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Biogeosciences
Discussions

Interactive comment on "A mechanistic account of increasing seasonal variations in the rate of ocean uptake of anthropogenic carbon" *by* T. Gorgues et al.

T. Gorgues et al.

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We are thankful to both reviewers for their thoughtful comments on the manuscript. In responding to the reviews, be believe that the manuscript has been strengthened, and the main points are now more clearly presented. The revised manuscript is attached as supplement.

In particular, we appreciate the long and detailed comments of the second reviewer. The reviewer stated clearly that he/she agreed with our main interpretation of our model sensitivity studies, but that we were not adequately addressing either (a) the application of these results for interpreting observations, or (b) the implications for the extension of

the ocean carbon observing system. These are important points, and we have given careful consideration to being clear in addressing them.

Regarding the interpretation of existing observations, we have now clearly stated that the mechanisms emphasized here are sufficiently large to make a first-order contribution to some of the surface ocean pCO2 transients reported in the literature. And that these changes occur in the absence of any interannual or decadal changes in the physical state of the ocean. In fact the entire purpose of this study was to specifically provide a quantitative estimate of the amplitude of this contribution. Certainly, there are other mechanisms that can impact sea surface pCO2, and understanding the interplay between these mechanisms will clearly involve detailed work beyond the scope of this paper.

As for the implications for the extension of the existing observing system, we have now clearly recommended that sea surface pCO2 measurements resolve the seasonal cycle over large scales. According to the model, CO2 uptake estimates that rely exclusively on summer measurements would under-estimate the rate of CO2 uptake by the ocean by 0.6 PgC.yr-1 during the 1990s. This is an important point, and we thank the reviewer for suggesting to us to emphasize this issue.

In the text that follows, we give more specific responses to the comments of the two reviewers.

"Reviewer comment"/Authors comment

"Response to reviewer #1: The analysis might be strengthened by an alternative choice of methodology for Figure 4. If the authors kept everything constant and only varied the one parameter (e.g. LeQuere et al. 2003, McKinley et al. 2004), then they could see the impact of only DIC, only ALK, etc. Instead, they show with MALK that seasonality in ALK is NOT important. The result would be stronger evidence for their conclusions – for example line 16 on page 753. They do make their point with the figures they show. At a minimum, some explanation for this choice is needed."

Reviewer #1 is probably talking about figure 3 (only DIC is considered in figure 4). The methodology used in our study is equivalent to the one suggested by the reviewer. Either you use the annual mean of all the fields except one which varies seasonally (reviewer suggested method) or you use seasonally varying of all fields except one which is kept at its annual mean (method used in our study). The effect of the seasonality of the considered field is addressed with both methodologies. The reason we chose to have all seasonally varying fields except one that is kept at its annual mean is to allow comparison with our reference simulation that is using all seasonally varying fields.

"Specific comment: 1. Reference Shuster et al. (2009) in DSR II. It is essentially an update of Shuster and Watson 2007. Also Watson et al. (2009) in Science."

done

"2. Figure captions. Add the word "modeled" to clarify."

done

"3. Figure 3 is too small in the PDF version. The four panels of Figure 4 are OK. Can Figure 3 be split into two pieces like this?"

Figure 3 corresponds to the processes analyses, so it would be good if we can keep it one block. However, the authors agree that the figure appears too small in the manuscript available at the Biogeosciences Discussion website. Maybe, during the final process of publication, the figure could be printed in the portrait mode? If this is not possible, we will split the figure in 2 blocks.

"Response to reviewer #2: General comments:

In this study authors use a 3D ocean circulation model to investigate the seasonal trends of ocean pCO2 (or DpCO2) over 30 years (1970-2000). In order to evaluate the processes that control the seasonal changes of anthopogenic pCO2 trends, they use a steady seasonal dynamical forcing. This represents an interesting sensitivity analysis, especially regarding previous simulations (using the same model but different forcing,

