A modeling study of temporal and spatial pCO_2 variability on the biologically active and temperature-dominated Scotian Shelf

Response to Comments by Reviewer 1

(Reviews are included in black font; Responses are in blue font)

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 – and 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.

Response: We appreciate the constructive comments and will pay close attention in our revision to clarify the objectives of our study and to provide or emphasize the methodological details requested by the reviewer. The main objectives of our study are to show (1) that the Scotian Shelf, as a whole, acts as a net source of CO_2 to the atmosphere, (2) that local processes drive seasonal and spatial variability of pCO_2, and (3) to present an assessment of how well our regional model captures these processes. The methodological details will be provided as detailed in the responses below. A discussion of the Revelle factor is beyond the intended scope of this study. Likewise, as explained in more detail below, the upwelling event is not a major contributor to the shelf-wide air-sea flux and we did not mean to suggest it is. This will be stated clearly in the manuscript (see response to next comment).

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?

Response:
First, we would like to emphasize again that the main objectives of our study are to show (1) that the Scotian Shelf, as a whole, acts as a net source of CO_2 to the atmosphere, (2) that local processes drive seasonal and spatial variability of pCO_2, and (3) to present an assessment of how well our regional model captures these processes. We accomplish this by combining two high-resolution data sets, a timeseries (CARIOCA buoy) and regular cross-shelf transects (Atlantic Condor cruises) with a high-resolution regional model.

In response to: "the authors need to show how the localized upwelling event contributed to the overall regional flux somehow.":

One of the main messages of our paper is that the flux across the shelf is relatively uniform and that these localized summer upwelling events do not contribute significantly to shelf-wide fluxes but would be more important locally. We believe this is an interesting contrast to other shelves with summer upwelling (e.g. the California Current System or CCS), where these events have been shown to contribute significantly to air-sea fluxes. This will be stated more clearly in section 4.3 Regional Flux Estimates (line 237) where we will add:

"*Bin 1 along the Atlantic Condor transect (upwelling bin, Figure 1) has an annually integrated flux of 2.3 mmol C m^{-2} yr^{-1} compared to bin 2 (shelfbreak bin, Figure 1) with an annual flux of 2.2 mmol C m^{-2} yr^{-1}.*"

In section 5 Discussion (line 284) we will add:

"*Additionally, annual air-sea CO_2 fluxes in bin 1 (upwelling bin, Figure 1) are estimated in the model to be 2.3 mmol C m^{-2} yr^{-1} compared to bin 2 (shelfbreak bin, Figure 1) with an annual flux of 2.2 mmol C m^{-2} yr^{-1}. This is compared to the entire shelf flux of 1.9 mmol C m^{-2} yr^{-1} and the flux at the CARIOCA buoy of 2.4 mmol C m^{-2} yr^{-1}. Our results indicate that the upwelling events are such short term events that they do not significantly affect the fluxes and that shelfwide fluxes on an annual scale are relatively uniform.*"

In response to: "where in Figure 1 does this upwelling occur (at the buoy and along the black line/transect?)":

We will add the location of the CARIOCA buoy and condor transect to the upwelling figure (top panel Figure 6).

In response to: "How fine of a resolution do we need to observe to get the shelf-based flux estimate direction right?":

As mentioned above, air-sea flux is rather homogenous across the shelf and the localized upwelling events do not noticeably affect shelf-wide air-sea flux. Although more observations would be better, of course, it appears that the combination of the high-resolution time series and the cross-shelf transects provide adequate resolution to support our conclusions.

In response to: "how does this flux compare with other regional/broader scale fluxes reported for the North Atlantic":

We provide such reported fluxes in Figure 9 where we compare our flux estimates to other regional and global fluxes reported for the region (from Grand Banks to Gulf of Maine).

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?

Response: We will update our manuscript using an extended simulation from 1999-2014. This would encompass the observation years from the CARIOCA buoy (2007-2014). When plotting model and observations from different years in the same graph, we will perform a simple detrending where we map these observations onto the common year using the long-term atmospheric pCO_2 trend of ~ + 2 µatm/year. Analysis of the warming observed on the east coast of North America is outside of the intended scope of this paper, which aims to address seasonal variability. A model analysis of long-term trends with the same model is forthcoming.

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.

