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Discussions

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Interactive comment on "Drivers of future seasonal cycle changes of oceanic pCO$_2$" *by* M. Angeles Gallego et al.

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We thank the referee for reviewing the manuscript and for giving insightful and detailed comments that helped to improve our manuscript noticeably.

Comment 1:
The changing seasonality in the surface ocean pCO$_2$ 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 CO$_2$ by the oceans will lead to environmental stress conditions, which will emerge earlier in time due to the seasonal pCO$_2$ and pH amplification. The authors present here an extensive analysis building on state-of-the-art modeling output to estimate how strong

the CO_2 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 *Landschützer et al.* (2018) and *Kwiatkowski and Orr* (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.

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 *Landschützer et al.* (2018) and *Kwiatkowski and Orr* (2018) 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:

Response: We revised large part of the manuscript to properly identify which findings are novel and which ones already exists in the current literature.

We added two supplementary figures: Fig. S3 that shows a comparison of pCO_2 seasonal amplitude by *Landschützer et al.* (2017) and *Takahashi et al.* (2014), as well as their thermal and non-thermal components. Fig. S4 shows a comparison of summer-minus-winter pCO_2 amplitude between models, for 2006-2026 and 2080-2100 periods. The figures are shown in the supplement of our response.

Below we address the specific referee's comments. Subsequently, we list other changes that were made to the manuscript, as well as references added.

Comment 2: Page 6 lines 1-2: "In general, towards the end of the century pCO_2 amplifies more in high latitudes, . . .". This is the same result as for the past years based on observational data ((*Landschützer et al.*, 2018), Figure 4) and for the future pH as a direct consequence of CO_2 ((*Kwiatkowski and Orr*, 2018),

Figure 3)).
Response: We changed the sentence to: " In general, towards the end of the century the pCO_2 amplifies more in high latitudes, but so does the standard deviation uncertainty among models. This regional pattern agrees with the observation-based findings of *Landschützer et al.* (2018) which show that high latitudes have already experienced a larger amplification than mid-low latitudes from 1982 to 2015. Furthermore, the same pattern is projected by CMIP5 models for the seasonal amplification of $[H^+]$ by the end of the century (*Kwiatkowski and Orr*, 2018). This is expected from the near-linear relation between pCO_2 and $[H^+]$."

Comment 3: Page 9 lines 6-7: "We demonstrate that on average the global amplification of pCO_2 is due to the overall longterm increase of anthropogenic CO_2". This is the same conclusions *Landschützer 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.
Response: We changed the sentence to: "The projected amplification by the earth-system models and the possible causes of it, are consistent with observation-based amplification for the period from 1982 to 2015 (*Landschützer et al.*, 2018). In agreement with the observational results, also the model projections towards the end of this century demonstrate that the global amplification of δpCO_2 is due to the overall longterm increase of anthropogenic CO_2. A higher oceanic CO_2 concentration enhances the effect of solubility changes on δpCO_2 and alters the seawater carbonate chemistry, also enhancing the DIC seasonality effect."

Comment 4: 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 *Landschützer et al.* (2018)and *Kwiatkowski and Orr* (2018) 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 (*Landschützer et al.*, 2018)..."
Response: We agree, we removed the sentence.

Comment 5: Page 9 lines 17-19: "The first complete analytical Taylor expansion of pCO_2 in terms of the variables DICs, TAs, T and S showed that DICs and T contributions are the main counteracting terms to control the pCO_2, both under present-day and future conditions. The prevalence of one term over the other in various regions remains similar, even under enhanced CO_2 conditions". This has also been shown by *Landschützer et al.* (2018) under past/present conditions, yet again this is not mentioned anywhere. Furthermore, by stating "The first complete Taylor expansion . . ." I suppose the authors mean within their own study, yet it created the impression that the authors refer to the first complete Taylor expansion overall, whereas, e.g. *Kwiatkowski and Orr* (2018) use the same Taylor expansion in their analysis.
Response: By "first complete analytical Taylor expansion, "we refer to the incorporation of T and S analytical terms, and therefore it is the first complete with analytical expressions in the four terms, which - to our knowledge- has not been done before. However, we agree this might be misleading, so we changed the sentence to: "The models confirm the well-established mechanisms controlling present-day δpCO_2 (*Takahashi et al.*, 2002; *Sarmiento and Gruber*, 2006; *Fay and McKinley*, 2017). DIC_s and T contributions are the main counteracting terms dominating the seasonal evolution of δpCO_2. Furthermore, the models show that under future conditions

the controlling mechanisms remain unchanged. This result confirms the findings of *Landschützer et al.* (2018) that identified the same regional controlling mechanism for the past 30 years. The relative role of the DIC and T terms is regionally dependent. High latitudes and upwelling regions, such as the California Current system and the coast of Chile, are dominated by DIC_s and the temperate low latitudes are driven by T. Only in the North Atlantic and North-Western Pacific the models show a dominance of thermal effects over non-thermal effects, which is in disagreement with observations. This further illustrates the urgent need for models to accurately represent regional oceanographic features. " The discussion on the difference between models and observations was added in the results and discussion section.

