

Biogeosciences Discuss., author comment AC1
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Reply on RC1

Richard P. Sims et al.

Author comment on "Tidal mixing of estuarine and coastal waters in the western English Channel is a control on spatial and temporal variability in seawater CO₂" by Richard P. Sims et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-166-AC1>, 2021

Reviewer 1

The authors have collected a valuable data set that has the potential to support interesting new insights into the marine carbon biogeochemistry of this region. Some interesting and important points are raised along the way such as the discrepancy between a coastal ocean data product (Landschützer et al.) and the higher-resolution data collected here, and the non-negligible difference in seawater pCO₂ depending on the point in the tidal cycle that the sample was collected. But the overall narrative of this manuscript is not convincing. An important study could be written based on this data set but I do not think this manuscript hits the mark. I encourage the authors to reconsider the framing and methodology to get the most out of this data set with a different approach.

We thank the reviewer for their comments and insights.

We acknowledge the reviewers concerns about the framing of the manuscript and accept that the narrative needs to be slightly changed as not to overstate the findings. We have edited the title, abstract and conclusion to reflect a more toned down framing about the role of the tides on fCO₂. Lines 2, 22, 26, 382 and 423.

The reviewer suggests taking an alternate approach and to reconsider the methodology. This paper is not alone in using empirical relationships with salinity to derive the carbonate system in rivers. The decision to combine our relationship with the output of a hydrodynamic model is what allows us to map CO₂ in this unique way. The reviewer identifies many weaknesses in the approach taken in the paper which we do not dispute and which we clearly state. For these reasons we are reticent to make fundamental changes to the methodology. We have made changes to be more explicit in the weaknesses of the approach we have used and have quantified this with an uncertainty analysis Lines 317-322. We fully acknowledge there are avenues for improvement and we have already attempted to detail how we and our readers might overcome some of the limitations present in the study in the future.

Major concerns (in decreasing importance)

In essence, the approach taken is (1) calculate the differences in S and pCO₂ between each sampling point and station L4, denoted ξ_S and ξ_{pCO_2} , (2) draw a linear regression between ξ_S and ξ_{pCO_2} , (3) apply this linear regression to a regional model of S in order to map pCO₂.

- *What is the main control on seawater $p\text{CO}_2$?*

My main concern is that, contradicting the title of the study, tidal mixing of estuarine and coastal waters does not appear to be a particularly important control on spatial and temporal variability in seawater CO_2 .

The relationship between ξS and $\xi p\text{CO}_2$ shown in Figure 8 is essentially the proxy for this tidal mixing and the basis of the claim in the title. The first thing to remember is that this figure shows differences from station L4, but there is already a significant seasonal cycle in $p\text{CO}_2$ at L4, driven primarily by biological activity (lines 90–92), which is the main component of temporal variability. At this point, it already seems like the relationship in Figure 8 (tidal mixing) is a second-order control on $p\text{CO}_2$ overall. But even then, the relationship in Figure 8 explains only 21% of the variance in $\xi p\text{CO}_2$. So, 79% of the variability in $\xi p\text{CO}_2$ (i.e. variability in $p\text{CO}_2$ occurring over and above the 'background' variability in $p\text{CO}_2$ already present at L4) is not explained by tidal mixing. So I cannot see how the title and main conclusions can be justified. An interesting question would be, what is generating all that additional variability unexplained by tidal mixing? Can that also be predicted from variables in the regional model you used for a more accurate map? For example from Figures 3 to 7 it looks like some of the $p\text{CO}_2$ variability is more related to temperature rather than salinity, but it's hard to tell from these plots (scatter plots of S vs $p\text{CO}_2$ and T vs $p\text{CO}_2$ would be helpful for interpretation).

Furthermore, the '21% issue' above must lead to a very significant uncertainty in mapped $p\text{CO}_2$ values, which is not considered in the current manuscript. This uncertainty would be even further multiplied by the fact that there are quite some differences between the modeled and measured salinity values. The authors claim this agreement was 'good' but give an RMSE of ~ 1 (lines 289–290). The total range in ξS from the measurements is only just greater than 1 (see Figure 8) so I don't see how an RMSE of the same magnitude can be construed as good agreement. See further Figure S6 where the R^2 of the modeled vs measured ξS is only 0.32 and there is a quite considerable deviation from an ideal 1:1 gradient (slope is ~ 1.4 , offset ~ 0.1) which does not appear to be corrected for in the mapping.

The temporal changes at L4 are fully detailed in section 2 as the reviewer mentions. We agree with the reviewer that at least in the Tamar region tidal mixing is not the largest control on CO_2 and we have reiterated this in the text. Lines 284 and 388.