Rodgers et al 2008). Authors conclude that the seasonal trends of DpCO2 is explained by a complex coupling between sea surface temperature and trends of anthropogenic DIC. This conclusion (from the simulations) is correct but how this offers new findings and impact our knowledge on the ocean carbon cycle is not easy to follow in the discussion. The comparisons with observations for example and the discussions regarding previous work need clarifications (and quantification ?). Authors should evaluate what is the impact on the global ocean carbon uptake when changes in seasonality are or not taken into account. Compared to the values recalled in the introduction (ocean uptake, about a third of emissions, i.e. 2 PgC/yr) would that change the global ocean uptake by 0.01 PgC/yr or 0.5 PgC/yr ? On the other hand, if I follow author's recommendations, their study also aims at testing errors or bias on the ocean carbon uptake when an ocean observing network does not resolve the seasonal cycle. I missed this discussion in the paper. In the abstract authors suggest their analysis highlights the need of seasonal observations in the extratropical oceans. This really depends on the question addressed. If the aim is to get the best documentation of long-term trends in air-sea CO2 fluxes many experimentalists suggest to focus on regular repeated winter observations while other suggest to focus on regional scale, including full seasonal cycle (as in this study), and extrapolate the regional results to basin scale using diagnostic approaches (e.g. using satellite data and multiparemetric or neural netwok, e.g. Watson et al, 2009). On the same topic, the ocean community has recently produced a white paper for future CO2 observing system (Monteiro et al., 2009 , co-author Rodgers) and the study presented by Gorgues et al. might help to define an optimum observational strategy but this is not really discussed. I think authors should extend the analysis or focus their results and discussions on the processes analysis (-mechanisctic account- in the title) and how the seasonal DpCO2 trends impact on regional and global air-sea CO2 fluxes."

The main point of this study is to quantify the amplitude of the modulation of the seasonal amplitude of pCO2, in order to aid in the interpretation of observations (e.g. Lefevre et al., 2004; Corbiere et al., 2007; Schuster and Watson, 2007; Schuster et al., 2009). To this end, we set up an idealized model configuration using NEMO with a 2° resolution as in Rodgers et al., 2008 but with different forcing (ERA40 climatology in this study vs. interannual NCEP in Rodgers et al., 2008). The modeling approach used in this study also differs from Rodgers et al., 2008 as we specifically model the anthropogenic anomaly of pCO2. In our idealized model, ocean dynamic and biology are climatological and do not endure interannual variability. This idealized model configuration allows us to isolate and describe global mechanisms responsible for the seasonality of the $\triangle$pCO2 trends. To the knowledge of the authors, this is the first time those mechanisms have been identified and described. $\triangle$pCO2 trends in the real ocean are clearly the result of a complex interplay of mechanisms involving ocean dynamic variability, biological variability and also the processes described in our study. It is our belief that the mechanisms we have identified and explained here will be of broad use in the community for interpreting data. Another goal of this study was to offer an estimation of the bias in the computed trends due to the sampling strategy favoring summer data (see figure 5 in our manuscript). Reviewer #2 has suggested that we identify critically spatial/temporal scales for measurements in order to account for the changing seasonal cycle. This is an excellent suggestion, and we have now pursued this in creating Table 1 and Figure 5. These figures reveal that the seasonal bias towards summer measurements leads to a substantial under-estimate of the rate of CO2 uptake by the ocean. This is now stated clearly in the text. We also clearly state that the outline of a future observing strategy will be part of an OSSE. In fact, work on this has already begun as a collaboration between the three authors and several groups that run Earth System Models. This is being pursued as a separate study as it is beyond the scope of the work presented here.

“C1: In the introduction (and in the conclusion), authors indicate that pCO2 increases more rapidly in boreal summer than in winter and refere to Lefevre et al (2004) and Schuster and Watson (2007) who investigated the ocean pCO2 trends in the north atlantic. Regarding the work presented by Lefevre et al, authors should check the results they use in the discussion because Lefevre et al first normalilzed pCO2 at con-

