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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

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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 2:

First and foremost the author does not attempt to prove in any way his proposed hypothesis. It is not clear at all that the described mechanisms are actually represented in the model and how significant they may be.... what one really needs to do is run the coupled ocean-atmosphere model with an imposed perturbation on the CO_2 atm seasonal cycle. The resulting air-sea CO_2 fluxes could then be compared with the air-sea CO_2 fluxes in the control simulations. I agree, and accept that I completely misjudged the interest in the mechanisms alone. Addressing this I have now conducted the experiments described by the reviewer. 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 that

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to be able to show this is in itself valuable. The new experiments are now discussed in length in the methods section (2.2), the results section, figures 1, 7, 8 and 9, and the results feature prominently in the abstract and conclusion. Note however that the new experiments I undertook are not coupled ocean atmosphere simulation, because I wanted to extend the analysis already done and therefore carefully control the conditions by prescribing atm. Conditions to a coupled physical-biogeochemical ocean model. To examine the coupled response I extent the work by analysing pre-existing coupled control run simulations as discussed with reference to and presented in figure 10.

The reviewer's following three paragraphs suggest ways that understanding could potentially be obtained from analysis of control runs/pre-existing simulations if the additional simulations were not possible. I am very grateful for these suggestions and discussion, however because of the time-extension granted for my response, and the surprisingly short period of time the new simulations took to reach equilibrium, I have been able to extend the manuscript through examination of additional simulations, and a complimentary analysis of control simulations.

The author never shows the actual seasonal cycle of atmospheric CO_2 simulated by the model The inclusion of this information makes a lot of sense, particularly given the inclusion of the new experiments. The described data is now presented in figure 1 along with the observed atm. CO_2 (both for comparison, but to help explain the conditions imposed on the new simulations).

he does not discuss the reasons why the amplitude of this seasonal cycle is changing with time, and the relative effects of terrestrial and oceanic effects I have included new text to highlight the factors potentially contributing to this change: '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)'. Beyond the sentence 'Intra-annual CO_2 variation is primarily driven by seasonal uptake and release of CO_2 by the terrestrial biosphere (Machta et al., 1977; Buchwitz et al., 2007), with a small (Cadule et al., 2010), but potentially changing (Gorgues et al., 2010) contribution from the ocean. ' I have not addressed specifically the relative effects of terrestrial and oceanic processes. This is because, although the warming ocean and changing Revelle factor will have an impact in some areas, my understanding is that where the atm. CO_2 seasonal cycle has been seen to be changing, that seasonal cycle is essentially driven by terrestrial processes. I apologies if I've missed this in the literature, if so please correct me.

It would be best if the author actually showed that the seasonal cycle of atmospheric CO_2 increased with climate change in the 21st century simulations of this model (for example), and discussed those mechanisms briefly Although I can certainly see this being interesting – and there is scope for a fascinating study looking at this in relation to the observed historical changes – my opinion is that this is somewhat tangential to the aim of the manuscript I present which is to try and understand how the system might respond to such changes, rather then what causes these changes.

Note that over the global oceans, the variation of surface-water pCO_2 is actually much greater than that of atmospheric pCO_2, and the direction and magnitude of the sea-air CO_2 flux are hence mainly regulated by oceanic CO_2. Will this have any relevance for the proposed mechanisms? Changes in this are examined in a really nice study by Gorgues (referenced in the manuscript), but whilst this dominant control on the absolute air-sea flux seasonality are inherently accounted for in these experiments, I look at deviations from these changes. These processes could of course become important, and quite possibly dominant, if the atm. CO_2 seasonal cycle were driven by a mechanism that also effected the ocean physics/biology. To understand the relative importance of these processes would require a very different set of experiments.

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You need to put in the actual equations you used to calculate the air-sea CO_2 fluxes under Methods (line 4, page 8306), this would make things easier for the reader Certainly. This is now included at the level I think required to help readability (details are included in referenced papers) in the much extended methods section.