Comment 6: Page 9 lines 23-26: "Spatially, we found that the magnitude of the contributions depends on the mean pCO$_2$, 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 DICs and T contributions is most effective when they are six months out of phase." This mirrors again a conclusion drawn in *Landschützer 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 *Landschützer et al.* (2018) and in terms of pH by *Kwiatkowski and Orr* (2018) .
Response: The sentence was changed to: "Moreover, the pCO$_2$ seasonal cycle amplitude depends on the relative magnitude and phase of the contributions. Spatially, we found that the magnitude of the contributions depends on the mean pCO$_2$, its local sensitivities ($\gamma_{DIC,TA,T,S}$) and the amplitude of their seasonal cycles ($\delta(DIC, TA, T, S)$). The phases depend on the regional characteristics of the seasonal cycles and they moderate the counteracting nature of both contributions. The ensemble mean reproduces the highly effective compensation of DIC_s and T contributions

when they are six months out of phase, confirming previous studies (*Takahashi et al.*, 2002; *Landschützer et al.*, 2018)."

Comment 7: 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 *Landschützer 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 *Landschützer et al.* (2018) or *Kwiatkowski and Orr* (2018) , but this is also not mentioned/compared.
Response: We added in the conclusions, page 9, line 21: "The TA and S terms have a small impact in most regions, except on the high latitudes where the TA contribution complements the DIC one, enhancing the non-thermal effect in this region. Interestingly, in the high latitudes, the amplification through second order terms are as important as the change in the seasonality of the drivers. Their high values arise from changes in mean pCO_2 acting over the changing T seasonality. "

Comment 8: Very interesting regional differences occur between the observation-based assessment of *Landschützer et al.* (2018) and this study, that are not discussed at all. *Landschützer et al.* (2018) 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?.
Response: We added in the "Results and discussion" section, page 6, line 12: "The models show that the δpCO_2 in the 40°N to 60°N band is controlled by T, which disagrees with the above mentioned observations that show a non-temperature dominance in this band. The difference between models and observations arises from two regions: the North Atlantic basin and the North Western Pacific; specifically near

the Oyashio Current, and the outflows from the Okhotsk Seas (see Supplementary Fig. S3). Most models show a T dominance in the North Atlantic basin; only CESM1-BGC and GFDL-ESM2M show a DIC dominance (see Supplementary Fig. S4). The North Atlantic is one of the major sinks of anthropogenic CO_2, however some models fail to estimate its uptake capacity (*Goris et al.*, 2018). *Goris et al.* (2018) found that models with an efficient carbon sequestration present a DIC-dominated pCO_2 seasonal cycle in the North Atlantic, but models with low anthropogenic uptake show a T dominance in this region. In the North-Western Pacific, *Mckinley et al.* (2006) found that coarse models are not able to capture the intricate oceanographic features of this area, and therefore the pCO_2 seasonality is not well captured."

Comment 9: 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.

Response: As suggested by the referee, we have done a major revision of the manuscript. We thank again for the suggestions that helped to improve the manuscript; we placed our results and their relevance among the current literature and compared/contrasted our findings which previous results, in particular those by *Landschützer et al.* (2018).

Comment 10: Abstract line 1: "observations" its observation-based
Response: Changed to "observation-based results"

Comment 11: Introduction page 1 line 22: a third of the anthropogenic CO_2 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.
Response: We changed it to: "the ocean has absorbed nearly half of the anthropogenic CO_2 produced by fossil fuel burning and cement production since the industrial revolution (*Sabine et al.*, 2004)"

Comment 12: Page 2 line 21: [CO2(aq)]is introduced. For the non carbonate seawater chemists that read BG it would be helpful to explain the difference between [CO2] and [CO2(aq)]
Response: We changed it to: " This is due to the ability of CO_2 to react with seawater to form bicarbonate $[HCO_3^-]$ and carbonate $[CO_3^{2-}]$, leaving only a small portion of the dissolved carbon dioxide in the form of aqueous CO_2 ($[CO_2(aq)]$). $[CO_2(aq)]$ together with the carbonic acid ($[H_2CO_3]$) are defined as $[CO_2]$. Therefore, it is useful to define the total amount of carbon as DIC, which is the sum of the three carbon species ($[HCO_3^-]$, $[CO_3^{2-}]$ and $[CO_2]$)."

Comment 13: 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 where this study differs from the

observation-based analysis of *Landschützer et al.* (2018).
Response: In this figure we compare the pCO_2 amplification calculated as model output with the value from the Taylor expansion. The Taylor expansion is less precise in higher latitudes, probably because second order terms gain importance. The difference with *Landschützer et al.* (2018) was addressed in comment 8.