Response:

We fully agree with the Reviewer that the Scotian Shelf and the CCS behave very differently. In fact, we believe this is one of the interesting findings of our study and we would like to make this clearer in the Discussion section by referring and comparing to Feely et al. (2008). More specifically, we will emphasize how these two systems are different, bringing in reference to the transects in Figure 2 of Feely et al. (2008) (included below) and compare to our Figure 6. We will point out the differences in size and shape of the two shelf regions, which lead to different types of water being upwelled. In the CCS, the shelf is much narrower than the Scotian Shelf (California shelf ~10 km; Scotian Shelf ~120-240 km), which means that upwelling in the CCS brings water from deeper in the water column (below 150 - 200 m) of the open ocean across the shelf break to the surface. In contrast, upwelling on the Scotian Shelf brings intermediate-layer water (from between 20 – 100 m) from the shelf itself. While the temperatures of the upwelled water are similar in both systems (~8ºC), upwelled water in the CCS has much higher DIC concentrations (~2200-2250 umol kg^{-1}) than the Scotian Shelf (2060 mmol m^{-3}). This will be added to the Discussion section.

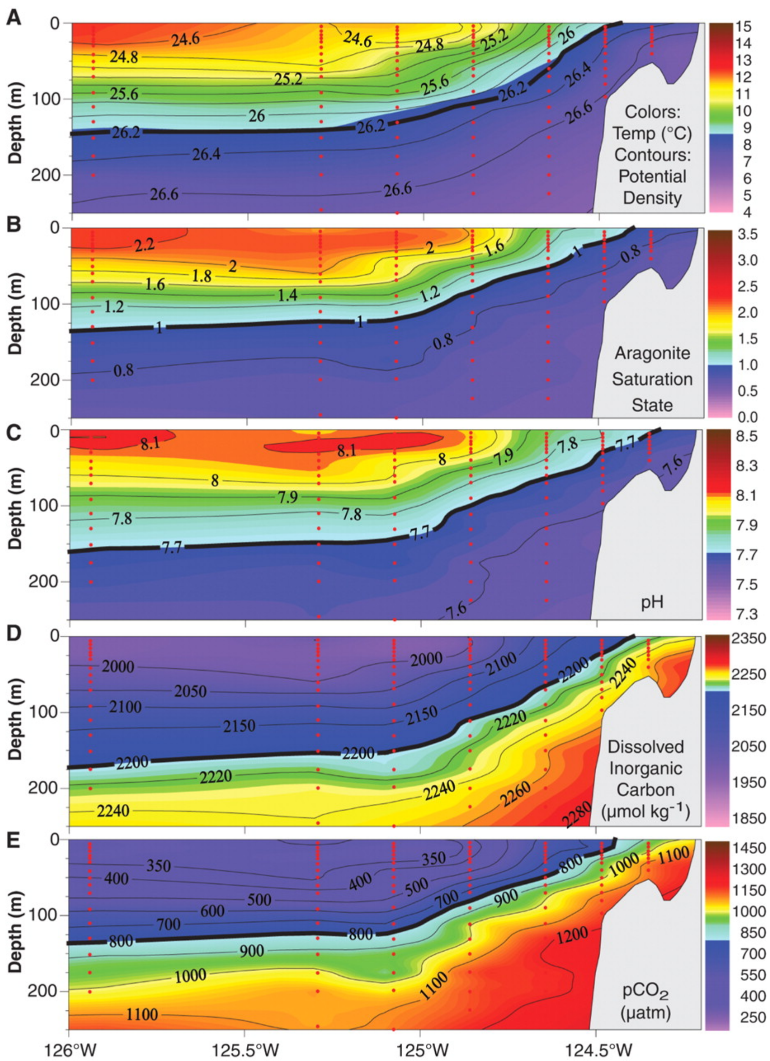


Figure 2 from Feely et al. (2008)

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#f000
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?

Response:

We agree with the Reviewer that an evaluation of DIC and TA will be useful. We will add this using in-situ observations available from the Department of Fisheries and Oceans (DFO) Atlantic Zone Monitoring Program (AZMP). We will focus on DIC and TA. A discussion of the Revelle factor is beyond the intended scope of this manuscript.

The methods require 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?

Response:

Regarding Lines 112ff, we would like to expand the text as follows:

"For a detailed description and validation of the biological model, we refer to Laurent et al. (2020), who showed that it outperforms global models for the region in terms of model skill at representing nitrate and chlorophyll. Our model was evaluated on a seasonal scale for the entire model domain, mainly in the surface (top 100 m). Laurent et al. (2020) found that the model captures the timing of the spring bloom relatively well, but underestimates the magnitude of the chlorophyll concentrations during the bloom and tends to overestimate nitrate throughout the year. "

Regarding our use of our K1 and K2 constants, we would like to modify the text to: *"…we use dissociation constants (K1 and K2) from Millero et al. (1995) using Mehrbach et al. (1973) data on the seawater scale which are deemed appropriate for the typical salinity ranges from 27 to 36.6 in the model domain (lower salinities are highly localized in the Gulf of St. Lawrence Estuary)."*

Regarding atmospheric CO_2 concentrations:

As stated on lines 115 to 116: "Atmospheric $p$$CO_2$ is set to the seasonal cycle and secular trend derived from Sable Island monitoring data contributed by Environment Canada's Greenhouse Gas Measurement Program (Environment and Climate Change Canada, 2017)."

