

Review:

Ciscato et al.: „Impact of simulated coastal ocean alkalinity enhancement on the seasonal cycle of the air-sea CO₂ flux and surface ocean pCO₂ in European waters under a low- and a high-emission scenario “

Key results

The authors use Earth system model simulations to investigate the seasonal effect of continuous ocean alkalinity enhancement along the European coastline. They apply two emission scenarios, namely a low-emission (SSP1-2.6) and a high-emission (SSP3-7.0) scenario. The study shows that OAE enhances the additional ocean CO₂ uptake mainly in winter and lowers the surface ocean pCO₂ mainly in summer. The sensitivity of the seasonal carbon cycle towards disturbances is increased in the high-emission scenario. The authors conclude that OAE is located best in regions with a shallow bathymetry and well-mixed waters.

This study is an important contribution to the current OAE research, as it applies alkalinity addition more locally and zooms into the temporal effects of alkalinity enhancement on the ocean carbon cycle. To date, many model studies on OAE still consider large-scale or even global alkalinity additions, and investigate the effects on a rather decadal than monthly time scale. Hence, the study of Ciscato et al. improves the understanding of regional ocean alkalinity enhancement on the carbon cycle seasonality and vice versa, also because of the opposing findings of previous studies on seasonal variations of OAE efficiency (e.g., Zhou et al 2025¹: higher efficiency in summer than in winter; Nagwekar et al. 2024²: air-sea CO₂ flux follows the seasonality of delta surface alkalinity, which is in turn determined by the MLD). The findings might be of particular interest for the development of monitoring-reporting-verification (MRV) technologies (i.e., timing of largest CO₂ uptake) as well as for the investigation of ecological effects of OAE.

While I encourage the publication of this manuscript, I also suggest some restructuring of the manuscript which might improve the clarity of the results and the findings. Most importantly, the discussion could focus more on the interpretation of the results (why is the timing of OAE-related changes in surface pCO₂ and enhanced CO₂ uptake different? How does this relate to the MLD and how does it go together with findings from previous studies?) as well as on conclusions and recommendations based on the findings (i.e., what is the best timing for alkalinity addition to be most efficient, could changes in the carbonate system seasonality and its amplitude potentially harm the ecosystem, ...). Furthermore, some rephrasing and consistent terminology could make it easier to read the paper. Below you can find my general comments as well as line-by-line comments.

¹ <https://www.nature.com/articles/s41558-024-02179-9>

² <https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2023EF004213>

General comments

Title: The title is rather long. How about replacing “seasonal cycle of the air-sea CO₂ flux and surface ocean pCO₂” by “seasonal carbon cycle” or “seasonality of the carbon cycle”?

Key points / Abstract: Could the second point be more tailored towards the effect on CO₂ uptake, which is key of OAE? I.e., does it matter that surface pCO₂ is lowered in summer but CO₂ uptake is much less enhanced than in winter? Would you say that OAE should rather be conducted in winter? The third point (sensitivity of the seasonal carbon cycle) is a bit unclear – is it a general statement or relevant for OAE? If the latter, in which sense and why does it matter that OAE causes a larger disturbance of the carbonate system in a high-CO₂ ocean? Finally, point c in the abstract: yes, but I’m not sure if you can conclude this from your results. Isn’t it rather that alkalinity is not exported out of the region or to depth (= circulation effect) and that these high alkalinity concentrations as you can see in Fig. 4 help to sustain a large amount of additional CO₂ uptake?

Short summary: I would suggest to rather use the limited space to describe the findings of your study instead of OAE in general. I would assume that interested readers have at least a general understanding of ocean alkalinity enhancement.

Methods: I suggest to be careful with the term “study area” – do you use it for the North Sea only (as suggested by the subsection title of 3.1) or for the European waters as displayed in Fig. 1, red box? And then you also have the coastlines / regions of alkalinity addition, which would need a consistent naming.

