenly into the surface layer of the coastal grid cells outlined by the coloured line in Figure 1a, spanning a vertical depth of roughly 3 meters. As the model runs with a horizontal resolution of 1/2°, this corresponds to approximately 55 km in the meridional direction and 45 km zonally in southern Europe, decreasing to about 30 km in northern Europe.'

You can find updated Figure 1 for reference in the answer below this one.

- Figure 1: Could this plot be improved to either show the full land mask (it's currently cut off on the east and west margins by unexplained curves) or by cropping the domain shown to remove these missing portions; more generally, you're showing a lot of domains that aren't part of your later analysis; you might also want to note in the caption that the x and y axes are not longitude and latitude – instead this plot seems to use the model's i-j grid

Thank you for the comments. We agree that Figure 1 could give a much clearer representation of the geographical extent of our analysis. Please, see below the updated Figure 1, with caption.

Lines 139-143: 'Figure 1: (a) shows the spatial distribution of alkalinity addition along the European coastline (excluding the Mediterranean and the Baltic seas); (b) zooms into the regional domain, highlighting the 'European region' in lightblue and the 'coastline region' in darkblue; (c) shows the time series of coastline alkalinity addition in units of Tmol y-1 from 2025 to 2100.'



- Figure 1: thank you for clearly including the British Isles as part of European waters – it helps us Remainers keep the flames of hope alive! ;-)

:-)

- Ln. 152-157: you don't say at what depth the alkalinity is added; and, while you make what you do clear, you might want to note how this compares to other studies – either here or in the discussion; it sounds a little like CDRMIP and different from the likes of Palmieri & Yool's 2024 study where ALK was added on the seafloor

Thank you for pointing this out. The new text reads:

Lines 144-146: 'In the OAE runs, alkalinity is introduced continuously and evenly into the surface layer of the coastal grid cells outlined by the coloured line in Figure 1a, spanning a vertical depth of roughly 3 meters'.

Additionally, we touched upon how our method compares to other studies in the Discussion section:

Lines 450-463: 'In order to highlight the relevance of these findings, we place our results in the context of previous research that has investigated OAE-induced seasonal changes under varying background emissions. Starting from the mode of OAE deployment, which have a strong impact on carbonate changes, most studies have investigated surface alkalinity addition: at the coastline (Feng et al., 2017, Moustakis et al., 2025), throughout the global ocean (González and Ilyina, 2016, González et al., 2018), or at specific locations to investigate regional dynamics (Ilyina et. al., 2013, Nagwekar et al., 2024). Similarly, our simulations simulate the addition of a fast-reacting material, assuming fast dissolution at the ocean surface. Palmiéri and Yool (2024) simulated the addition of about 29 Tmol -1 alkalinity equivalent to the global shelves at the seafloor, finding that the depth of application has a minor impact on carbon sequestration potential. Regarding the rate of alkalinity addition, we apply continuous OAE deployment, which implies a constant replenishment of surface alkalinity to drive air-sea disequilibrium. This approach differs from studies that investigated pulsed addition, which is likely to become a more realistic strategy (He and Tyka, 2023, Zhou et al. 2024). '

- Ln. 157: as you model alkalinity prognostically, you might want to express the quantity of alkalinity added in equivalents as well as carbon units

Thank you for the comment. We changed the text to:

Lines 148-151: 'OAE is applied from 2025 to 2100, with linear increase over the first decade of addition (2025-2034), until the equivalent of 1 Gt y-1 of fast-reacting calcium hydroxide ($Ca(OH)_2$) is reached. From 2035, this amount, which equals 27 Tmol y-1 of alkalinity (or 44 mol m-2 y-1), is held constant until the end of the century (Figure 1c).'

- Ln. 165: the explanation for slicing to a European region feels a bit vague; you will need to be clear later on what the effect of cropping to this region is; in the context of the cited study, Palmieri & Yool (2024), you might be adding ALK in this region but the DIC is only absorbed outside this region

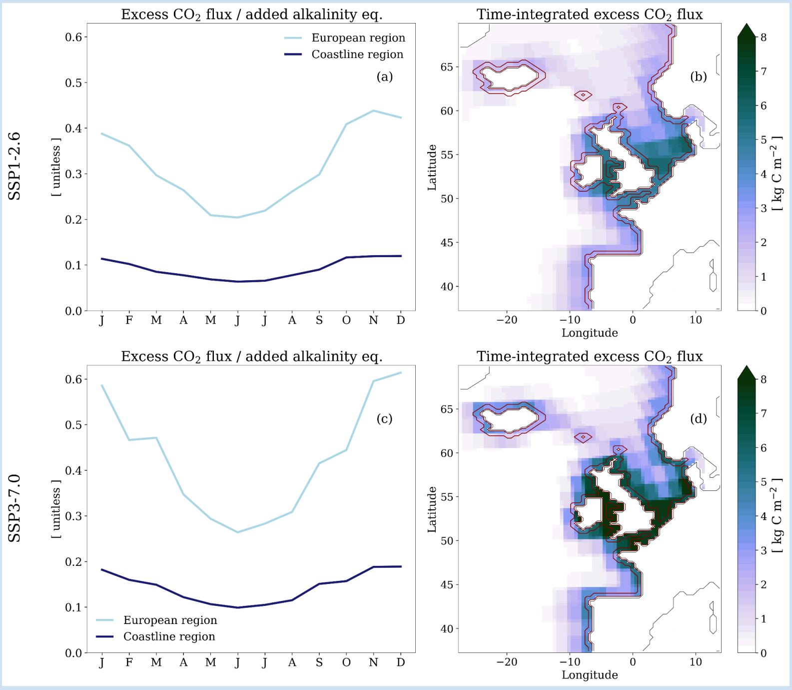
Thank you for raising this point. The slicing that we applied felt like a good compromise between keeping most of the region in the analysis and not diluting the seasonal signal too much. In addition, we were more interested in investigating dynamical changes in the proximity of the OAE location. Following your suggestion, we clearly stated how keeping the analysis confined to the defined area leaves out part of the carbon that may be taken up elsewhere. We made this especially clear by adding a figure on OAE efficiency within and outside the region of alkalinity addition. Please see the updated text below:

In the Methods section:

Lines 188-194: 'Our analysis focuses on the comparison between two regions: the 'coastline region', which corresponds to the shelf grid cells highlighted by the dark blue line in Figure 1b, offers details on regime shifts that take place at the epicenter of the domain; the 'European region', which corresponds to the coloured ocean region in Figure 1b, measures how the European system as a whole might respond to OAE. As Palmiéri & Yool (2024) showed that half of the OAE-driven carbon uptake can happen far from the injection region, this approach helps maintain some focus beyond the coastline of alkalinity addition without risking to dilute the OAE signal.'