stant temperature before evaluating the pCO2 trends (i.e. warming/cooling not taken into account ?). “ Our apologies for the misunderstanding. The authors actually did check the results of Lefevre et al., 2004 prior citing their results. Indeed, Lefevre et al. normalize the pCO2 at constant temperature, but this temperature is a monthly mean and it therefore undergoes seasonal variability. “Lefevre et al also mentionned that the seasonality of the pCO2 trends might be biased due to sparse data during winter. In addition, they suggest that seasonality of the trends could be explained by changed in primary productivity, but this was very speculative (not quantified at retgional scale).” Lefevre et al., 2004 is cited in the introduction. I quote: ‘In these studies, hypotheses for the North Atlantic involving declining biological productivity (Lefevre et al., 2004) or winter-time mixing and reduced buffer capacity (Schuster and Watson, 2007) have been offered to explain the decreasing $\triangle$pCO2 trends in boreal summer.’ Now, I quote Lefevre et al., 2004 in their conclusion: ‘There is a clear seasonal pattern in the rate of increase, which suggests that the cause may be primarily biological in origin.’ In the author’s opinion, this citation is fair.

Now, if reviewer #2 is referring to the citation in our manuscript’s conclusion (Line 15, page 11). In that case, we were referring to Lefevre et al., 2004 data that are showing that, during boreal summer in the north atlantic, pCO2 in seawater increases faster than the atmospheric concentration (see their figure 4), as in our model. We modified the citation to make it clearer that we are referring to the data showed by Lefevre et al., 2004. We changed the sentence from: ‘This view is in agreement with the hypothesis of a change in the seasonal cycle of the ocean uptake articulated by Lefevre et al. (2004).’ to: ‘This view is in agreement with the hypothesis of a change in the seasonal cycle of the ocean uptake as shown by the data published by Lefevre et al. (2004).’

“Back to the seasonality, Corbiere et al (2007) found the increase of ocean pCO2 in winter is higher than in summer (opposed to Lefevre et al). On the other hand, Schuster and Watson (2007) did not really discussed the seasonal trends as their study was based on the difference between two periods (92-95 versus 2004-05). In recent years, new data-based analysis have investigated the ocean CO2 trends and I suggest authors to discuss their results (and compare the simulations ?) with other studies in the North Atlantic (Corbiere et al 2007; Schuster et al 2009), Equatorial Pacific (Ishii et al. 2009), Southern ocean (Metzl 2009; Takahashi et al 2009). In all these studies, authors used seasonal observations to analyze the trends of pCO2. A synthesis of the DpCO2 trends has been also recently presented in Le Quere et al. (2009)."

Reviewer #2 is pointing out discrepancies between results from the study of Lefevre et al., 2004 and the study of Corbiere et al., 2007. In Lefevre et al., 2004, it is shown in the North Atlantic that the pCO2sw increases more rapidly in boreal summer than in winter. However, Corbiere et al., 2007 showed the opposite with pCO2sw increasing faster during winter than during summer. We thank the reviewer for pointing this out. Either way, this does question the assumption of a seasonal cycle remaining constant as hypothesized in Takahashi et al., 2006 and stress the necessity of understanding the processes that can be responsible for a modification of the seasonal cycle of pCO2sw. We now cite the studies suggested by reviewer #2 in our manuscript.

"C2: The amplification of DpCO2 seasonal trends depends on both atmospheric and sea surface ocean CO2. Therefore, the method section should include a description of atmospheric CO2 used in the simulations. Are authors include seasonal and interannual changes in atmospheric CO2 or add 1.5 uatm/yr as used in the DpCO2 data-based reconstruction (legend in figure 1) ?" The boundary condition used for the simulations here is taken from the Ocean Carbon-cycle Model Inter-Comparison (OCMIP). This field uses a spline fit to ice core and Mauna Loa observations of carbon dioxide in units of ppm from 1765-2001. Those data include interannual variations in atmospheric CO2. Data can be found here: http://quercus.igpp.ucla.edu/OceanInversion/inputs/atm_co2/splco2_mod.dat The manuscript now refers explicitly to the atmospheric CO2 chosen for use in the simulations. "Also, I would recommend to show time series of both atmospheric and ocean pCO2 for several regions (see for example Figure 8 in Rodgers et al 2008),

e.g. North and Equatorial Pacific, Circumpolar zone, Southern Ocean >50S, North Atlantic subpolar gyre..., but not Hudson bay or Arctic coast in Russia..." The regions emphasized in our study were singled out due to their distinct process controls on pCO2. For this reason Hudson Bay and the Arctic coast in Russia were an integral part of the discussion. We chose not to divide the domain by latitude bands, in order to emphasize large-scale processes. If the reviewer insists on monthly plots of integrated fluxes over specific regions, that could very easily be incorporated into the manuscript. However, in our opinion this would lengthen the manuscript without shedding light on the relevant mechanisms.