We will add the trend equation with seasonal cycle to the Supplement with a figure illustrating it.

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?

Response:

We will add a more detailed description of the DIC and TA data in the Methods section (they are from DFO's AZMP program mentioned above). The relationships are reported there already (lines 132-138). We would like to modify the text as follows:

"*The model is initialized on January 1, 1999 from Urrego-Blanco and Sheng's (2012) solution for temperature and salinity. Nitrate (NO_3^-) concentrations are initialized from regional climatologies as in Laurent et al. (2020). DIC and TA initial and boundary conditions were created from observationally based relationships with temperature (T) and salinity (S) using bottle data from regional cruises from 1997-2011 encompassing as far south as the Gulf of Maine and as far north as the Labrador Sea (observations from DFO's AZMP program, see: dfo-mpo.gc.ca/science/data-donnees/azmp-pmza/index-eng.html#publications). Initialization relationships used only observations from December, January and February (TA = 43S + 800,*

$r^2 = 0.96$; $DIC = 1153 - 21.6T + 29.1S - 0.41T^2 + 0.63ST$, $r^2 = 0.90$). Boundary conditions used observations that encompass the entire year ($TA = 41S + 875$, $r^2 = 0.92$; $DIC = 912.6 - 2.4T + 35.7S - 0.45T^2 + 0.12ST$, $r^2 = 0.80$)."

Why did we not use other relationships? Aside from the obvious reason of timing (we have been working on the model for a few years while McGarry et al. 2021 and Xu et al. 2020 were just recently published), we believe it is crucial to use observations from our shelf region. McGarry et al. (2021) focuses on Gulf of Maine and does not include most of our study region. Similarly, Xu et al. 2020 focuses on MAB and SAB, and not our study region. Since the Gulf of Maine, MAB and SAB are all more strongly influenced by Gulf Stream water than the upstream shelves that we focus on (e.g. Fennel et al. 2019), it is important to use hydrographic relationships that are specific to our region of focus. We do not believe the CANYON fields are appropriate as they are derived from open-ocean not shelf data. Furthermore, as CANYON requires the use of oxygen data in addition to temperature and salinity, which we did not have access to for the entire region, and any workarounds would introduce errors. Likewise, we did not use LIAR because it was optimized for the open ocean not the shelf.

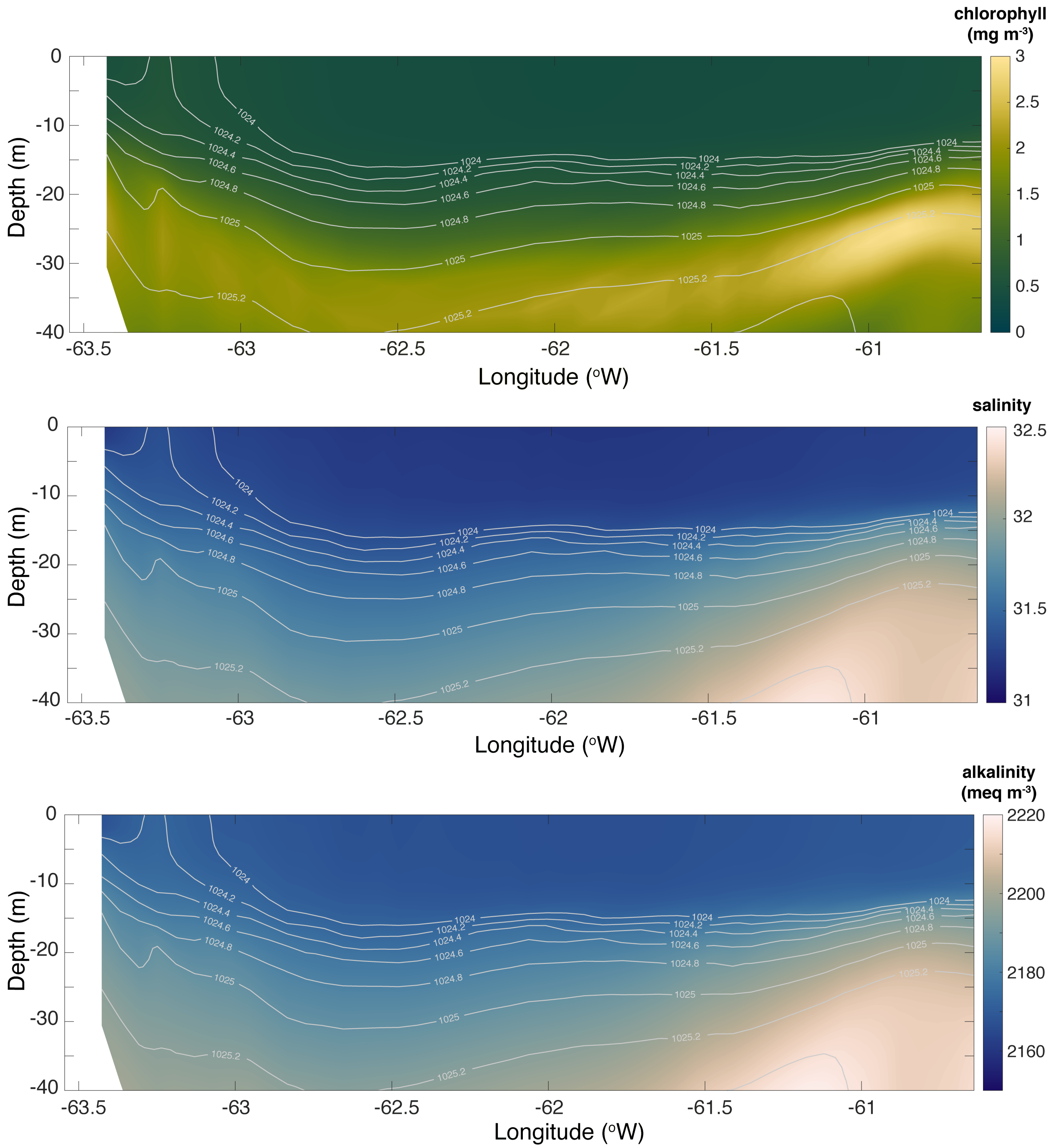
Finally, the point that the upwelling event signal leads to reduced outgassing compared to the rest of the shelf (Line 280-281) is not clearly shown and is related to the main point of the work. The reader is still considering (because none of the other fields were shown) that maybe the phytoplankton 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