Results: It is in parts unclear whether you are looking at the results of the coastal regions or of the European waters as defined in the methods; this could be added more clearly to the text. Furthermore, I do not understand entirely why you focus on the North Sea and its spatial patterns in section 3.1, while this does not play a big role in the subsequent results. I would suggest to either create similar plots as in Fig. 3 for the North Sea (and add the respective description and discussion of the results) or omit the North Sea section and focus only on the entire European waters and the coastline. For all parts, it would be useful to add a few more numbers to the text instead of only writing “is higher/lower than”. This helps the reader to get a feeling for the effect range. Finally, I wonder if you could compute efficiencies (Δ DIC over Δ added alkalinity or Δ fCO₂ over Δ added alkalinity) for each month, which could give you a better handle to discuss the seasonal effects of OAE.

Discussion: The discussion repeats in large parts the results and could focus more on interpreting the findings. For example, a main finding is that the seasonal cycle of surface alkalinity is reversed by alkalinity addition (despite the continuous, year-round alkalinity addition), with interesting feedbacks of the other carbon cycle components. Which implications could this have? What does it mean for real world OAE applications if the time of lowest surface pCO₂ and the time of the largest amount of additional CO₂ uptake are different? Why is this the case? Furthermore, also here it is in parts unclear whether you are talking about the baseline, the OAE simulation, or the difference between both.

Conclusion: This is rather a summary of the results and a list of limitations (and related recommendations). I would suggest to restructure this part and rather focus on the take-aways and learnings from the study – accepting the limitations – for example regarding real-world applications of OAE including MRV and potential effects on the ecosystem.

Line-by-line comments

Abstract

L. 33-34: “when carbon cycle seasonality is temperature-driven” – I don’t understand this sentence. When is this the case? What if it is not temperature-driven?

Introduction

L. 68-69: “and eventually stored in the ocean for millennia” – are we sure about this? For example, Köhler (2020)³ reports that the carbon is only sequestered as long as the added alkalinity stays in the ocean. There might be some alkalinity loss, for example by sediment processes. Hence, I suggest to phrase this more cautiously, e.g. “and has the potential to be stored in the ocean for a long time”.

L. 81: “DIC leads to CO₂ loss” – please check the wording; CO₂ is a part of DIC, and “DIC” is not a process.

L. 97: “with potential reflections on marine ecosystems at different scales” – temporal or spatial scales? Statement is a bit unclear.

L. 93-105: I feel that none of the arguments gives a reasoning why it is important to investigate the seasonality of OAE. Perhaps you can restructure, arguing that despite the continuous addition of the same amount of alkalinity, the surface carbonate system can be affected differently depending on mixed layer etc, which then in turn modifies the CO₂ uptake on a seasonal scale (and then give some references – there are some studies which have looked at this already, either with continuous addition or with seasonal/pulsed additions).

Materials and Methods

L. 122: “with a horizontal resolution of 1/2°” – is it possible to give the resolution in km in addition? This could be a bit more intuitive.

L156-157: “equivalent to 1.4 μmol C m⁻² y⁻¹” – why in units of carbon?

³ <https://www.frontiersin.org/journals/climate/articles/10.3389/fclim.2020.575744/full>

Results

L. 182-189: “Furthermore, this is the region where the OAE-driven seasonal change is strongest in our model” – as already mentioned before, it is currently unclear to me why you take a closer look at the dynamics of the North Sea? Not sure if the North Sea per se is the region of largest change; looking at Fig. 4a,b,d,e, the change is largest where the bathymetry is shallow, but this could also be the case for other regions in your study area, right? So I would rather link the investigation to the bathymetry rather than a specific region, or link the findings displayed in Fig. 2 more to a plot such as the ones in Fig. 3.

L. 190: “the seasonal air-sea CO₂ flux in the study area”- unclear which “study area”; the North Sea or the red rectangle in Fig. 1?

L. 207-215: Could you give numbers for the cumulative, area-integrated CO₂ flux in the northern and southern North Sea?

L. 242: “Under both SSPs the MLD-averaged alkalinity spatial pattern” – for the baseline or the OAE simulations?

L. 245: “like Iceland, Spain and Norway” – matter of taste, but I would always talk about the “*off the coasts of Iceland, Spain and Norway*”, because Iceland, Spain and Norway are not open ocean.