"C3: Page 749, line 19: when describing the global ocean results (figure 1) authors mention specific results in the Arctic Coast of Russia and in Hudson Bay. Authors recall this again page 749 line 25, page 750 lines 18 and 26, page 752 line 3., page 753 line 19, page 754 line 12.... I understand (from figure 1, the blue lagoons...) that in these regions the trends are clearly different than in other parts of the ocean, but do authors really believe the model is able to reproduce processes in these specific regions ? Is it important regarding global ocean uptake, or processes analysis ? Where are the data that could confirm these trends ?" Those regions are mostly important regarding the processes analysis. Processes described in our study may be at play in those regions but we do not have any data to support this assumption. Again, our goal is not to realistically model the decadal trends in every region of the ocean, but to identify processes which could explain seasonal variability in the ocean pCO2 trends that have been observed in some region and might be also present in others.

"C4 : Page 750 and Figure 1. I'm embarassed with these results and comparisons. First, the authors indicate in the Introduction that data-based evaluation of trends assuming that pCO2 seasonal cycle remains unchanged (e.g. Takahashi et al 2006, 2009), may create suspicious trends and in the Results section, authors compare their simulations with Takahashi et al (2006, 2009), and indicate that simulations are coherent with observations. Second, the simulations presented in Figure 1 are compared with trends from observations (again from Takahashi et al 2006, 2009). For this, authors use the published pCO2 trends, they add 1.5 uatm/yr for the atmospheric CO2 and present values of DpCO2 results in uatm/decade. This is a tricky calculation as this strongly depends on the atmospheric trends, as well as ocean data (very few data over the period 1970-1980 but included in the model results). For curiosity, I've checked some boxes presented in Figure 1, and I think there are errors in the computations. For example, in the North Atlantic, reported trends in original figure (Takahashi et al 2009) are +1.14 uatm/yr (at 60-55N, 40-50W) , +1.94 (60-55N, 30-40W) , +2.00 (15-20S). Using 1.5 uatm/yr in the atmosphere (as specified by the authors) this would translate DpCO2 trends respectively to +3.6 uatm/decade (correct), -4.4 uatm/decade (correct) and -5 uatm/decade (not correct). Therefore, when looking at figure 1, the model seems to be correct in the tropical atlantic, but in fact it is not. As the LDEO dataset is now available (including the most recent 2008 version, see the link at http://cdiac.ornl.gov/whatsnew.html, news Nov 2009), I suggest authors to use the original data set and evaluate the ocean pCO2 trends as done in the simulation, selecting both mean annual and seasonal trends. A simple plot of the model pCO2 (and/or pCO2 trends) versus observations (when it is feasible) would be much more easy (attractive) to read than a map like Figure 1 where observations (sometimes wrong ?) are added in green." The sign of the ΔpCO2 trends in the box (15°-20°N: 20-30°W) was indeed not correct in this version of the manuscript and it has been corrected and all other boxes have been checked. The first main criticism in this comment is the validity of comparing our mean ΔpCO2 trends with data that may be biased toward boreal summer values in northern Hemisphere. Indeed, there is a slightly better agreement between the data (from Takahashi et al., 2009) and the ΔpCO2 trends computed from summer model outputs (see scatter plots and maps in Fig 1,2 and 3).

Second, the authors of this study considered using the LDEO dataset. However, deriving trends at global scale from this dataset would require an intensive work and could justify a publication by itself. Therefore it is beyond the scope of this study and we chose to use the most recent computed trends published, i.e. Takahashi et al., 2009.