Response:

We believe that lines 280f need to be reworded to de-emphasize the upwelling events. As stated above, we do not see this as the main focus of our paper, and these are relatively short-term events that have no marked influence on annual or shelf-wide fluxes.

Regarding the comment on phytoplankton growth, we would like to add the transects below to the supplement so readers are informed of other variables at the time of the upwelling event. Our interpretation is that during the event, physical transport is the main control on the spatial variability.

In addition, as per the suggestion from the other Reviewer, we will add a Taylor Decomposition to better illustrate how these different factors are affecting the pCO_2 signal during the upwelling events.


chlorophyll
(mg m-3)
salinity
alkalinity
(meq m-3)
Depth (m)
Longitude (°W)
-63.5
-63
-62.5
-62
-61.5
-61
0
-10
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3
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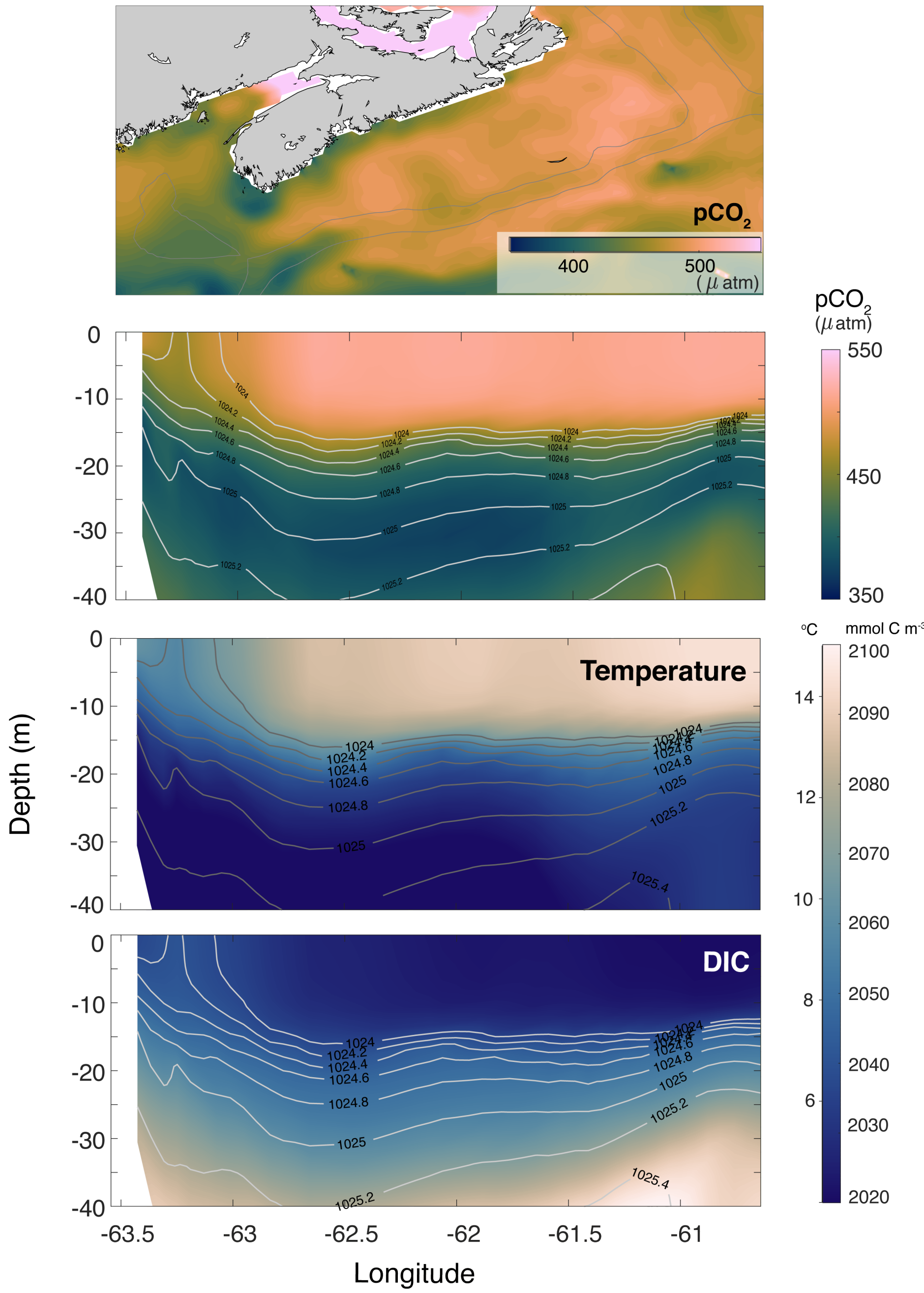

Minor Comments:
Line 52-52: Please add the Feely et al. 2008 citation here (https://science.sciencemag.org/content/320/5882/1490).
Response: Yes, we can add this reference.

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?
Response: Since we are not focusing on short-term, high-wind events, most of the gas transfer functions yield similar results without much divergence. A few years ago, we updated from Wanninkhof (1993), which is the gas transfer function originally used in the Fennel model, to Ho

et al. (2006) because we were criticized for using on outdated parameterization. Although it is thought that Wanninkhof (1993) potentially overestimates gas transfer, particularly at higher wind speeds (Ho et al. 2006), both yield similar results for the air-sea CO_2 fluxes in our model.

Lines 213-214: Can you add statistics to support "good agreement" here?
Response: In Figure 3, we report overall statistics comparing the model to the Atlantic Condor transect, with an RMSE of 20.3μatm and a bias of 4.1 μatm.