In the Results section:

Lines 388-392: ‘Figure 7: The seasonal carbon sequestration potential defined as the ratio between the change in CO_2 flux and the added alkalinity carbon equivalent averaged over 2090-2099 (a, c), and the 2090-2099 time-integrated excess CO_2 flux (OAE minus control run) (b, d). The red contour line represents the region of alkalinity addition.’



Lines 403-411: ‘When comparing the European region and the coastline region, Figure 7 (a, c) shows that the carbon sequestration potential of OAE is much higher in the former than it is in the latter. This behaviour is explained by the fact that excess carbon uptake is not confined to the place of alkalinity addition, as highlighted in panels (b) and (d). While most additional air-sea CO_2 flux takes place near the coastal shelves, with little increase over the open ocean, this excess downward flux extends beyond the region of alkalinity addition (Palmiéri & Yool, 2024). Furthermore, compared to SSP1-2.6, high emissions strengthen the ocean carbon sink for the same amount of alkalinity addition and within the same spatial extent due to a lower buffer factor in a high-CO_2 world (Nagwekar et al., 2024)’

In the new Limitations section:

Lines 532-536: ‘Lastly, this manuscript does not address alkalised water that is subducted before air-sea equilibration is complete, and uncertainty remains on whether that water could resurface to drive CO_2 uptake elsewhere. In Palmiéri & Yool (2024), for example, it was estimated that about 50% of CO_2 uptake favoured by global coastal OAE happened remotely from the alkalinity injection sites, as it is partially reflected in Figure 7.’

- Ln. 174: you say that “ocean pCO2 and the CO2 flux are written out only for the surface”, but they’re actually only CALCULATED for the surface too. They’re air-sea interface terms not 3D ocean terms.

Thank you for your comment. Good point that CO2 flux and CO2 fugacity can be ‘written out’ only for the surface. We rephrased this sentence as follows:

Lines 122-127: ‘In FOCI-MOPS, the ocean biogeochemical state, including DIC and alkalinity, is calculated on the NEMO-MOPS ocean grid, while the coupled air–sea CO_2 flux information is transferred through OASIS3-MCT from the ORCA05 ocean grid to the coarser atmospheric T63 grid. The CO_2 flux is then calculated in the atmospheric component of FOCI and subsequently remapped conservatively back to the ocean grid, where it is applied as the surface DIC boundary flux (Matthes et al., 2020).’

- Ln. 175: “alkalinity is calculated” isn’t quite right – it’s more *represented*

Thank you for the comment. We changed the phrasing of the sentence:

Lines 197-198: ‘While ocean pCO_2 and the CO_2 flux are calculated only for the surface, alkalinity is represented over all 46 FOCI ocean layers.’

- Ln. 182: some validation of the model seems in order; as already noted above, the Chien et al. (2022) paper deals with large scale validation and is not focused in the region here; and certainly not focused in the thin coastal strip that you use for parts of your analysis; given the importance of temperature SST seems a good start; ditto pCO2 (if observational resolution permits it); I’m not sure how good DIC and ALK datasets are in this specific region, but if they’re available, definitely validate against these

Many thanks for raising the point about validation. We added a dedicated section to explore FOCI-MOPS fields against GLODAP fields for the region of alkalinity addition. While we were able to find data for ALK, DIC, salinity and SST, pCO2 observations were either absent or too sparse in this region. Please see below the new section:

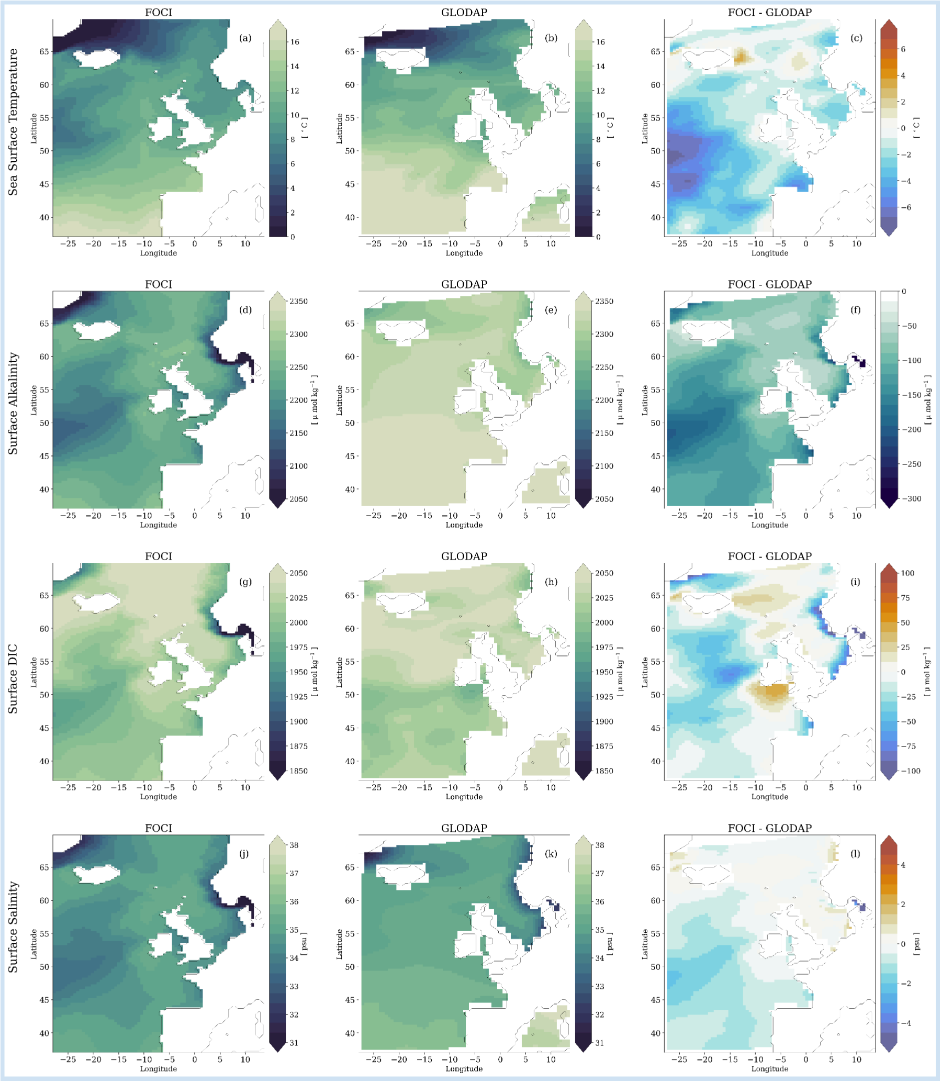
Lines 157-186:

‘2.3 Regional model validation

At the global scale, both alkalinity and DIC in FOCI-MOPS agree well with observations, while still showing a positive bias at the surface and a negative bias below 3000 meters of depth. However, model fidelity lowers towards the coasts, which are less resolved with a 1/2° resolution. While this is a common bias in global models that has been highlighted in Chien et al. (2022), here we validate FOCI-MOPS in the region where alkalinity is added in our simulations using GLODAPv2 observations (Lauvset et al., 2016, Key et al., 2015) (Figure 2).

For coastal sea surface temperature, FOCI-MOPS agrees well with GLODAP observations, with slightly lower values near the continental shelf. Towards the North Atlantic, FOCI-MOPS shows a prominent cold bias (Figure 2c), which was already observed in previous model validation studies (Chien et al., 2022, Matthes et al., 2020). Such a feature is likely due to the lack of mesoscale dynamics, which affects the Gulf Stream path compared to an eddy-resolving ocean model (Matthes et al., 2020, Ojha et al., 2026, preprint).

Figure 2: Comparison of FOCI-MOPS mean fields (1972–2013) and 2021 GLODAP observations, along with their differences, for sea surface temperature (a, b, c), surface alkalinity (d, e, f), surface DIC (g, h, i), and surface salinity (j, k, l).



Generally, surface alkalinity has a minor negative bias compared to the GLODAP dataset, which increases off the coasts of Norway and towards the open ocean (Figure 2f). DIC fields in FOCI-MOPS generally agree well with observations, while showing a negative bias in the North Atlantic and a positive bias south of the Irish coasts. By the Norwegian shelf and in the southern North Sea, the strong negative bias may reflect on the background carbonate system under OAE application. A bias toward colder, lower-alkalinity conditions also reflects the fresh bias shown in Figure 2l, which is stronger in the North Atlantic but absent towards the coast.

Overall, FOCI-MOPS shows good agreement with observations but we recognise that the use of a lower-resolution model may limit the accuracy of absolute values in our simulations. However, we emphasise that our analysis focuses on differences between simulations (i.e. between an OAE run and a control run, or between SSPs), such that systematic errors are expected to affect experiments similarly and therefore largely cancel.'

Additionally, we added this as a limitation to the new Limitations sections:

Lines 517-525: 'Some limitations and future recommendations can be drawn from this study. With regard to FOCI-MOPS, the ESM represents coastal biogeochemical processes less well compared to the open ocean (Chien et al., 2022), which may affect the accuracy of absolute values in the region of alkalinity addition. While model results are mostly in agreement with GLODAPv2 observations, some of the biases may affect the veridicity of output values. Alkalinity in the Baltic Sea, for example, is poorly resolved by FOCI, which could have an impact on the North Sea boundary conditions. However, since our analysis focuses on relative differences between runs, the main conclusions in this manuscript would be less affected by such structural biases, as these are expected to affect both simulations similarly. Furthermore, while freshwater input from rivers is modelled in FOCI, riverine alkalinity is not accounted for. As river runoff has a strong seasonality, implementing such processes would improve the accuracy of future OAE simulations.'

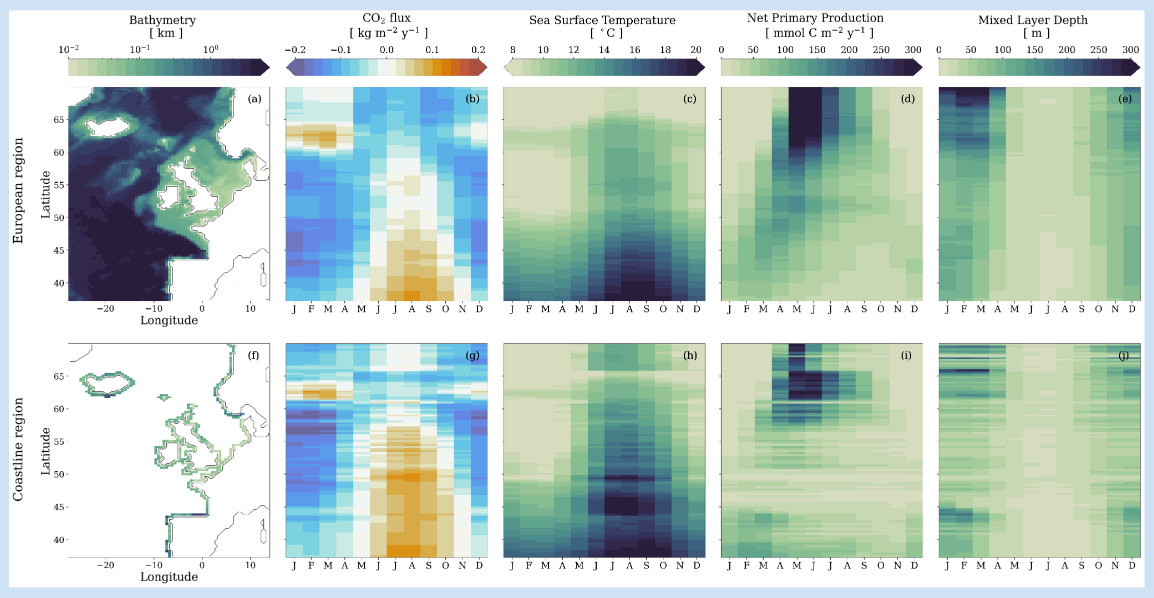
- Ln. 201: you note a North Sea section here – any chance it could be plotted on Figure 1?