L. 242-246: can you give numbers for the change in MLD (both the seasonal variation and the modification by OAE) in meters? Is it possible to estimate the size of the area (in km²) where the MLD change is largest?

L. 249-259: “as a result of phytoplankton bloom and consequent high NPP (fig. 2c)” – the referenced figure is only for the North Sea and not for the entire region though. I suggest to either rephrase the sentence or add a Hovmöller plot for the entire region.

L. 250-252: “In the OAE simulation, ocean pCO₂ absolute values drop and the amplitude decreases from 35 μatm to 23 μatm in SSP1-2.6 and from 73 μatm to 55 μatm in SSP3-7.0.” – I don’t understand this statement. Yes, pCO₂ is decreased with the added alkalinity, but as the seasonal change is, according to your interpretation, caused by NPP, this amplitude would be the same if NPP remains the same, right? Or is there a change in NPP and if yes, why? And if so, which additional effects does this have on the seasonality of the carbonate system?

L. 253: “At the coastline” – I suggest to start this paragraph a bit more smoothly, e.g. “While in the European average, pCO₂ exhibits the same seasonality in both OAE and baseline simulations,”

L. 255: “in the OAE scenario” – I suggest to avoid the term “scenario” if not used in the context of “SSP scenarios”, otherwise this can be confusing. Rather use the term “simulation”.

L. 255: “ocean pCO₂ decreases by about” – suggest to add the initial value (“decreases from X μatm by about”); same at the end of the sentence (“reduced by 11 μatm”) and in the subsequent sentence.

L. 259-260: “with the strongest variation occurring in summer” – for coastal or entire European region? Can you give the months in summer that you are referring to?

L. 262-263: “this results in a reduction of the seasonal pCO₂ amplitude that is more pronounced under low emissions” – hm, doing the maths using the numbers of two paragraphs before gives me: for SSP1-2.6: 35-23 = 12 μatm, for SSP3-7.0: 73-55= 18 μatm. Hence, the effect on the amplitude is more pronounced under high emissions, isn't it?

L. 264-267: “Thus, while in a low-warming climate, alkalized water becomes less sensitive to DIC fluctuations, higher atmospheric CO₂ partially counteracts the buffering effect driven by alkalinity addition, leading to a higher Revelle factor.” – yes, but this is common sense, isn't it? Or what is your specific finding in terms of seasonality? Furthermore, I suggest to be more careful with statements about the Revelle factor; if you haven't looked at it (maybe not written as an output?) you can only speculate.

L. 277: “a distinct spatial pattern of pCO₂ seasonal change is identified” – add that this is caused by OAE.

L. 288-289: “Alkalinity addition enhances the CO₂ flux seasonal cycle everywhere” – that sounds as if also outgassing in summer is enhanced; is this the case?

L. 289-290: “turning the system into a CO₂ sink year-round” – this sounds like an interesting and relevant finding that could be highlighted more.

L. 286-292: This entire paragraph would benefit from numbers which help the reader to understand the magnitudes of the impact.

L 293: “OAE-induced CO₂ flux amplitude has an average span of” – can't you simply say that the effect of OAE on the seasonal CO₂ flux is XYZ?

Fig. 2c: Does it make a difference if you look at depth-integrated NPP? I don't know your model, but considering the entire euphotic zone can sometimes change the picture of spatial NPP patterns compared to surface-only values.

Fig. 3 caption: “ocean pCO₂” – change to “surface ocean pCO₂”

Fig. 4 caption: “Seasonal amplitude change (OAE – baseline)” – I'm not sure if I understood the computation entirely. Could you add this either in more detail to the caption or as a short paragraph to the methods?

Discussion

L. 307-312: I suggest to move this part to the end of the discussion in a dedicated “limitations” paragraph.

L. 313: “In agreement with Schwinger (2022)” – this statement is true for both scenarios, right? I suggest to add this to the sentence.

