Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-212-AC2, 2018

Interactive comment on "Drivers of future seasonal cycle changes of oceanic pCO_2" *by* M. Angeles Gallego et al.

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Received and published: 3 July 2018

We are thankful for the referee's comments. The referee's specific comments helped with the revision of our calculations and improved the manuscript.

Comment 1: In this study, the authors assess future changes in the seasonal cycle of surface ocean pCO_2 using simulations from 7 different CMIP5 Earth system models subjected to RCP8.5 forcing. A Taylor series decomposition approach is used to identify the important drivers of pCO_2 seasonality and its future changes. The authors find that the pCO_2 seasonal amplitude will increase by a factor of 1.5 to 3 by the end of the current century. The primary cause of this increase is the increase in ocean mean pCO_2 (a response to increasing anthropogenic emissions), which enhances the pCO_2 seasonal variation occurring

in response to seasonal variations in temperature (T) and dissolved inorganic carbon (DIC). Changes in T and DIC seasonality at high latitudes are also relevant for understanding the model-simulated changes in pCO_2 seasonality. This is a nice study that complements some recent work (e.g., *McNeil and Sasse* (2016); *Landschützer et al.* (2018); *Kwiatkowski and Orr* (2018)) examining the changing seasonality of ocean carbonate chemistry variables over recent decades and in the future. The paper is generally clear, well written and logically organized, and the scientific methods are sound. However, I do strongly agree with Referee 1's assessment that the authors should do a better job of placing their results in the context of previous work. While this is done to some extent already and, in all fairness, the authors certainly cite the relevant literature it tends to get a bit lost in the discussion and it's often a little unclear which results are novel and which simply confirm previous findings. It could be helpful to add a separate Discussion section before the Summary and Conclusions in which results from the current study are compared and contrasted with those from previous studies. In addition to this, I've included several specific comments and technical corrections below for the authors to consider. I feel that a suitably revised version of the manuscript - addressing the points raised here - should be publishable in Biogeosciences.

Response: We revised the "Results and discussion" section, where we compared and contrasted our results with the existing literature. We added several changes in this section based on referee 1 and 2 comments. Most of the changes were discussed in the response to referee's 1 comments. In what follows we address referee 2's specific comments:

Comment 2: p. 5, lines 19-20: "*McNeil and Sasse* (2016) using a data-based approach" It would be good to clarify what you mean here by a "data-based approach".

Response: We changed to "*McNeil and Sasse* (2016) used observations and a neural-network-clustering algorithm to project that by year 2100..."

Comment 3: p. 5, lines 23-25: "Using observations *Landschützer et al.* (2018) found..." I have two issues with this sentence. First, it's unclear to me where the mean 20 muatm increase by the end of the century comes from; the values given earlier in this paragraph (see also Fig. 1) are significantly larger than this (e.g., 41 muatm increase between 40°S-40°N). Second, I wouldn't expect the rate of change of pCO_2 seasonality in observations to match that in the CMIP5 models in the RCP8.5 simulations, since many of the important drivers of pCO_2 variability (e.g., atmospheric CO_2) are changing at much faster rates in the latter than they are in the former.
Response: We did three changes based on this comment: We corrected the calculation and the sentence was changed to: " Using observations, *Landschützer et al.* (2018) found an increase of 2.2 μatm per decade, which is smaller than our findings of a total 42 μatm increase by the end of the century between 40°S-40°N, and a global-mean change of 81 μatm on the high latitudes. This difference is again possibly due the higher mean pCO_2 values in models than observations. " The discussion of the difference between models and observations was addressed in the response to Referee 1, Comment 15.

Comment 4: Fig. 3/Fig. S1: These figures show (among other things) that the Taylor expansion generally does a good job in reproducing the actual pCO_2 calculated from model output. However, there seems to be an inconsistency between the two figures. Specifically, Fig.3 suggests that the Taylor expansion slightly overestimates the seasonal amplitude of pCO_2 (this is most evident for the 40°S-70°S latitude band), while Fig. S1 suggests exactly the opposite: an underestimation of the seasonal amplitude.

Response: The labels of the S1 figure's axis were switched (x-axis label was y-axis, and vice versa). Figure S1 was corrected.

Comment 5: p. 7, line 15: "decrease in the future to a global mean value of 0.035" .This number seems to be too small looking at Fig. 4c (middle column).
Response: There was an error in the calculation. The sentence was changed to: "This value agrees with our global mean ensemble estimate of 0.0428. However, our analytical expression of γ_T shows that this value varies regionally and, by reasons unknown to us, it might decrease in the future to a global mean value of 0.0415, (Fig. 4, row (c), third column). "

Comment 6: p. 7, line 26: "with lower temperatures in winter and higher in summer" It might be good to clarify here that you do not mean lower temperatures in an absolute sense (i.e., winter temperatures are certainly projected to be higher at the end of century under RCP8.5 than they are at present).
Response: This sentence was changed and we added some context: "All models show a slight increase in δT, only one model showed a slightly decrease in the southern region, and two models showed a decrease in the equatorial region during October to December. It is important to note that Fig. 5 shows the seasonal values, with the mean T removed. Therefore, when considering the positive T trends, the absolute summer values show an increase and the winter values a decrease. This agrees with the results of *Alexander et al.* (2018); who showed that models project a seasonal intensification of T, with larger warm extremes and reduced cold extremes. The authors attributed the T seasonality intensification to an increased oceanic stratification and an overall shoaling of the mixed layer depth, which confines seasonal changes in a reduced volume of water, producing larger changes at the surface. They

also showed that the intensification trends are stronger in summer than winter, as the mixed layer depth is shallower in summer. Moreover, ice covered regions will experience the largest increase in T seasonality, as the ice melting/freezing modulates the surface water temperature (*Carton et al.*, 2015). "

Comment 7: p. 8, lines 26-27: "we decomposed the DICs and T contributions..." I only see the seasonal cycle and mean pCO_2 components in Fig. 6b, not the sensitivity component.

Response: We changed the sentence to: "To further disentangle which of the two main drivers (DIC_s or T) is most affected by $\Delta\overline{pCO_2}$, we decomposed the DIC_s and T contributions in their sensitivity, seasonal cycle and $\overline{pCO_2}$ components. Figure 6, (b), shows the total DIC and T components and the $\Delta\overline{pCO_2}$ and seasonal cycles effects. The effects from the sensitivities are not depicted, as they only play a minor role. Only the $\Delta\gamma_{DIC}$ term gains importance in the Southern Ocean (not shown). "

Comment 8: Technical corrections: 1) p. 1, line 2: Should be a rate of 2-3 muatm per decade?.

Changed to decade.

2) p. 3, line 5: "Methodology" misspelled.

Corrected.

3) p. 3, line 14: Should be scarce?.

Changed to "scarced".

4) p. 7, line 11: Should probably remove the word "change" here, since the annual cycle amplitude change is actually 168 muatm minus 96 muatm (i.e., 72 muatm).

We removed the word "change" and changed the sentence to: "therefore, for a $\overline{pCO_2}$

equal to 800 μatm, the δpCO_2 amplitude due to δDIC amounts to 168 μatm. "

5) p. 7, line 15: Should be "row (c)".

Changed to "row c)"

References

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