Thank you for the comment. As already mentioned above, we realised that the North Sea section does not fit well within the scope of the analysis and we decided to replace it with a section on the seasonality of baseline variables for the two manuscript's domains, namely the European region and the coastline of addition (now highlighted in the updated Figure 1). Hence, we wouldn't need to add the North Sea transect in Figure 1.

- Figure 2: consider using a diverging palette (e.g. classic blue-white-red; or the one used in Figure 4) for the CO2 flux so that zero flux is easier to spot

Thank you for the suggestion. The palette of the air-sea CO2 flux plot has been changed to a diverging palette. Please, see the updated Figure and its associated caption. The order of the plots has also been changed, following your input in a later comment.

Lines 222-226: 'Figure 3: Hovmöller diagram of the zonally averaged European region (top) and coastline region (bottom) for bathymetry (a, f), the CO_2 flux (b, g), sea surface temperature (c, h), MLD-integrated net primary production (d, i), and the mixed layer depth (e, j) over the 2090-2099 mean. In the CO_2 flux plot, negative values indicate ocean uptake.'



- Figure 2: I like that you kept the graininess of the model grid here – much more honest than the interpolated contour plots people use

:-)

- Figure 2: why show surface NPP when, due to mixing, it's surface layer productivity that controls CO2 uptake?

Thank you for your comment. We were slightly confused by what would be best to plot here. Did you mean that it could be more consistent to plot MLD-integrated NPP? If yes, we added the MLD-integrated NPP as highlighted in figure 3 above this comment.

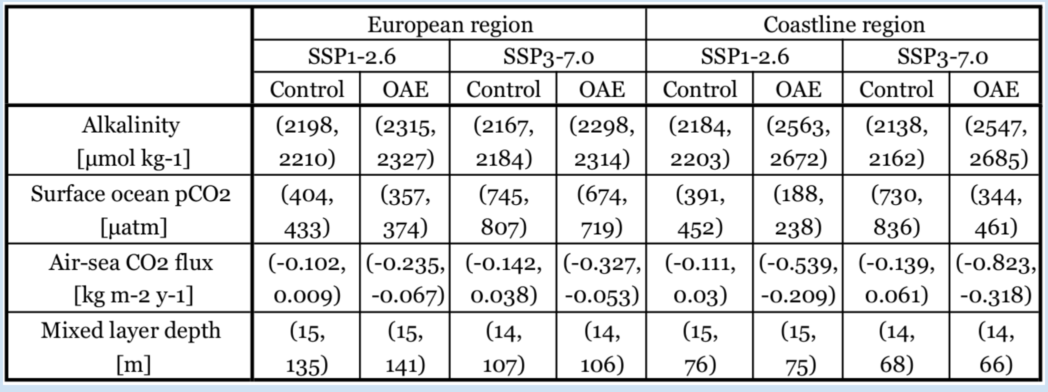
- Figure 2: the ordering of the subplots seems a little arbitrary; by default, I would have gone SST, MLD, NPP, CO2 flux to reflect some sort of process-ordering, but in reading your results, I might have at least put SST and CO2 flux closer together so it's easier to see the common patterns.

Thank you for this comment. We initially intended to show all the drivers and then the outcome (CO2 flux) but we see how your suggestion makes the figure more readable. We put SST close to air-sea CO2 flux to show how temperature has a predominant effect driving air-sea CO2 flux, especially at low latitudes, while NPP comes right before SST to show how that has a much higher impact in driving the flux direction at higher latitudes. To avoid copy-pasting the figure twice, we refer you to the comment just above this one.

- Ln. 217: a general comment I'd make about this section is that there are lots of numbers given in the text and it would be better to consolidate these into a table so that readers can clearly see the major features identified both geographically and between scenario runs.

Many thanks for suggesting this. We built a simple table that summarises average values for the time-period that we focus on. See the caption and the table below.

Lines 252-256: 'Table 1: Annual minimum and annual maximum values averaged over 2090-2099 for alkalinity, surface ocean pCO$_2$, the air-sea CO$_2$ flux, and the mixed layer depth. Results are presented for the European region and the coastline region under both emission scenarios in the control and OAE runs. Note that air-sea CO$_2$ flux negative values indicate uptake by the ocean.'

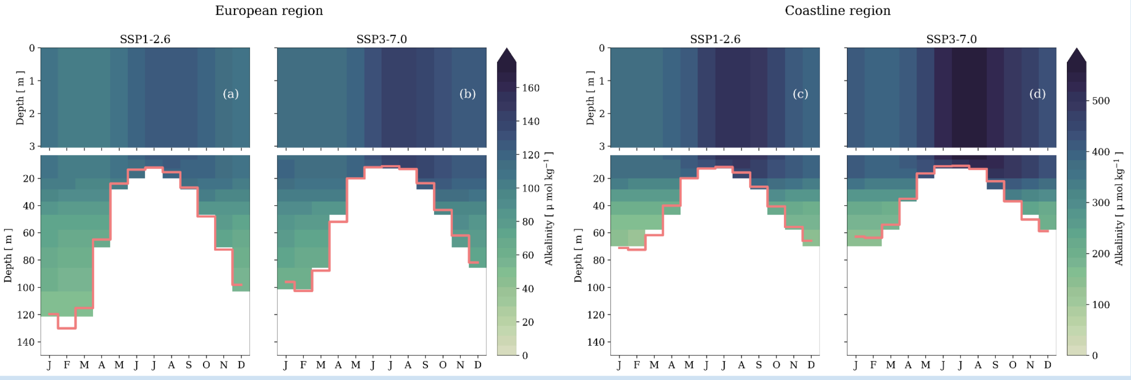


	European region				Coastline region			
	SSP1-2.6		SSP3-7.0		SSP1-2.6		SSP3-7.0	
	Control	OAE	Control	OAE	Control	OAE	Control	OAE
Alkalinity [µmol kg-1]	(2198, 2210)	(2315, 2327)	(2167, 2184)	(2298, 2314)	(2184, 2203)	(2563, 2672)	(2138, 2162)	(2547, 2685)
Surface ocean pCO2 [µatm]	(404, 433)	(357, 374)	(745, 807)	(674, 719)	(391, 452)	(188, 238)	(730, 836)	(344, 461)
Air-sea CO2 flux [kg m-2 y-1]	(-0.102, 0.009)	(-0.235, -0.067)	(-0.142, 0.038)	(-0.327, -0.053)	(-0.111, 0.03)	(-0.539, -0.209)	(-0.139, 0.061)	(-0.823, -0.318)
Mixed layer depth [m]	(15, 135)	(15, 141)	(14, 107)	(14, 106)	(15, 76)	(15, 75)	(14, 68)	(14, 66)