Comment 14: 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 consistency.
Response: We leave the nomenclature as it is, as by "mean" we refer to the mean value of the data, instead of the deviation of the mean, which is the seasonal cycle.

Comment 15: 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.
Response: We added a supplementary figure S3, for better comparison with data from *Takahashi et al.* (2014), for a reference year 2005 and with *Landschützer et al.* (2017). We also added at page 5, after line 14 : "The ensemble mean initial seasonal amplitude range is in good agreement with observational estimates calculated for the reference year 2005 (*Takahashi et al.*, 2014), and for the 1982-2015 period (*Landschützer et al.*, 2017). The agreement between models and observations is remarkably good in the equatorial regions, but the initial amplitude is slightly overestimated in the mid and high latitudes (see Supplementary Fig. S3).The higher amplitude in models than observations is expected, as the initial period 2006-2026 already experienced an amplification compared to previous years. Moreover, *Tjiputra et al.* (2014) found

that the ocean's pCO_2 historical trend is larger in models than observations when it is estimated in large scale areas of the ocean. However, they found that models' pCO_2 trends agree with observations when the trends are subsampled to the locations where the observations were taken, and therefore they do a good job reproducing well-known time series. Moreover, differences are expected as *Pilcher et al.* (2015) suggested that CMIP5 models perform well in reproducing the seasonal cycle timing, but still show considerable errors in reproducing the seasonal amplitude of pCO_2 due to differences in the mechanisms represented in each model, especially in subpolar biomes. "

Comment 16: Page 5 line 21: "Our mean amplification factor estimation agrees with the lower end range of *McNeil and Sasse* (2016)." Please add numbers for the reader of this study.
Response: We changed this sentence to: " Our mean amplification factor estimation agrees with the threefold amplification found for most of the ocean by *McNeil and Sasse* (2016)."

Comment 17: Page 6 lines 8-9: "Our estimated contributions from DICs and T to the present day pCO_2 are in good agreement with the data based estimates (*Takahashi et al.*, 2002; *Fay and McKinley*, 2017)." Please add a visual comparison or numbers for the readers of this study.
Response: Instead of comparing with *Takahashi et al.* (2002), and *Fay and McKinley* (2017), we used the dataset of *Takahashi et al.* (2014) and calculated thermal and non thermal components for year 2005. We also added a comparison with the thermal and non-thermal components for years 1982-2015 that Peter Landschützer kindly provided to us. The results are shown in Supplementary Figure S3, and the discussion

was added in section 3.2: "For most of the ocean, the ensemble mean estimated contributions from DIC_s and T to the present-day δpCO_2 are in good agreement with the data-based estimates of *Takahashi et al.* (2014); *Landschützer et al.* (2017), particularly in the equatorial regions (see Supplementary Fig. S3). However our temperature and DIC contributions are slightly larger in mid and high latitudes, for the same reasons the pCO_2 seasonal amplitude is overestimated (see Section 3.1). Also, differences arise between our DIC_s contribution and the observation-based so called "non-thermal" contribution, because the non-thermal contribution also includes the total alkalinity and salinity effects. Nonetheless, between 40°S-40°N our ensemble mean shows that δpCO_2 is dominated by changes in temperature that control CO_2 solubility, which decreases in summer enhancing pCO_2, in agreement with observations. The Southern Ocean is controlled by DIC, that responds to changes in upwelling and phytoplankton blooms. Both mechanisms act together to decrease (increase) DIC in summer (winter) (*Sarmiento and Gruber*, 2006). " The discussion of northern high latitudes is added in comment 8.

Comment 18: 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.
Response: The Revelle factor and the sensitivity are different, and sometimes confused. We included the relationship because *Takahashi et al.* (1993) computed the Revelle factor. This sentence was rearranged as: "This follows the approach of *Takahashi et al.* (1993), however instead of computing the Revelle factor we use γ_{DIC}, both terms are related by $R = DIC \cdot \gamma_{DIC}$."

Other changes added:

- page 6, line 26 we added: " In this region some models underestimate the pCO_2 trend (*Tjiputra et al.*, 2014), and therefore the seasonal amplification might be underestimated too."

- page 7, line 1, we added: "Lower buffer factors (higher sensitivities factors) are found in regions where DIC and TA have similar values, and they will decrease (increase) as the DIC/TA ratio in the oceans increases (*Egleston et al.*, 2010). "

- page 7, line 7, we added: "$\gamma_{S_{fw}}$ decreases everywhere except in the Western Pacific Warm Pool. In this region $\gamma_{S_{fw}}$ increases probably due future changes in precipitation that enhance the fresh-water effect."

- page 7, line 33 was changed to: "*Kwiatkowski and Orr* (2018) demonstrated that the seasonality of the drivers is important to determine future changes in $[H^+]$ seasonality. In the same fashion, our results show that the four δpCO_2 drivers present changes in seasonality, and in particular δDIC_s and δT changes are important to explain future projections of the δpCO_2 amplitude."

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