Also, using the LDEO data will certainly not change the outcome of the comparison between our simulations and the data. However, if the editor does think that such a validation is required, we are willing to deliver it. The third main point of reviewer #2 is that our simulations do not realistically reproduce the ΔpCO2 trends. If scatter plots tends to prove him right, what the data do show on a map is a local minimum of the trends in the center of the subtropical gyre and local maxima found near the northern and southern borders of the subtropical gyres. This large scale pattern can also be found in the model outputs even if slightly shifted compared to the data. Validation with the scatter plots does not show the agreement on large scale structures. On that basis we decided the best way to validate our simulations is to concatenate figure 1 and figure 2 of our manuscript to have the Takahashi data over the annual mean but also the summer and winter model outputs. Also, it has to be remembered the high uncertainty associated with the data (shown on the maps and in the second panel of scatter plots of this review). Even so, the goal of this study was not to provide the best or most realistic simulation of decadal ΔpCO2 trends. Rather, the intention was to conduct sensitivity studies to focus on processes. Indeed, two of the potential main drivers of decadal changes in ΔpCO2 have been excluded by construction: there is no change in ocean circulation and no change in biological activity. Our perturbation studies can reproduce important features of the main observed structures, and thus this process-oriented study helps to identify processes which can contribute to the observed pCO2 trends. "I'm also concerned that authors used 1.5 uatm/yr for the atmospheric increase when we know that it has been accelerating, up to 2 uatm/yr during 1990s." In this study, we considered the linear trends of pCO2sw between 1970 and 2000. To be consistent, we have chosen to also use linear trends for the CO2 atmospheric increase between 1970 to 2000 (i.e. 1.5 μatm/yr).

"Page 754 Authors explain that DIC has less impact in winter because deeper mixed layer brings low anthropogenic DIC to the surface. This also brings water with high DIC. The trends of DIC and pCO2 in surface water depend on both atmospheric CO2 and subsurface DIC. What would we learn by exploring the trends of the difference between DIC in surface and subsurface ? What are the regions where over 30 years the anthropogenic DIC is coming back to the surface (e.g. mode waters ?)." In our simulations, we only model the anthropogenic DIC and the anthropogenic pCO2. The pre-anthropogenic DIC does not vary from 1970 to 2000. Therefore, the high amount of pre-anthropogenic sub-surface DIC that is brought to the mixed layer is irrelevant to compute the ΔpCO2 trends.

The figure 3 of the manuscript shows that this is the anomaly of the annual mean of DIC which mostly impacts the pCO2. A comparison of the trends of the annual mean anthropogenic DIC (in μmol.kg-1.yr-1) between the surface and the subsurface (1 z-level below the annual maximum of the mixed layer depth) is provided for this review (fig4).

This figure shows that anthropogenic DIC increase faster in the surface than in subsurface everywhere in the ocean with local minima in the subtropical gyres and maxima where the variability of the mixed layer is maximum. In other words, anthropogenic DIC is increasing faster in the surface than in subsurface at places where the winter mixing allow it to enter the interior ocean and decrease its concentration at the surface. At the contrary, places where the mixed layer does not significantly vary see their trends in anthropogenic DIC between surface and sub-surface close to zero (trends in surface are close to zero as well as trends in sub-surface). This was already mentioned in the manuscript and the figure is very close to our figure 1A and does not bring new elements relevant to the discussion. The authors decided to keep it only for the response to the reviewer.

From that diagnostic it's almost impossible to deduct where the mode waters will surface and quantify the amount of anthropogenic DIC transported back into the mixed layer. It would require to study lagrangian trajectory (See Rodgers et al., 2003 and Gorgues et al., 2010) and to study the impact of diffusion on those water masses. This could be the subject of an independent manuscript and is beyond the scope of this study.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C1473/2010/bgd-7-C1473-2010-supplement.pdf

Interactive comment on Biogeosciences Discuss., 7, 745, 2010.