We will further speculate as to what could be driving the rest of the variability. Lines 390–392.

Not including an uncertainty analysis was an oversight, one is now included. Lines 317–322.

The reviewer has highlighted a small inaccuracy in the manuscript, the stats provided for the model salinity comparison were pertaining to the whole model domain and not the masked part of the model we use, the corrected stats have a higher R^2 and lower RMSE. Line 300.

As the model is using $f\text{CO}_2$ measured at L4 and ξS as opposed to absolute salinity, as long as relative salinity within the model is correct (which it appears to be in Figure 9), the absolute salinity in the model will not greatly affect the calculated CO_2 . Uncertainty in the absolute salinity is also due to boundary conditions coming from a larger model which is already stated Line 181.

- *Absence of uncertainty analysis*

One of the key motivators the authors state is reducing uncertainty in near-coastal air-sea CO₂ fluxes but there is no meaningful uncertainty analysis of the results produced. The final mapped pCO₂ values and air-sea CO₂ fluxes will all have substantial uncertainties propagated through from many factors including the original measurements (see point 5 below), variability not explained by the ξS regression (point 1 above), model-measurement S mismatch (point 1), gas-exchange coefficient and wind-speed averaging (point 4). For example Figure 12 definitely needs error bars or similar to interpret how meaningful the differences are. I would be surprised if the S-based correction from the L4 data is actually greater than the uncertainty (i.e. the green and blue points probably fall well within each others' uncertainty windows).

We have added an uncertainty analysis. Lines 317-322.

There are ongoing discussions e.g. (Woolf 2019) around how to best manage uncertainties in air sea flux calculations including using monthly wind speeds which we now reference Line 343.

- *Comparison with L4 and Landschützer et al. product*

There is a clear discrepancy between L4 data and the nearest point in the Landschützer et al. coastal data product (Figure 12) and no doubt some important points to be made there. But there are also other concerns with Figure 12 that are not really addressed.

Data for this study were collected only from June to September. It is striking that, in this time period, this study actually agrees relatively well with Landschützer et al., and it's the spring/winter months, for which the authors have no data to constrain their central relationship (Figure 8), where the biggest discrepancies are seen. I did not find this addressed. In fact we only know those winter months do deviate from Landschützer et al. because of the L4 data set which is not really the main focus of this study. Ultimately the 'correction' of L4 data using the Figure 8 relationship is relatively minor compared with the already-existing differences between L4 and Landschützer et al. In other words, what do the extra transect data presented here really add in this context, beyond what could already be said just by using L4 data?

The difference between the L4 data and our data is the component due to the tides and the river which is the focus of the paper and the one we wish to highlight as it has not been explored in depth before. The Landschützer product was provided to give context to the uncertainties in the coastal CO₂ data. We do not wish to distract from the focus of this paper by diverging into a lengthy discussion about the suitability of using such data products at stations like L4. This has caught the attention of both reviewers and is obviously of interest to the community and indicates there is need for a separate study addressing this.

- *Monthly wind-speed averaging*

Air-sea CO₂ fluxes were calculated with monthly wind-speed values 'to prevent wind speed variability overshadowing changes in the flux due to CO₂' (lines 322–323). Continuing on the theme of point 3 above, would this wind speed variability then be yet another factor that's more important than tidal mixing in controlling CO₂ dynamics here, yet is ignored by the method employed? It is really not clear why this decision would be taken. If you want to ignore the wind-speed effect isn't it better to just look at $\Delta p\text{CO}_2$, rather than calculating not-really-the-air-sea-flux? It also leads to inaccuracies if monthly mean U_{10} values are used and squared rather than first squaring U_{10} then calculating its monthly mean (not clear from the wording here which approach was used).

Yes absolutely, wind speed is a very large driver of the air sea flux on short timescales. It

was only possible to highlight the effect of the tides on the flux by using an average wind speed (otherwise the wind speed variability would have dwarfed the signal from the tides). There is a case for calculating the air sea flux, as it allows us to compare against other estimates. There are obviously some limitations that come with calculating the fluxes with CO_2 at three different temporal resolutions, as we plot the pCO_2 data from all three sources and are explicit about our methods we feel this is transparent. The reviewer is correct to identify that we should note that we first square the wind speed and then average (e.g. Monahan 2006). Lines 340-345

- *Showerhead vs membrane comparison*

It is noted that the comparison between the two different pCO_2 systems was reasonable at station L4 (RMSE 6.9 uatm) but very considerably worse elsewhere (RMSE 27.1 uatm) (lines 191–197). It is not explained why the quality of the comparison is so dependent upon sampling location nor is there discussion in this section of which sensor is better trusted.

A sentence has been added in the discussion explaining this explicitly. Lines 365-367