Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2021-48-RC1, 2021

Comment on bg-2021-48

Anonymous Referee #1

Referee comment on "A modelling study of temporal and spatial pCO_2 variability on the biologically active and temperature-dominated Scotian Shelf" by Krysten Rutherford et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-48-RC1, 2021

This work seeks to identify the role of local event-scale variability – namely upwelling – in determining the regional air-sea carbon dioxide fluxes over the Scotian Shelf through the integration of several different data sets as well as the use of a regional numerical model. The paper features wonderful contextualization of previous flux estimates with observational limitations and integration of multiple kinds of data for this regional problem. The problem itself is quite timely as recent work has identified that the coastal ocean rates of change in carbon dioxide may always reflect the global changes. The manuscript requires additional details in the methods section – most notably about the regressions used to drive the initial and boundary conditions and river values, some issues with time surrounding the observations used and the simulation years, as well as the methods pertaining to evaluation of the model itself. In additional, more attention needs to be paid to the role of the Revelle Factor in driving these interregional differences between the upwelling on this shelf and the CCS. Finally – an most importantly – the authors need to clarify how the upwelling event contributes to the shelf wide estimates more clearly. The paper would be publishable in Biogeosciences if these issues can be addressed by the author team. More specific comments follow.

Major Comments:

The main message appears to be that local processes are important for carbon content of the temperate Scotian shelf region. In the context of that message, the authors need to show how the localized upwelling event contributed to the overall regional flux somehow. One way might be to show this flux as a map. While there is quite a bit of information on the in situ observed location's variability, there is very little about how that compares to the region as a whole – is it representative?. For instance, where in Figure 1 does this

upwelling occur (at the buoy and along the black line/transect?) – and how does the simulated flux at the surface of the entire region compare to this localized event? How fine of a resolution do we need to observe to get the shelf-based flux estimate direction right? Also, how does this flux compare with other regional/broader scale fluxes reported for the North Atlantic?

Secondly, it is critical to clarify time in this work. 2005 was the year when the warming started intensely on the east coast of North America. The model runs happen before that, but the comparisons are to data after that…. How does that impact the results? What about the time variability of carbon dioxide in the atmosphere over these various intervals?

The comparison to the California Current or other traditionally upwelling situations is not entirely accurate as the vertical gradient in DIC (presented in the figure here) is nearly half what it is in the CCS (Feely et al. 2004). The phytoplankton growth at the surface is quite efficient unless the winds blow too strongly and the phytoplankton can no longer grown in place. This aspect of the upwelling system is neglected in the text. The signature of the phytoplankton drawdown can be seen very far offshore as it takes nearly a year for CO2 to equilibrate at the surface. In addition, the two systems likely experience very different temperature, salinity, and alkalinity parameter spaces – all of which are important to consider for the response of the carbon system.

The Revelle Factor influence on the differences between what is observed on the Scotian Shelf and in the CCS should be included – for an example described in more detail see here: https://www.sciencedirect.com/science/article/pii/S0278434317303643#f0005

Because the Revelle Factor is important to consider within the context of this issue, it would be important to evaluate DIC and TA with in situ observations locally, here within this manuscript. Please add DIC and TA evaluation of the model fields. Do observations of these fields exist for the simulated period?

The methods requires quite a bit more detail. Specifically, what is the model skillful in (Lines 112) from other studies? Was it evaluated mostly at the surface? Over annual timescales? Or events like in this work? The K1 and K2 constants chosen are not meant for regions that experience a lot of freshwater influence. Can you justify their choice in this region by discussing the salinity ranges that this region observes? What atmospheric carbon dioxide concentration was used?

Most importantly in the methods – the boundary condition DIC and TA relationships and river concentrations require additional documentation. In the case of the boundary conditions, they appear to rely solely on data from the winter months from an unspecified location. Can you add these relationships to supplemental? And describe the data that they rely on? Are they from a similar time period that was simulated? Were adjustments made for time in the DIC field if they were observed more than 5 years earlier/later than the simulations? There are existing hydrographic relationships in the region and globally that could be used instead (McGarry et al. 2021; Xu et al. 2020; CANYON; LIAR) – why generate a new one?

Finally, the point that the upwelling event signal leads to reduced outgassing compared to the rest of the shelf (Line 280-281) is not clearly shown and is related to the main point of the work. The reader is still considering (because none of the other fields were shown) that maybe the phytoplankton growth rate in relationship to the winds -documented in Evans et al. (2015) could also be contributing to this. What does the subsurface pool of pco2 look like prior to these events? Is that getting efficiently drawn down or is the biological response week and so the physical transport is the main control over the surface carbon concentration? See more discussion on the role of event based air-sea carbon fluxes in annual variability for a region here:
https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/2010JC006625

Minor Comments:

Line 52-52: Please add the Feely et al. 2008 citation here (https://science.sciencemag.org/content/320/5882/1490).

The model gas transfer function chosen is Ho et al. (2006), which is different than the earlier Fennel model iterations. How does this choice (between all of the existing gas transfer functions available) influence your results?

Lines 213-214: Can you add statistics to support "good agreement" here?

Line 292: If you averaged your two regions together - would your results be more in line with theirs?

Line 314: " thermodynamic signal in *p*CO2 outweighs the influence of biological activity " This is not clearly shown in this work.

Figure 2 - Add statistics (RMSE etc) directly to these plots. Is the smoothing of the model part of the issue? what about the time/spatial mismatch? Is the socat data being interpolated to the location of the mooring? was the model? how was that extracted? These details need to be added to the methods as well – evaluation methods.

Figure 3 – The summer gradient generated by the upwelling (observed) does not appear to be captured by model. Can you address this with respect to the localized mechanism that is the focus of this work? Please add some discussion of this to the text. Is the time period the same between simulated and observed?

Figure 4 – the longitudinal gradient in the observations does not appear to be well captured by the model. Is there additional evidence that the model simulates the upwelling in this area well?

Figure 6 - Highlight the “nearshore” region you mention in the text on this figure. The DIC gradient is not as severe as in the CCS. Consider putting it in this space:

https://www.sciencedirect.com/science/article/pii/S0278434317303643#f0005

Figure 7 – Please add other parameter time series to this plot including temperature, salinity and most important winds (both modeled and observed).

Figure 8- More detail needs to be added to methods about how these comparisons were made.

Figure 9 – Please add vandemark discussion to the text. What is the far right ”section”?

Finally, the title would be more informative if it were about the science question the paper is trying to address.

McGarry, K., Siedlecki, S. A., Salisbury, J., & Alin, S. R. (2021). Multiple linear regression models for reconstructing and exploring processes controlling the carbonate system of the northeast US from basic hydrographic data. *Journal of Geophysical Research: Oceans*, 126, e2020JC016480. https://doi.org/10.1029/2020JC016480

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