Line 292: If you averaged your two regions together - would your results be more in line with theirs?
Response: No, if we averaged our two regions together our estimate would not be more in line with the estimate from Laruelle et al. (2015).

Line 314: " thermodynamic signal in pCO2 outweighs the influence of biological activity " This is not clearly shown in this work.
Response: Agree, this statement is a reference to Shadwick & Thomas (2014). We would like to modify the text to: *"In summer, temperature-normalized pCO_2 continues to decrease rather than follow the increasing temperature signal of non-normalized pCO_2. Previous studies have noted that, in summer, the thermodynamic signal in pCO_2 outweighs the influence of biological activity (Shadwick et al. 2011; Shadwick & Thomas 2014), which could explain the differences in seasonality between pCO_2 and temperature-normalized pCO_2 in the present study. We believe this thermodynamic influence is an important factor driving the net outgassing observed on the Scotian Shelf, particularly when combined with the delivery of DIC-rich water from the Labrador Sea."*

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.
Response: We will add the RMSE and bias directly to the plots. The model was not smoothed and model and data are shown in the same location (no spatial mismatch). By plotting data from multiple years, we provide information about the range of temporal variability and hope to eliminate artifacts due to temporal mismatch. Note though, that we will redo figures and correct for trends by mapping values form different years onto the same reference year. We believe the main issue is that the magnitude of the bloom is not large enough in the model to capture the rapid and large decline in pCO_2, as stated in the text. The model was extracted at the buoy location. The SOCAT data was averaged over the Scotian Shelf, as indicated in the figure caption.