-Ln. 227: vertical processes are invoked here but no results are presented in support of these; perhaps a comparison of Hovmollers of decadally-averaged ALK from the two scenarios (or differences of scenarios from their control twins) would assist?

Thank you for the comment. We fully agreed that a plot showing vertical profiles was missing in support of the description in the text. We added a hovmöller of the change in monthly averages for the last simulation decade (for consistency with the other plots). See below the new figure and its caption:

Lines 294-298: ‘Figure 5: Hovmöller diagram of the zonally averaged alkalinity change (OAE minus control run) as a function of depth over the first model layer (top) and over the mixed layer (bottom). The left panels (a, b) represent the European region and the right panels (c, d) represent the coastline region. The red line corresponds to the mixed layer depth in the control run.’



Lines 281-292: ‘Importantly, under both emission scenarios and in both the European and the coastline regions, alkalinity addition reverses its surface seasonal cycle, with summer values exceeding winter values. This reversed pattern is highlighted in Figure 5, where seasonal alkalinity is shown as a function of depth: while under natural conditions, alkalinity increases with depth and winter mixing helps replenish the upper ocean from deeper layers, thus following MLD seasonality, in the OAE simulations, alkalinity is added at the surface in our simulations, and stronger vertical stratification in summer leads to greater alkalinity retention.

In all OAE runs, while alkalinity mostly accumulates in the first ocean layer (top row), the rest of the mixed layer (bottom row) also reflects an alkalinity increase, which is higher in summer than in winter, leading to the seasonal shift that is observed in Figure 4a, b. Surface alkalinity accumulation is higher in the coastline region, compared to the European region, due to a much higher alkalinity input in the region of addition, and in SSP3-7.0, compared to SSP1-2.6.’

- Figure 3: is the “coastline average” the values in the grid cells indicated in Figure 1?

Thank you for pointing this out. The coastline average indeed is the average along the coastal grid cells. We see how we did not make the description of the region selection fully clear and we changed the text as follows:

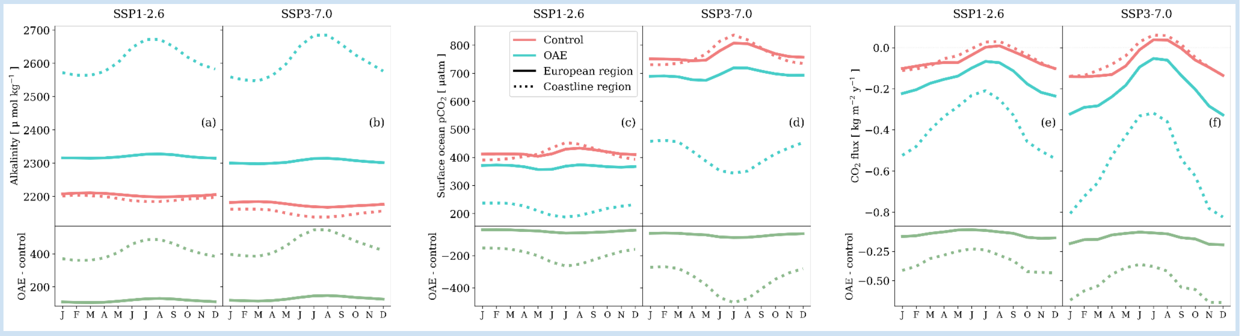
Lines 188-192: ‘Our analysis focuses on the comparison between two regions: the ‘coastline region’, which corresponds to the shelf grid cells highlighted by the dark blue line in Figure 1b, offers details on regime shifts that take place at the epicenter of the domain; the ‘European region’, which corresponds to the coloured ocean region in Figure 1b, measures how the European system as a whole might respond to OAE.’

Additionally, we changed all references and figure labels to ‘European region’ and ‘coastline region’ to avoid confusion and be consistent throughout the manuscript and figures.

- Figure 3: might it be an idea to have the upper and lower plots sharing the same vertical axes so it’s easy to compare them?; it looks like it should generally work

Thank you for suggesting this. Based on your comment, we decided to combine the upper and lower plot to allow for easier comparison. See below the updated caption of Figure 3.

Lines 267-273: 'Figure 4: Seasonal cycle averaged over 2090-2099 for surface alkalinity in SSP1-2.6 (a) and in SSP3-7.0 (b), for surface ocean pCO_2 in SSP1-2.6 (c) and in SSP3-7.0 (d), and for the CO_2 flux in SSP1-2.6 (e) and in SSP3-7.0 (f), with the respective difference depicted by the green line. Continuous lines represent the European region and dashed lines represent the coastline region. In the CO_2 flux plots, negative values indicate ocean uptake'



- Figure 3: looking at this figure's results from the 2090s makes me wonder if there's any value in exploring the same (or similar) quantities across the whole of the simulation period?; perhaps amplitude of seasonality would be good to see given the two scenarios clearly diverge on properties like this

Thank you for raising this suggestion. While we think that plotting the whole simulation period would bring clarity on how the system developed from its initial state, we argue that such an analysis would slightly diverge from the scope of this manuscript, which specifically focuses on the seasonal changes driven by OAE and is less interested in understanding the evolution from initial conditions. Additionally, a paper is being drafted at the moment based on the same simulations which will look further into such dynamics.