How is the 1x CO_2 calculated exactly? I tried (and clearly failed) to make this clear in the original manuscript with the sentence 'Within this paper, '1x seasonal cycle' therefore refers to the seasonality simulated for the preindustrial period within the model', but have I hope clarified this now in the modified sentence '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' and added a new figure (figure 1) showing what these values are.

What are the monthly values of CO_2 used, and how do they vary globally? These values are now presented in the new Figure 1

In figure 2, even if these fluxes are instantaneous and do not represent a steady state, it would be interesting to show some absolute numbers and not just the difference from the situation with no seasoanl cycle. The absolute values for the no seasonal cycle case are shown in the dashed blue line. The deviations from the no seasonal cycle case are small, so I considered it to be clearer to plot just the differences from that no seasonal cycle case. However, with the new experiments available, I now include the new figure 8 which presents the absolute globally averaged values occuring in response to the perturbation (i.e. moving from no seasonal cycle to the observed seasonal cycle).

In your Methods section, please describe your carbon biogeochemistry and ecology subroutines for the ocean and land components... I completely agree and a brief description of these model components has been added to the methods section, including further references to allowing the interested reader to examine this aspect in detail.

The paper does not contain enough discussion of the geographical patterns and mechanisms behind the seasonal variation of the air-sea CO_2 fluxes... for example, some background is needed to explain the patters in Fig. 2 (dotted line) and setup the stage for your proposed mechanisms... A new paragraph has been added to do just this at the start of the results section, and hopefully therefore nicely introduces this section.

The reviewer highlights the complexity of the ocean processes controlling CO_2 uptake and asks again if these mechanisms have any relevance for my proposed mechanism. Please see my response to the reviewer's sixth point

...while the proposed mechanism might indeed take place, it might be dwarfed by the concurrent seasonal changes in biology or by changes in wind... I can't tell (without analysing the full climate model simulations) whether your proposed mechanisms... will be a major or minor player in this complex system. I absolutely agree, and I am glad that the review process has motivated the extra simulations which allow me to at least partly answer this question. What the extra experiments show is that these mechanisms and feedbacks are small (of the same order of magnitude globally as interannual variability) – figures 7 and 8, and accompanying text. I do not however attempt to answer how the magnitude of the changes in air-sea flux responding directly to the change in atmospheric CO_2 seasonality might compare to those brought about by concomitant changes in biological activity/winds etc. caused by whatever (hypothetically in this case) caused the change in atm. CO_2 seasonality. I do not address this question in the manuscript because I did not what to focus on the driver of the atm. CO_2 seasonality change, instead of focusing on the response. Since the CO_2 seasonality could come about in response to a number of different changes, to examine these all would become a substantial study in itself. Given that I now find the change occupying in response to the seasonality change to be small, I would suggest that the follow-up study is of limited importance, however the point that the reviewer raises is very interesting, and worth discussion, so have noted this with the

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following sentence in section 4 'A further point to note is that, assuming the CO_2(atm) seasonality change is occurring in response to a change in some component of the climate system (rather than for example, a change in land use), the ocean ecosystem, and factors impacting the air-sea flux (e.g. temperature and windspeed) may respond themselves to the climate forcing, modifying, and potentially dominating the response to the change in CO_2(atm) seasonality'.

...I recommend that the author resubmit this paper at a later time once he manages to validate (or invalidate - negative results are also valuable additions to our knowledge!) the presence and significance of the proposed mechanisms in the Hadley model simulations. I hope that the reviewer considers the considerable revision to this manuscript, coming about from the four additional simulations, and analysis of two further pre-existing control run simulations, to address adequately the concerns. I also welcome the reviewers comment that 'negative results are also valuable additions to our knowledge' since the small net response we observe in the latest Hadley Centre model could by some be considered a negative result! From my point of view there is certainly value in this study, because (despite them being small) the differences we see in surface ocean CO_2 concentrations between fixed CO_2 and interactive CO_2 control runs are a prominent feature, and without understanding them they are a cause for concern. Further, in response to the many valuable suggestions my all of the reviewers and consequently the additional experiments undertaken, the new manuscript can now provide confidence that (at least with respect to the influence of CO_2 seasonality) the CMIP5 experimental design is sound (see discussion in introduction and conclusion).

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