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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 3, C. Curry:

I would like to particularly thank Charles Curry for going to the effort of undertaking analysis using the CanESM1 model to help review this paper. I have now adopted the reviewer's approach within the heavily revised manuscript, adding a new section to look at the control run simulations in such a way that is comparable to the analysis presented by the reviewer himself. I'm not sure if this is considered appropriate by the journal, but to acknowledge that the idea for this analysis came from Charles Curry I have included a reference to the review itself – I see the potential to do this to be one of the many great things about open access and open review publishing, so hope this can be included if a final manuscript is accepted for publication.

details of how the 1xCO_2 experiment was conducted - e.g. what exactly were the

monthly values of XCO_2 used, and how did the vary with geographic location? I now include this information in the new figure 1b, with considerable detail added to the methods section: 'The different magnitude atm. CO_2 seasonal cycles were calculated by multiplying the difference between individual monthly atm. CO_2 values and the annual average atm. CO_2 concentrations in each latitude-longitude box, by the specified factor (zero, one or two), and adding that to the annual mean value at that point. Within this paper, '1x seasonal cycle' therefore refers to the seasonality simulated for the preindustrial period within the HadGEM2-ES model when run with fully interactive carbon cycle components (figure 1b), '0x seasonal cycle' refers to a situation without any temporal variability, and '2x seasonal cycle' considers the annual variability at each point to be twice that simulated for the preindustrial period. '

The interdependence between ocean CO_2 uptake... sea-ice cover and solubility certainly sound reasonable... [but the changes relative to the control are relatively small]... thus the statistical significance of such model changes is an outstanding question. [The rest of the review then examines this question as far as possible within pre-existing CanESM1 simulations.] I fully agree, and acknowledging that I misjudged the interest in the mechanisms alone (the previous focus of the paper), I have substantially revamped the manuscript essentially to investigate this question. I have looked into this is two new ways. Firstly I undertake a set of new experiments examining the cumulative uptake occurring in response to a switch from a 'no seasonal cycle' case to prescribing the observed seasonal cycle (see section 2.1 for a description of the experiments). Results from these simulation are presented in section 3.2 and discussed in relation to the new figures 7-9. The change is shown to be robust (agreement between ensemble of three simulations in figure 8, and spatial distribution of statistical significance in figure 7a), but the cumulative change small. Secondly, following the methodology presented by the reviewer using results from CanESM1, I examine the difference between two control simulations to examine the impact once the globally averaged air-sea CO_2 flux has come back into equilibrium (i.e. following on from the analysis described above). Like Curry, I present results from a simula-

tion with spatially and temporally fixed atm. CO_2 concentrations, and one where the model interactively produces an atm. CO_2 seasonal cycle (see section 2.3 for methods). As discussed in the text (section 3.3, figure 10a), I find a similar pattern in the change in air-sea CO_2 flux to that seen in CanEMS1, but a similar limited extent of significant difference. Interestingly then, looking at surface ocean CO_2 concentrations rather than air-sea flux, a see statistical significance over a much larger area between the two simulations. I propose that the mechanisms I examine play an important part in maintaining this difference in simulated surface ocean pCO_2. By examining these additional, and pre-existing simulations I conclude that the impact of a change from no seasonal cycle to the observed seasonal cycle has only a very limited impact on air-seas flux. I then highlight that it is important to demonstrate the answer to this (as far as I'm concerned) non-intuitive question given the CMIP5 move to specifying atm. CO_2 concentrations, rather that letting model's calculate CO_2 concentrations from emissions (and hence simulating a seasonal cycle).

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