- Figure 4: one can imagine that the calculation of CO2 flux on a much coarser grid that's implied here might affect the results; it might be helpful to articulate how exactly the CO2 flux calculation is done – i.e. are ocean properties (SST, DIC) averaged to the atmospheric grid and then pCO2, CO2 flux is calculated, or is ocean pCO2 calculated on the ocean grid averaged to the atmospheric grid and then CO2 flux calculated, or is everything done on the ocean grid, and the CO2 flux is integrated to the lower-resolution atmospheric grid?; most models I know do the latter; in any case, and explanation – probably in the methods – is needed to explain what's done and what the implications of this are

Thank you for this comment. We agree that the grid on which the air–sea CO_2 flux is calculated is particularly relevant for a regional analysis of European coastal waters, where strong gradients in SST, sea ice, winds, biological activity, alkalinity, and surface ocean pCO_2 can occur over relatively short distances.

The calculation of the CO2 flux follows the existing FOCI coupling architecture, where surface fluxes are calculated in the atmospheric component. A practical advantage of this approach is that the CO_2 flux is computed consistently with the rapidly varying atmospheric boundary-layer state, including wind speed and atmospheric CO_2, which directly control air–sea gas exchange. However, for the regional coastal analysis presented here, this also implies that oceanic surface CO_2 information is first remapped to the coarser atmospheric grid before the flux is calculated, leading to smoothing of sub-atmospheric-grid coastal gradients. We have now added this description to the Methods section and we also discussed the implication of this coupling strategy.

Lines 122-129: ‘In FOCI-MOPS, the ocean biogeochemical state, including DIC and alkalinity, is calculated on the NEMO-MOPS ocean grid, while the coupled air–sea CO_2 flux information is transferred through OASIS3-MCT from the ORCA05 ocean grid to the coarser atmospheric T63 grid. The CO_2 flux is then calculated in the atmospheric component of FOCI and subsequently remapped conservatively back to the ocean grid, where it is applied as the surface DIC boundary flux (Matthes et al., 2020). Since the CO_2 flux calculation is performed on the coarser atmospheric grid, sub-atmospheric-grid oceanic gradients are smoothed before the flux is calculated.’

- Figure 4: is it informative to plot the point-by-point correlation between delta-ALK and delta-pCO2?; this might help clarify the text around different regions responding differently; and use of different coloured points for the two scenarios could allow them to be plotted on the same axes, and this might pull-out the divergence in the two runs seen in the region of the North Sea east of Scotland and west of Denmark

Thank you for this suggestion. We tried to plot what you suggested (by latitudinal bands) but the result was not informative to conclude any consistent pattern due to the amount of data points. In addition, we concluded that higher / lower amplitude may be induced by how strong the summer pCO2 reduction is (i.e. if the summer reduction of ocean pCO2 is very strong, the seasonal cycle is amplified as well as reversed). We explain this in the text below:

Lines 343-349: ‘Such a strong latitudinal and scenario divergence in the seasonal amplitude of surface ocean pCO_2 may be due to different factors: north of 60° N, where the air-sea CO_2 flux is driven by its biological component (Figure 3d), the seasonal cycle could respond differently from the European region and the coastline region averages, where the CO_2 flux is controlled by SST; at lower latitudes in SSP3-7.0, the increasing seasonal cycle of surface ocean pCO_2, which is spatially confined to the coastal shelves, is probably driven by an even stronger OAE-induced pCO_2 decline, which increases its seasonal amplitude as it is highlighted in Figure 3d for the coastline region.’

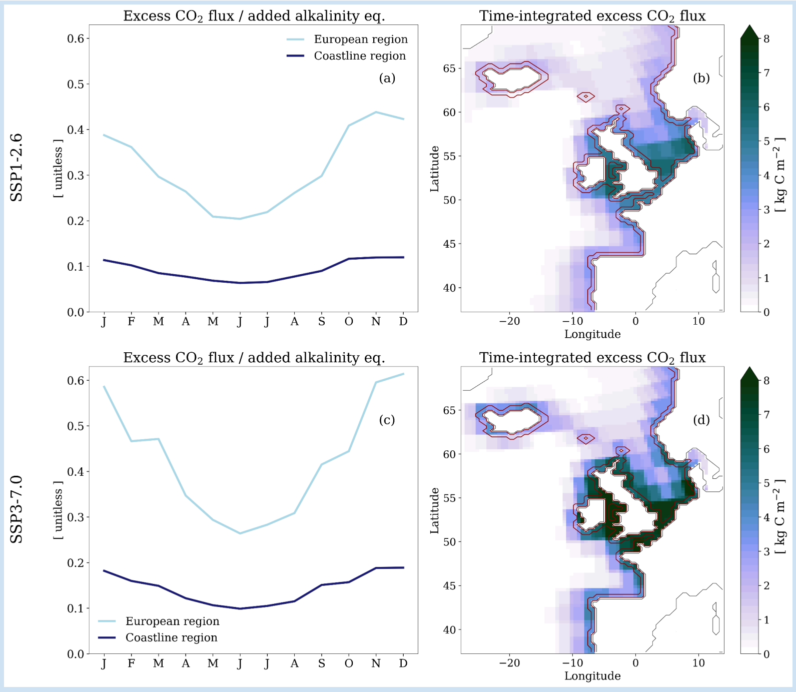
- Ln. 293-303: per my earlier remarks, consolidating the figures here in a table that perhaps separated the numbers between regions as well as between the scenarios might help the manuscript articulate the important differences better; text descriptions are less clear than even a simple table

Thank you for remarking this. We fully agree that summarising the main values in a table would add clarity to the text. We addressed this issue in an earlier response, where the new table is shown. We refer you to the comment above.

- Ln. 339: “becomes more sensitive” – I’m unclear on what becoming more sensitive means for carbonate chemistry; perhaps illustrating with a clear example (“for instance, …”) would help

Thank you for your input. We realise that the term ‘sensitive’ is potentially misleading and we removed references within the text. Please, read the updated paragraphs that refer to this mechanism below, drawing from new Figure 7:

Lines 388-392: ‘Figure 7: The seasonal carbon sequestration potential defined as the ratio between the change in CO_2 flux and the added alkalinity carbon equivalent averaged over 2090-2099 (a, c), and the 2090-2099 time-integrated excess CO_2 flux (OAE minus control run) (b, d). The red contour line represents the region of alkalinity addition.