L. 313-321: I think this paragraph needs more discussion. You repeat your findings, but it is still unclear to me how the lowered surface pCO₂ in summer and the increased CO₂ flux in winter fit into the same picture. I think the carbonate system responses need to be clearly described, perhaps a schematic would help. Furthermore, it should also be discussed with regard to previous studies that were looking at seasonal OAE effects.

L. 315-316: “Additionally, OAE-induced modifications to ocean pCO₂ are highest” – I suggest to rephrase to “Additionally, OAE-induced reductions of surface ocean pCO₂ are largest”

L. 322: “Both in European waters and at the coastline, highest alkalinity is recorded” – in the baseline or the OAE simulations?

L. 326-328: “Considering the physical features of the southern NS, where a shallow water column is well-mixed throughout the year, OAE is likely to encourage fast equilibration and allow for efficient carbon sequestration under enhanced alkalinity.” – okay, but what about the remaining North Sea and the rest of the European region?

L. 330: “In the OAE simulations, the seasonal cycle of surface alkalinity is reversed under both SSPs, especially in summer” – I would continue this sentence with a reason why this is the case and speculate which effects this could have on biogeochemistry and ecology.

L. 332-333: “show a less significant decline” – I suggest to avoid the term “significant” if not used in a statistical context.

L. 333-336: “the amplitude compression signal”, “the SSP3-7.0 coastal amplitude”, “the seasonal amplification” – these are a variety of terms that might all be used in a similar sense. I suggest to create certain terms for a particular OAE-induced change in the carbonate system seasonality and use them consistently throughout. If it is too difficult to describe in words which amplitude is changed into which direction, you could accompany the term definition with a simple schematic.

L. 336-337: “For the CO₂ flux, since the seasonal amplification is stronger in SSP 3-7.0, higher background emissions make the system more susceptible to external alterations” – isn’t the argumentation the other way around? Because of higher emissions the sensitivity and, thus, the seasonal amplitude is stronger.

L. 348-349: “OAE alone is less efficient in mitigating seasonal amplitude changes in the high-emissions scenario compared to the low-emission scenario” – you lost me here (and in the following sentences). The purpose of OAE is not to modify any seasonal signal; and I’m a bit

confused about the direction of changes. Again, perhaps a schematic or summary figure could help. And a small note: check to be consistent, use either “high-emissions scenario” (with s) or “high-emission scenario” (without s).

L. 351-352: “an overall expansion of the CO₂ seasonal cycle” – expansion spatially, temporarily, stronger amplitude? This is currently unclear.

Conclusions

L. 358: “under low and high warming” – this comment might be a bit picky, but the warming is secondary, the main driver of the scenarios is actually the change in emissions. The warming then depends on the model-specific sensitivities. So I would talk about “high and low-emission scenarios”.

L. 362-364: “the sensitivity of the seasonal carbon cycle increases under high emissions, enhancing the CO₂ flux seasonal amplification and contrasting the pCO₂ seasonal dampening” – I suggest to rephrase to “the sensitivity of the seasonal carbon cycle increases under high emissions, enhancing the OAE-driven excess CO₂ uptake in winter and dampening the OAE-driven reduction in surface ocean pCO₂.”

L. 368-381: I would suggest to move this into a dedicated limitations paragraph at the end of the discussion rather than adding this to the conclusion. You summarize what you could have done better (and there are always things to improve), but in a conclusion you should rather talk about potential next steps that could follow building on your scientific findings.

L. 372: “Additionally, as river runoff features are deeply seasonal...” – is deeply the correct word here? Shouldn't it be rather something like “has a strong seasonality”?

L. 377-379: “Lastly, our OAE simulations picture a highly idealized scenario, which is unlikely to become a real-world case.” – yes, but this is actually the case for almost all past and current OAE modelling studies, right? But we can still learn from it – I suggest to rephrase this sentence to admit the simplifications but highlight the value of the study.