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Biogeosciences Discussions

Interactive comment on "Atmospheric CO_2 seasonality and the air-sea flux of CO_2" *by* P. R. Halloran

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Before addressing the reviewers specific comments, I would like to highlight the significant changes that this manuscript has undergone in response to the review process, and thank all of the reviewers for the many constructive comments which have I feel broadened greatly to potential interest in, and applicability of, this manuscript.

I have extended the original off-line analysis considerably with two additional sets of simulations, a newly run ensemble (and parallel control run) where I prescribe observed atmospheric CO_2 seasonality to runs previously in equilibrium with spatially and temporally fixed atm. CO_2 concentrations, and a fixed atm. CO_2 and freely-evolving (and hence seasonally varying) atm. CO_2 preindustrial control run pair. These new simulations allow me to quantify both the cumulative out-gassing occurring in response to the

change, and the difference in air-sea flux and surface ocean CO_2 concentration once the system is back in equilibrium.

I have then modified the focus of the paper considerably, reflecting the finding that the cumulative out-gassing occurring in response to the seasonality change is small, and highlighting the value of this finding in providing reassurance that the methodological design followed by CMIP5 (fixing atm. CO_2, and eliminating potential feedbacks through CO_2 seasonality), is, from this perspective, quite acceptable – I believe, a valuable (and not necessarily intuitive) finding. This change of focus is hopefully well represented by the new title.

The original mechanistic investigation of the instantaneous response to an atm. CO_2 seasonality change now exists (improved in response to reviewers suggestions) in sections 2.1 and 3.1, with the addition of figure 1b. Sections 2.2-3 and 3.2-3, together with additional background and discussion and new figures 1a and 7-10, go through the findings from the additional simulations and analysis, and hopefully answers the fundamental question which came out of the three reviews, which was – is the change seen in response to changing seasonality significant.

Reviewer 1:

- more discussion to be included in the manuscript As I hope you will agree, the manuscript has been extended significantly, with additional discussion included throughout. As an example, the beginning of the methods section now discusses in considerably more detail the structure of the model I've used, and relevant components and equations within that model. The background processes occurring to control CO_2 uptake/release from the ocean are then discussed in the first paragraph of the results section

- the author mentions that the amplitude of the atm. CO_2 seasonal cycle has increased recently but did not explain which mechanism lead to this I absolutely agree that it would be fantastic to add this, but to my knowledge (and if possible please correct me), this question is not adequately answered by the existing literature. I have included a new sentence to highlight contributing factors ('The sign of this change is consistent with the observed lengthening and intensification of the growing season, and associated increase in net primary production, occurring in response to warming (White et al., 1999; Goulden et al., 1996), rising concentrations of atmospheric CO_2 (Lewis et al., 2004), and local increases in solar radiation intensity (Graham et al., 2003) '), but I believe that to get to the bottom of this question is well beyond the scope of this work, and actually, not directly important here in where I focus on the response to the change, rather then the drivers of that change.

- add a figure showing the atm. CO_2 seasonal cycle used in the model (Hadgem2s interactive cycle) I absolutely agree that this should have been a key figure. The described data, along with the observed atm. CO_2 seasonal cycle (relevant to the new simulations, but also for comparison with that simulated by the model), are now included as Figure 1.

- why were 1x and 2x atm. CO_2 seasonal cycle chosen As now described in the text (Page 8306 L 18), 'The chosen seasonal cycle magnitudes represent conditions as they may be specified in Earth System Model simulations with CO_2 prescribed as in the bulk of CMIP5 experiments (Taylor et al., 2009), as they would be simulated by a fully interactive Earth System Model for the preindustrial, and at an arbitrarily chosen value representing a significant increase over the preindustrial seasonal cycle to ensure a clear signal '

- if possible, experiments with preindustrial physical forcing but different atm. CO_2 seasonal cycle over at least 100 years would give us a much better picture and more convincing results I completely misjudged the interest in the mechanisms alone, and addressing this have now conducted the described experiments. These new experiments now provide a key focus for the manuscript, and although they demonstrate the cumulative response to the changes originally described to be small, I believe they make the manuscript much more valuable. Note that given how quickly

the disequilibrium arising from the change in seasonal cycle disappears, 100 year simulations were not required.

- If not possible to do such experiments, discuss what is the benefit of such a simple experiment? What are the caveats? The experiments could be (and have been) conducted, so it was not necessary to follow up this comment.

- how do you justify the simplicity of the experiment on the statement in your abstract 'intersting implications for glacial-interglacial climate change' which occur on much longer timescales than this experiment. Agreed. Pleistocene cyclicity came to mind because of the links to seasonality change, but in light of the results of new experiments (and particularly the timescale over which a new equilibrium is reached) I have removed this from the abstract and discussions.

- do we know the atm. seasonal cycle during the glacial-interglacial. No, I'm pretty sure we don't, and this was part of my original point, if there were changes which responded significantly to atm. CO_2 seasonality, I wanted to encourage thinking about just this question – however this is now not discussed in the manuscript (see above).

- While figure 2 shows that latitudinal change in air-sea CO_2 flux, what is the net global annual change? Is it significant, or within the model error bars? This question is hopefully now addressed comprehensive by the analysis of the new experiments and the control run simulations, figures 8 and 10 and the associated discussion. The answer is that it is statistically significant (figure 7a), but small – less than one standard deviation of control run variability (figure 8).

- The author may be correct in conceptually identifying the changes in CO_2 flux is due to the solubility and sea ice effects. But is this also the case in fully coupled model simulations... Having longer simulations with active carbon cyle processes would be really beneficial in this case. Again I completely agree. Please see my response to the reviewer's 5th question.

- P 8308 L 25: Add refence(s) to the statement 'The steady-state atmos...' I've added a reference to the really nice 2002 paper by Volker et al.

- P 8309 L 28: Add refernce(s) after 'a large seasonal cycle...' This statement does not feature in the revised manuscript

- P 9311 L 8 'Given the findings...' This is a very important statement and it would be better if the author can show whether the simulated change is significant or not. The new simulations clearly show this is a minor issue, and as such the broader manuscript change (which have include the removal of this line) hopefully address adequately this point.

Interactive comment on Biogeosciences Discuss., 8, 8303, 2011.