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

Anonymous Referee #1

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Summary:

Gallego and co-authors investigate the magnitude and drivers of the seasonal changes in the sea surface pCO2. They use 7 CMIP5 models and compare the changing seasonality between 2 periods in the early 2000s and at the end of the coming century using the RCP8.5 scenario. The authors perform a Taylor expansion to investigate the relative contributions of the individual drivers (T, S, DIC, TA) as well as their changing sensitivities. The authors conclude that the seasonal pCO2 cycle will intensify by a factor of 1.5-3% by the end of the century, mainly owing to the sensitivity of the pCO2 cycle to changes in DIC and T, with both terms counteracting each other.

Strengths:

The changing seasonality in the surface ocean pCO2 and its potential impact on ocean acidification and marine life has recently received a lot of attention. More and more evidence emerges that the excess uptake of CO2 by the oceans will lead to environmental stress conditions, which will emerge earlier in time due to the seasonal pCO2 and pH amplification. The authors present here an extensive analysis building on state-of-the-art modelling output to estimate how strong the CO2 amplification is expected to be by the end of the century and what the main drivers of this amplification are. In my view, one strength of the conducted analysis is, that it nicely bridges between 2 recently published studies by Landschutzer et al 2018 and Kwiatkowski et al 2018 (both cited in the main text), hence I do believe the study has its place in the current literature and the results will be of interest to experts and the wider BG readership.

Weaknesses:

Unfortunately, while bridging between the current literature is the strong point of the presented manuscript, it also reveals its strongest weakness. On many occasions the authors fail to clearly highlight what is novel about their analysis and what has been previously shown. While the authors do give credit e.g. to the Landschutzer et al and Kwiatkowski et al studies at some place in the text (hence they must have read them), they fail to discuss their results in context to what is already known by these other studies. In some cases, the authors even create the impression that conclusions drawn here are novel, whereas they have been highlighted in other studies. To name the concrete examples:

.) Page 6 lines 1-2: "In general, towards the end of the century pCO2 amplifies more in high latitudes, . . ." this is the same result as for the past ears based on observational data (Landschutzer et al 2018, Figure 4) and for the future pH as a direct consequence of CO2) (Kwiatkowski et al 2018, Figure 3)

.) Page 9 lines 6-7: "We demonstrate that on average the global amplification of pCO2 is due to the overall longterm increase of anthropogenic CO2." This is the same conclusions Landschutzer et al 2018 reached based on examining trends in amplitude over the past 30 years, yet this is nowhere indicated. It is still a valuable result considering the focus of the study being the coming century, but it needs to be highlighted that other studies derive to the same conclusion.

.) Page 9 lines 11-12: "Our results extend and refine the current views, in which the future amplification has been attributed uniquely to the DIC sensitivity" – This is not correct. Both Landschutzer et al and Kwiatkowski et al discuss the attribution of other terms as well. The authors even briefly mention this in their introduction page 2 line 32: "Current literature suggests that the seasonal amplification is a consequence of an increase on the T and DIC contributions to pCO2 (Landschutzer et al 2018) ..."

.) Page 9 lines 17-19:"The first complete analytical Taylor expansion of pCO2 in terms of the variables DICs, TAs, T and S showed that DICs and T contributions are the main counteracting terms to control the pCO2, both under present-day and future conditions. The prevalence of one term over the other in various regions remains similar, even under enhanced CO2 conditions" – This has also been shown by Landschutzer et al 2018 under past/present conditions, yet again this is not mentioned anywhere. Furthermore, by stating "The first complete Tayler expansion . . ." I suppose the authors mean within their own study, yet it created the impression that the authors refer to the first complete Tayler expansion overall, whereas, e.g. Kwiatkowski et al use the same Tayler expansion in their analysis.

.) Page 9 lines 23-26: "Spatially, we found that the magnitude of the contributions depends on the mean pCO2 , its local sensitivities (DIC,TA,T,S) and the amplitude of their seasonal cycles ((DIC,TA,T,S)). The phases depend on the regional characteristics of the seasonal cycles and they moderate the counteracting nature of both contributions. The compensation of DICsÂă and T contributions is most effective when they are six months out of phase." This mirrors again a conclusion drawn in Landschutzer et al 2018 (see e.g. Figure 3 in their study), whereas a comparison, discussion or even mentioning of this circumstance is missing here. Also regional characteristic have been

discussed by Landschutzer et al 2018 and in terms of pH by Kwiatkowski et al 2018.

.) Another important result is only "hand wavy" introduced, namely that TA and S play a lesser role in the future pCO2 cycle amplification. One of the weak points of the Landschutzer et al 2018 study is that the authors ignore e.g. TA contributions, yet this study suggests that is of minor concern even when evaluating the century-long seasonal amplification. The authors also discuss second order terms here that have not been introduced in Landschutzer et al 2018 or Kwiatkowski et al 2018, but this is also not mentioned/compared.

.) Very interesting regional differences occur between the observation-based assessment of Landschutzer et al 2018 and this study, that are not discussed at all. Landschutzer et al find a DIC dominance in the high latitudes of both hemispheres, whereas the model based study suggests a T dominated increase in the high latitude northern hemisphere. Is this due to a model bias in seasonality. Is this the same across all models?

Recommendation:

The authors have conducted an extensive, interesting and certainly valuable analysis using state-of-the-art model outputs. Their methods are sound and their results nicely fit alongside the existing literature. The lack of discussion with the existing literature, however, is of major concern, particularly that the authors fail to acknowledge similar studies coming to the same conclusions. If the authors were to revise their manuscript and discuss their results in a fair way considering the existing literature, I believe this study can be considered for publication. The revisions however will affect the text throughout, hence I recommend major revisions of the manuscript.

Specific and minor comments to the text:

Abstract line 1: "observations" – its observation-based

Introduction page 1 line 22: a third of the anthropogenic CO2 produced by fossil fuel burning, cement production and deforestation since the industrial revolution" – the cited Sabine study suggest 48% since the beginning of industrialization. The referenced 1/3 refer to the annual uptake as stated in the second study cited, namely the Le Quere et al carbon budget.

Page 2 line 21: [CO2(aq)]is introduced.ÂăFor the non carbonate seawater chemists that read BG it would be helpful to explain the difference between [CO2] and [CO2(aq)]

Page 4 line 11 and Supplement figure S1: The comparison between individual models gets worse in the high latitudes. Any idea why? The high latitude northern hemisphere is also wher this study differs from the observation-based analysis of Landschutzer et al 2018.

Page 4 line 20, equation 3 and following: the delta terms also represent the mean seasonal cycle over 20 years (period 1 or period 2) hence they should have also an overbar (like the pCO2) for consstency.

Page 5 line 14: "The range agrees with previous estimates by Takahashi et al. (2002)." Please add the comparison (visual or in table form), e.g. in the supplement for the readers of this study. Otherwise the reader has to jump around several different manuscripts for a simple comparison.

Page 5 line 21: "Our mean amplification factor estimation agrees with the lower end range of (McNeil et al. , 2016)." – Please add numbers for the reader of this study.

Page 6 lines 8-9: "Our estimated contributions from DICs and T to the present day pCO2 are in good agreement with the data based estimates (Takahashi et al. , 2002; Fay et al., 2017)." Please add a visual comparison or numbers for the readers of this study.

Page 7 lines 6-7: "DIC must not be confused with the Revelle factor, which is defined as R = DIC x gamma DIC." – this statement comes a bit out of the blue and while true it is not clear to me why it appears here. Based on the equations/wording used in this

study I don't see the danger that these terms are mixed up.

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