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?
Response: Is the Reviewer perhaps referring to Figure 4? Figure 3 is not intended to show evidence of summer upwelling in either observations or the model but shows the annual and shelf-scale changes in pCO_2. Upwelling in the model is also illustrated in Figures 5 and 6. We

will add a more detailed comparison of modelled SST to satellite SST to validate the model's ability to reproduce the coastal upwelling in the region.

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?
Response: The occurrence of summer upwelling is well-documented on the Scotian Shelf, e.g. Petrie et al. (1987) used satellite images of the region to show the development of a band of cool water along the southern shore of Nova Scotia over the month of July 1984 caused by upwelling-favourable winds (see Figure below).

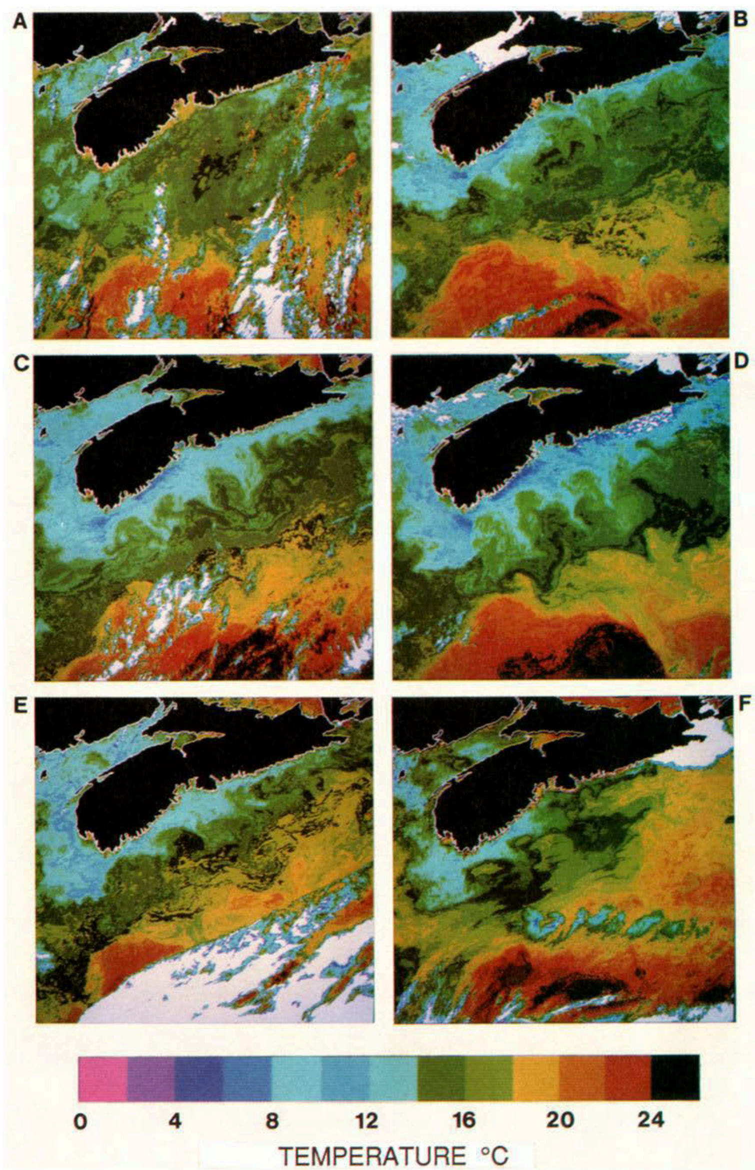

Figure 1: Satellite infrared imagery of sea surface temperatures from (a) July 7, (b) July 14, (c) July 21, (d) July 25, (e) July 31 and (f) August 6, 1984. Image is from Petrie et al. (1987) illustrating narrow band of cool water on the southern shore of Nova Scotia during a period of upwelling-favourable winds.

A more recent example from Shan (2016) showing both satellite images and simulated model snapshots of SST in July 2012 is given below and illustrates again the band of cool upwelled waters on the southern shore of Nova Scotia in the vicinity of the coast. Shan (2016) noted two distinct upwelling events during 2012, one that peaked July 22 and the other September 1, 2012.