Lines 393-402: ‘OAE-driven impacts on carbonate seasonality are reflected on the seasonal carbon sequestration potential. Both in SSP1-2.6 and SSP3-7.0, it grows over the winter months and decreases over the summer months. The European region is characterised by a much more variable sequestration potential throughout the year, with values ranging between 0.2 and 0.44 (0.26, 0.61) in SSP1-2.6 (SSP3-7.0), while in the coastline region values are more constant, ranging between 0.06 and 0.12 (0.1 and 0.19) in SSP1-2.6 (SSP3-7.0). Additionally, high emissions drive a larger carbon uptake potential, which is related to the scenario-dependent chemical efficiency of OAE whereby, per unit of added alkalinity, higher atmospheric CO_2 concentrations increase the air-sea disequilibrium and decrease the ocean’s buffering capacity (Schwinger et al., 2024, Nagwekar et al., 2024, Nagwekar et al., 2026). ’

Lines 503-511: ‘ This finding agrees with Schwinger et al. (2024), who showed that increasing emissions drive an even further ocean uptake of anthropogenic CO_2 compared to a non-transient OAE simulation, due to elevated surface DIC concentrations and a decreasing buffering capacity of the ocean. Similarly, Nagwekar et al. (2026) found that, in SSP3-7.0, a higher air-sea partial pressure difference compared to SSP1-2.6 increases the size of the carbonate system response to alkalinity addition, driving larger additional CO_2 uptake. This same result was previously highlighted by Nagwekar et al. (2024), where OAE efficiency was higher in SSP3-7.0 (compared to SSP1-2.6) due to higher background atmospheric CO_2 and, most importantly, due to a decreasing buffer factor.’

- Ln. 352: “stronger chemical leverage” – again, I’m not sure what exactly is meant by this

Thank you for raising this point. After substantial restructuring following your comment, this is the new texts that address the point mentioned above:

Lines 397-401: ‘Additionally, high emissions drive a larger carbon uptake potential, which is related to the scenario-dependent chemical efficiency of OAE whereby, per unit of added alkalinity, higher atmospheric CO_2 concentrations increase the air-sea disequilibrium and decrease the ocean’s buffering capacity (Schwinger et al., 2024, Nagwekar et al., 2024, Nagwekar et al., 2026).’

Lines 508-511: ‘This same result was previously highlighted by Nagwekar et al. (2024), where OAE efficiency was higher in SSP3-7.0 (compared to SSP1-2.6) due to higher background atmospheric CO_2 and, most importantly, due to a decreasing buffer factor’.

- Ln. 353: “exacerbated by OAE implementation” makes it sound like the specifics of how OAE is implemented are important when you might just mean the use of OAE in general

Thank you for the comment. As you suggested, the intention was to state that OAE in general exacerbates the outcome that we see. However, due to substantial changes in the text, this sentence was removed.

- Ln. 352-353: this concluding sentence is similarly difficult to follow without a concrete example that would help readers follow; do, for instance, you mean that

Thank you for pointing this out. We fully agreed that the connection between higher emissions and higher carbon sequestration was poorly described. We removed this sentence and added more information on the interpretation of the results as per your previous comments. We refer you to previous responses on high versus low emissions, where we describe the interpretation of the results.

Lines 503-511: ‘This finding agrees with Schwinger et al. (2024), who showed that increasing emissions drive an even further ocean uptake of anthropogenic CO_2 compared to a non-transient OAE simulation, due to elevated surface DIC concentrations and a decreasing buffering capacity of the ocean. Similarly, Nagwekar et al. (2026) found that, in SSP3-7.0, a higher air-sea partial pressure difference compared to SSP1-2.6 increases the size of the carbonate system response to alkalinity addition, driving larger additional CO_2 uptake. This same result was previously highlighted by Nagwekar et al. (2024), where OAE efficiency was higher in SSP3-7.0 (compared to SSP1-2.6) due to higher background atmospheric CO_2 and, most importantly, due to a decreasing buffer factor.’

- Ln. 360-361: same point I made about the abstract; i.e. this sentence, which reads like it’s making a nice compare-and-contrast statement, is kind-of talking about apples and oranges by flipping between CO2 flux for the winter but pCO2 for the summer; looking at Figure 3, net flux is lowest in the summer when air-sea delta pCO2 is actually the greatest; which points to perhaps other factors (winds and gas transfer velocity, mixing depth) as being important (but less acknowledged here)

Thank you for raising this inconsistency. We fully agree that the relationship between ocean pCO2 and CO2 flux changes were not addressed clearly enough in the text. Here’s an updated paragraph of the system dynamic:

Lines 369-378: ‘Importantly, in the OAE runs in both the European region and coastline region, excess surface alkalinity accumulates during summer, prompting a stronger reduction of surface ocean pCO_2 which, due to fast air-sea exchange, turns net summer outgassing into a year-round carbon sink. OAE drives the largest excess CO_2 uptake in winter, as the seasonal air-sea CO_2 flux remains in phase with the MLD cycle, regardless of seasonal variations in alkalinity (Nagwekar et al., 2024). In the coastline region, a shallow bathymetry favours year-round mixing and stronger CO_2 uptake than in the European region. As for the role of the emission scenario, SSP3-7.0 drives an even stronger carbon uptake in winter, while maintaining summer values similar to SSP1-2.6, which further increases the CO_2 flux seasonal amplitude. As it will be described later, this behaviour is due to the lowering buffering capacity of the ocean under higher emissions.’

- Ln. 362: “sensitivity” – would this perhaps be better as “size of the response” or similar?; it’s not clear to me that readers will understand what “sensitivity” means here

Thank you for the comment. As per your previous comments, we agree that ‘sensitivity’ has a specific meaning and that ‘the size of the response’ would fit better in this context. See below the new paragraph where this sentence has been rephrased:

Lines 500-511: ‘Lastly, we investigate the role of the background scenario on OAE impacts. In our simulations the ocean’s carbon sink is increased more strongly in SSP3-7.0 than in SSP1-2.6, as surface alkalinity reaches similar levels in the two scenarios (Figure 3a, b) but surface DIC increases more under high emissions (not shown). This finding agrees with Schwinger et al. (2024), who showed that increasing emissions drive an even further ocean uptake of anthropogenic CO_2 compared to a non-transient OAE simulation, due to elevated surface DIC concentrations and a decreasing buffering capacity of the ocean. Similarly, Nagwekar et al. (2026) found that, in SSP3-7.0, a higher air-sea partial pressure difference compared to SSP1-2.6 increases the size of the carbonate system response to alkalinity addition, driving larger additional CO_2 uptake. This same result was previously highlighted by Nagwekar et al. (2024), where OAE efficiency was higher in SSP3-7.0 (compared to SSP1-2.6) due to higher background atmospheric CO_2 and, most importantly, due to a decreasing buffer factor.’

