

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2021-166

Anonymous Referee #1

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Referee comment on "Tidal mixing of estuarine and coastal waters in the western English Channel is a control on spatial and temporal variability in seawater CO<sub>2</sub>" by Richard P. Sims et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-166-RC1>, 2021

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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 pCO<sub>2</sub> 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.

### Major concerns (in decreasing importance)

In essence, the approach taken is (1) calculate the differences in S and pCO<sub>2</sub> between each sampling point and station L4, denoted  $\xi_S$  and  $\xi_{pCO_2}$ , (2) draw a linear regression between  $\xi_S$  and  $\xi_{pCO_2}$ , (3) apply this linear regression to a regional model of S in order to map pCO<sub>2</sub>.

#### 1. What is the main control on seawater pCO<sub>2</sub>?

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<sub>2</sub>.

The relationship between  $\xi_S$  and  $\xi_{pCO_2}$  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 pCO<sub>2</sub> 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  $\sim 1$  (lines 289–290). The total range in  $\xi S$  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  $\xi S$  is only 0.32 and there is a quite considerable deviation from an ideal 1:1 gradient (slope is  $\sim 1.4$ , offset  $\sim 0.1$ ) which does not appear to be corrected for in the mapping.

## *2. Absence of uncertainty analysis*

One of the key motivators the authors state is reducing uncertainty in near-coastal air-sea  $\text{CO}_2$  fluxes but there is no meaningful uncertainty analysis of the results produced. The final mapped  $p\text{CO}_2$  values and air-sea  $\text{CO}_2$  fluxes will all have substantial uncertainties propagated through from many factors including the original measurements (see point 5 below), variability not explained by the  $\xi S$  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).

## *3. 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?

#### *4. Monthly wind-speed averaging*

Air-sea CO<sub>2</sub> fluxes were calculated with monthly wind-speed values 'to prevent wind speed variability overshadowing changes in the flux due to CO<sub>2</sub>' (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 CO<sub>2</sub> 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).

#### *5. Showerhead vs membrane comparison*

It is noted that the comparison between the two different  $p\text{CO}_2$  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.