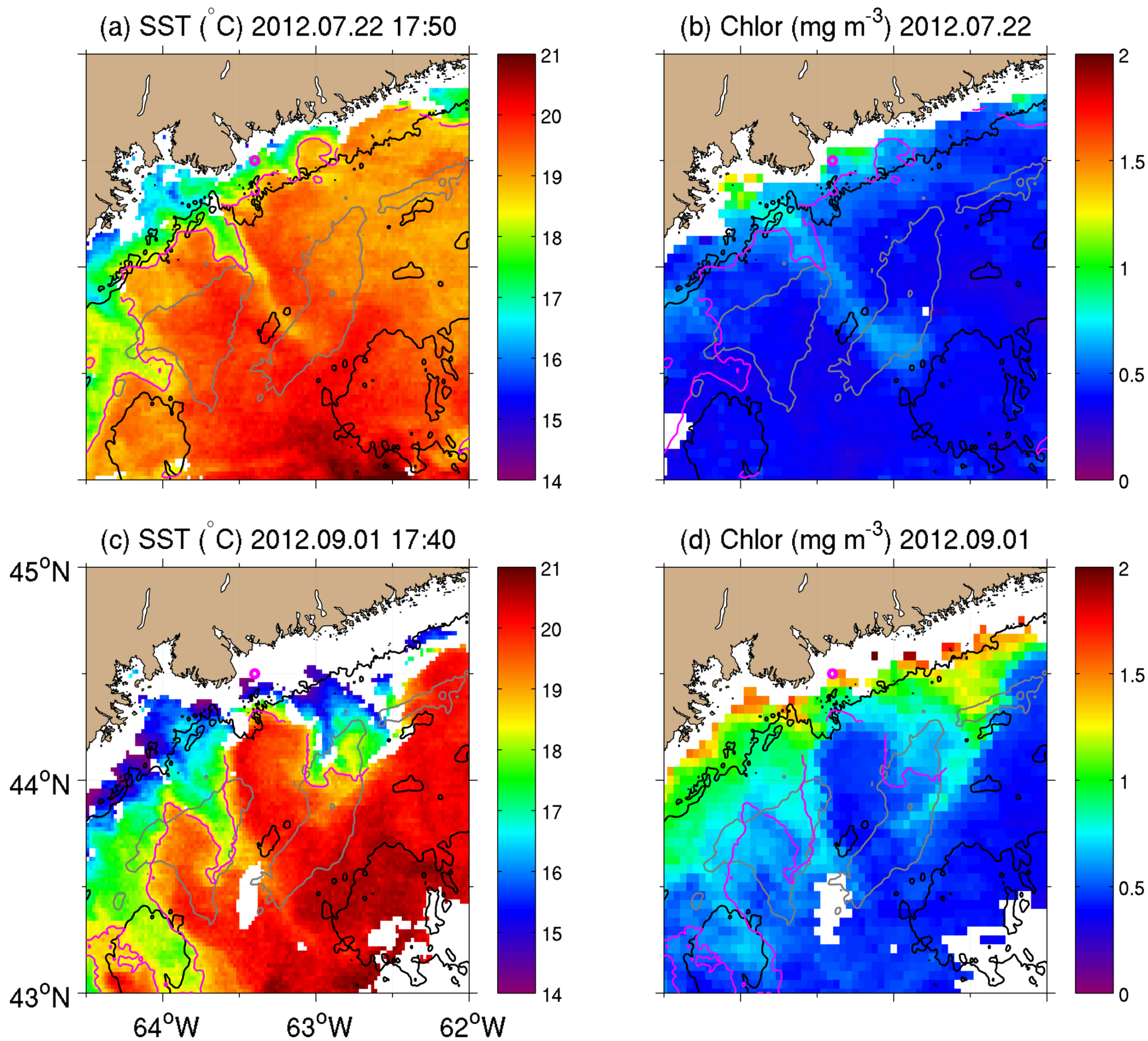

Figure 2: MODIS satellite remote sensing data of SST and Chlorophyll concentrations over the central Scotian Shelf and adjacent waters from July 22 and September 1, 2012 (from Shan 2016). Note that the shelf break is outside the frames. 100 m and 200 m isobaths are shown in black and gray contour lines, respectively.