- Ln. 368: given the work focuses on a regional experiment in a major shelf sea, this list of limitations omits reference to the model’s resolution and representation of both shelf seas physics and biogeochemistry; there’s a mention of riverine inputs of ALK, which is good, but I thought MOPS was more an open ocean model than a shallow shelf seas model (I say this because the model I use is somewhat similar)

Thank you for raising this issue. In addition to the added section on model validation, we fully agreed that we should accept this as a limitation in our study and we added a new sentence to the Limitations section:

Lines 517-527: ‘Some limitations and future recommendations can be drawn from this study. With regard to FOCI-MOPS, the ESM represents coastal biogeochemical processes less well compared to the open ocean (Chien et al., 2022), which may affect the accuracy of absolute values in the region of alkalinity addition. While model results are mostly in agreement with GLODAPv2 observations, some of the biases may affect the veridicity of output values. Alkalinity in the Baltic Sea, for example, is poorly resolved by FOCI, which could have an impact on the North Sea boundary conditions. However, since our analysis focuses on relative differences between runs, the main conclusions in this manuscript would be less affected by such structural biases, as these are expected to affect both simulations similarly. Furthermore, while freshwater input from rivers is modelled in FOCI, riverine alkalinity is not accounted for. As river runoff has a strong seasonality, implementing such processes would improve the accuracy of future OAE simulations’.

- Ln. 364: the statement about locations like the southern North Sea may be dependent upon the mode of ALK addition used for OAE; where dissolution of particulate minerals is used to add the ALK, temperature effects may favour warmer equatorial waters; this is specifically examined in Palmieri & Yool (2024) where the effectiveness of different shelf seas at the global scale is evaluated

Thank you for raising this point. We should have clarified the fact that results may be dependent on the type of OAE deployment. In addition, as we dropped the North Sea section, we think that it would be more consistent to simply consider regions with shallow bathymetry and shallow MLD to be suitable for coastal OAE as alkalinity would accumulate more easily at the ocean surface. This is how we modified the text:

In the Introduction section:

Lines 74-85: ‘OAE could have several impacts on seasonal carbon dynamics, depending on the selected mode and site of alkalinity addition. For example, the rate at which alkalinity is added could produce different magnitudes and timing of CO_2 uptake within the annual cycle, causing asymmetrical or phase shifts to the air-sea CO_2 flux, or altering seasonal extrema. The location of OAE may be affected by the seasonal driver of the air-sea CO_2 flux as well as by physical properties, like bathymetry or regional ocean circulation. Furthermore, adding alkalinity at the surface may surpass critical thresholds, like pH or calcium carbonate saturation, which could in turn affect the ocean's net annual uptake and potentially affect local ecosystems, as other components of the carbonate system would respond accordingly. Lastly, as the ocean pCO_2 seasonal cycle is expected to increase due to rising atmospheric CO_2 concentrations (Gallego et al., 2018), studying the influence of background emissions would help understand the role of different climate mitigation pathways on OAE deployment.'

In the Results section:
Lines 435-440: 'In the coastline region, the MLD largely corresponds to the depth of the bathymetry for most of the year (Figure 3), which implies that a shallow, well-mixed compartment may support surface alkalinity retention throughout the year, due to rapid air-sea exchange. This finding may however be closely tied to the mode of OAE deployment, that allows the surface ocean to be continuously replenished with alkalinity and drive the disequilibrium between the atmosphere and the ocean'.

- Ln. 368: a style point, but I'd be inclined to move this whole paragraph into the Discussion rather than Conclusions; keep the Conclusions to your main findings (and maybe bullet-point them – but that really is a style point)

Thank you for the suggestion. We moved the Limitations paragraph to a new dedicated section below the Discussion. We decided to keep the Conclusions section more discursive, also following another reviewer's comment.

Lines 539-554: 'The present study addressed the changes to the seasonal carbon cycle driven by coastal OAE application under a low- and high-emission scenario, contributing to the understanding of OAE-driven CO_2 intra-annual variability in a continental shelf. It was found that, when addition takes place at the top layer, excess alkalinity accumulates more strongly in summer, as a shallow mixed layer retains alkalised water at the surface. This has the consequence of reversing, as well as amplifying, the seasonal cycle of surface alkalinity. Additionally, OAE reduces ocean pCO_2 more strongly in summer, when it is naturally highest, which, thanks to fast air-sea re-equilibration, turns summer net CO_2 outgassing into CO_2 net uptake year-round. However, OAE drives the strongest CO_2 uptake change in winter, when it is naturally strongest, as air-sea CO_2 flux remains in phase with the MLD seasonal cycle. While the design of our OAE runs mimics an idealised application, which is unlikely to become a real-world case, coastal OAE implementation is expected to become one of the more feasible approaches, due to proximity with existing coastal infrastructures. Nevertheless, such interventions could create additional pressures on local communities and ecosystems. Thus, drawing from the results highlighted in this study, other coastal methods like pulsed or point-source experiments should be explored to ensure safe large-scale OAE deployment'.

- Ln. 384: on the subject of style points, maybe trim people to initials only in this paragraph

Thank you. We applied the changes you suggested, and the Author contribution section now reads:

Lines 557-560: 'Conceptualisation: C. C., D. P. K.; Performing model simulations: T. P. K.; Data curation: C. C., D. P. K.; Formal analysis: C. C.; Investigation: C. C.; Methodology: D. P. K., T. P. K.; Supervision: D. P. K.; Validation: D. P. K.; Visualisation: C. C.; Writing – original draft: C. C.; Writing – review & editing: C. C., D. P. K., N. M., T. P. K., S. A..'