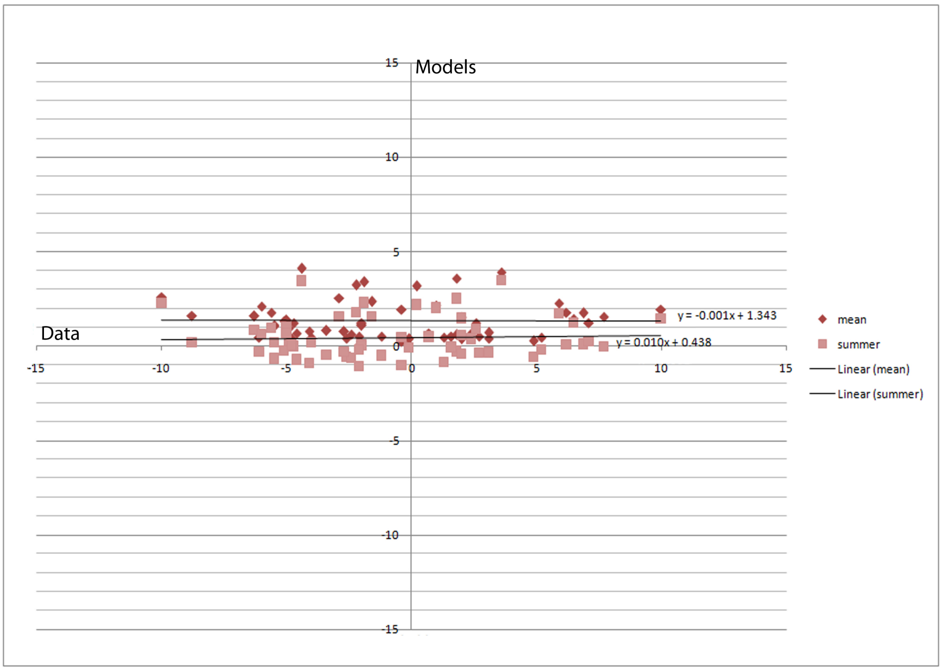

Fig. 1. Fig1:ΔpCO2 (μatm) trends of the model vs data for the annual mean and the summer.

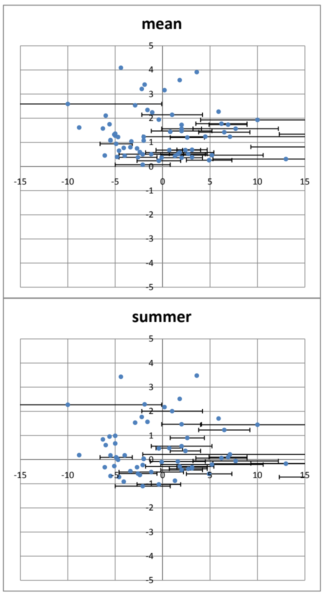

Fig. 2. Fig2: ΔpCO2 trends (μatm) of the model vs data for the annual mean and the summer with error bars.

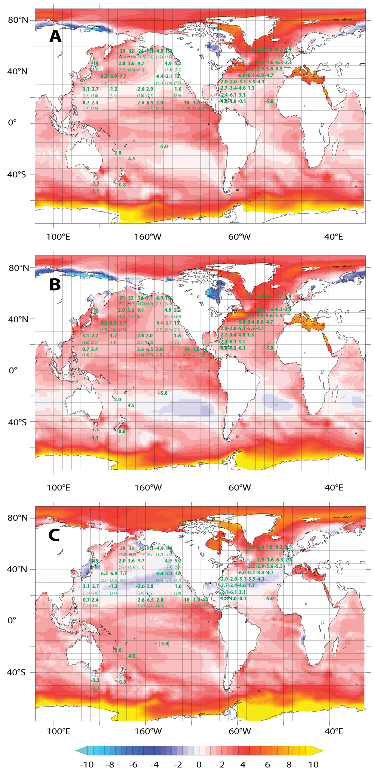

Fig. 3. Fig3: ΔpCO2 trends for the data (green numbers, probably biased towards summer) and for the model for (A) annual mean, (B) boreal winter and (C) boreal summer.

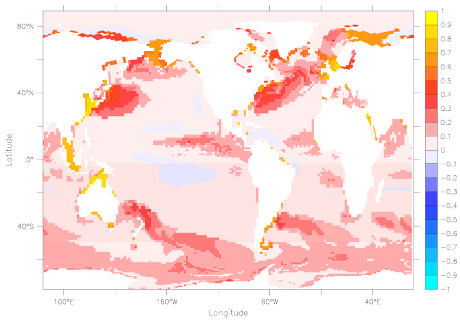

Fig. 4. Figure 4