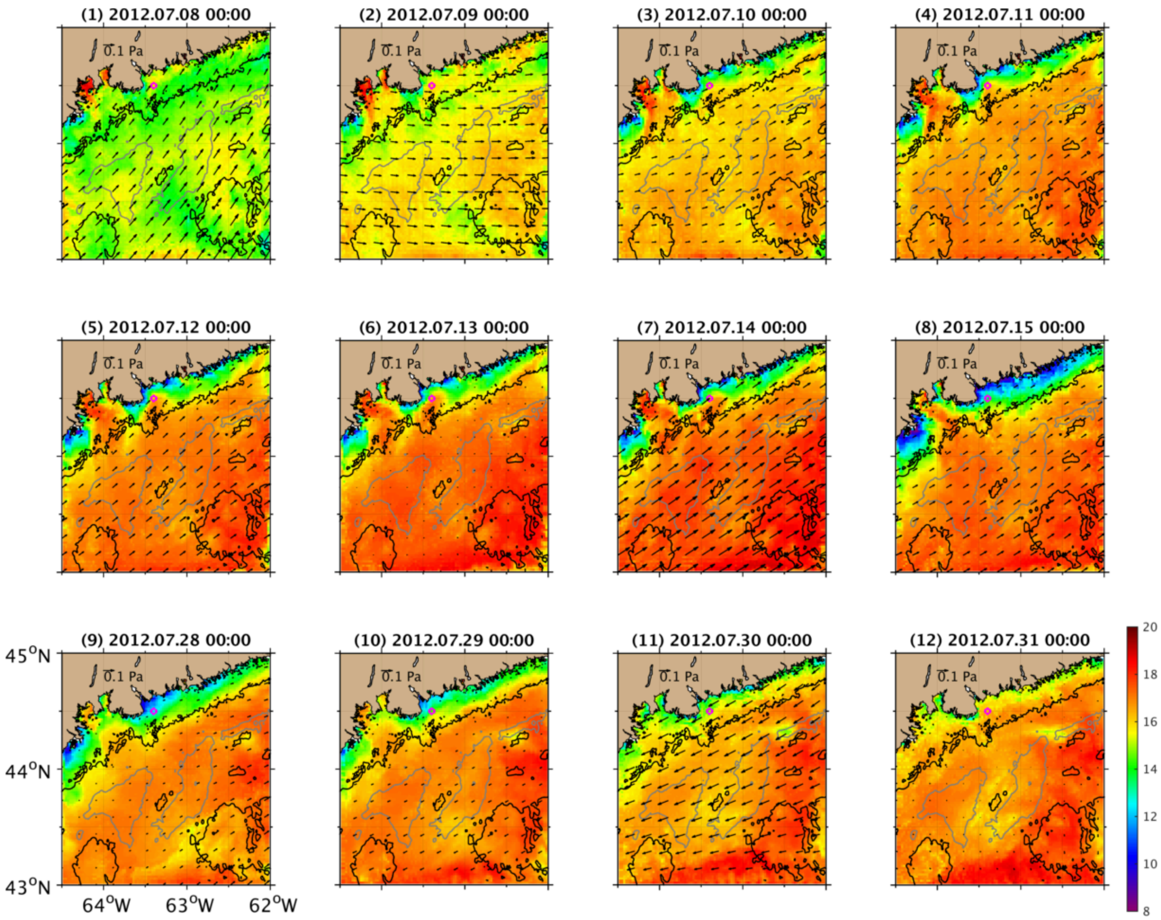

Figure 3: Snapshots of simulated SST over the central Scotian Shelf in July 2012 with instantaneous wind stress vectors plotting as black arrows (DalCoast-CSS model from Shan 2016).

These references also illustrate that some upwelling events create larger bands of upwelled water along the coastline, such as in Figure 1 and panel (c) shown in Figure 2. The intensity of the upwelling event and this the width of the coastal band of cold water is directly related to the strength and duration of the wind event the leads to upwelling. Hence upwelling bands are wider in some events that in others.

Our model does produce coastal upwelling events similar to those observed. We propose to add a figure into the supplement with a more detailed comparison of the modeled SST versus satellite SST to compare upwelling events and the model's skill at producing them.

References:

Petrie, B., B. Topliss, and D. Wright, Coastal upwelling and eddy development off Nova Scotia, *Journal of Geophysical Research*, 92, 12979-12991, 1987.

Shan, S. Eulerian and Lagrangian studies of circulation on the Scotian Shelf and adjacent deep waters of the North Atlantic with biological implications, PhD thesis, Dalhousie University, Halifax, NS, 2016.

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
Response: Indeed, this upwelling is very different from the upwelling in the CCS (see above comments).

Figure 7 – Please add other parameter time series to this plot including temperature, salinity and most important winds (both modeled and observed).
Response: We will add more parameters to this time series to the supplement.

Figure 8- More detail needs to be added to methods about how these comparisons were made.
Response: We will add more detail to the methods.

Figure 9 – Please add vandemark discussion to the text. What is the far right "section"?
Response: We can add Vandemark to the discussion. The rightmost section is a "merged" location as both Laruelle papers define a larger area and not solely the Scotian Shelf or Gulf of Maine. We will relabel this accordingly.

Finally, the title would be more informative if it were about the science question the paper is trying to address.
Response: We believe the Reviewer may have misunderstood our intended science question and hope this is clarified by the above responses.

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

Xu, Y.âY., Cai, W.âJ., Wanninkhof, R., Salisbury, J., Reimer, J., & Chen, B. (2020). Long-Term Changes of Carbonate Chemistry Variables Along the North American East Coast. Journal of Geophysical Research: Oceans, 125, e2019JC015982. https://doi.org/10.1029/2019JC015982