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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.

Anonymous Referee #2

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Review of a manuscript submitted to Biogeoscience

Title: A mechanistic account of increasing seasonal variations in the rate of ocean uptake of anthropogenic carbon. Authors: Gorgues, Aumont and Rodgers MS No:bg-2010-7

Decision: This manuscript is not acceptable for publication in its present form

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 adressed. 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.

Other comments:

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 constant temperature before evaluating the pCO2 trends (i.e. warming/cooling not taken into account ?). 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). 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).

C2: Page 748-749: 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) ? They should give a reference of the atmospheric data used for the simulation. 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...

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 ?

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. 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.

C5 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 ?).

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