Biogeosciences Discuss., 8, C4512–C4517, 2011
www.biogeosciences-discuss.net/8/C4512/2011/

Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 21 November 2011

Summary of the paper:

In this paper the author investigates the influence of an increase in the atmospheric CO2 seasonal cycle amplitude on the air-sea flux of CO2. The author uses simulated fields (including atmospheric CO2 values) from the preindustrial control simulation of the HadGEM2-ES model, and manipulates them off-line to calculate the instantaneous air-sea CO2 flux while imposing three conditions on the CO2atm seasonal cycle: no seasonal cycle, 1Xpreindustrial seasonal cycle, 2Xpreindustrial seasonal cycle. The author notes an increase in the flux of CO2 into the ocean at mid-latitudes and a decrease at high latitudes in the presence of an atmospheric CO2 seasonal cycle. The decrease/increase patterns are amplified with an increase in the seasonal amplitude signal (Fig. 2 in the paper), especially in the Northern Hemisphere.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The author explains these patterns showing two idealized mechanisms supported by two highly idealized figures (Fig. 3 and Fig. 5). The first mechanism ascribes the outgassing of CO_2 at high latitudes to a tight coupling between the CO2atm seasonal cycle and the ice cover seasonal cycle. The idea is that if the amplitude of the seasonal CO2atm cycle is amplified, it will not increase the CO_2 flux into the ocean in winter (when CO2atm is high) because of the presence of ice. On the other hand the ice-free periods coincide with low CO2atm so that the average CO2atm that the ocean will be exposed too is lower under an amplified seasonal cycle.

The second mechanisms would explain the increase of CO_2 flux into the ocean at mid latitudes. Again, this mechanism is idealized in Figure 5 and shows that during winter, when CO2atm is higher, the solubility of CO_2 in seawater is also higher, while the opposite happens in summer. The author shows the resulting cycle of CO2atm concentration at equilibrium for the normal and the doubled amplitude of CO2atm seasonal cycle. The author concludes that since there is an increase (from 1X to 2X seasonal cycle) in CO2atm at equilibrium, this means the ocean should be able to hold more CO_2 (additional ingassing according to Figure 2).

General Comments:

This paper presents an interesting set of hypotheses of how changes in atmospheric CO_2 seasonality might impact air-sea CO_2 fluxes. However, while the topic and premise of this paper as well as the general discussion in the paper related to potential applications for glacial-interglacial cycles or future climate change – are clearly interesting and worthy of further pursuit, the paper has numerous and very serious scientific flaws, as listed below.

First and foremost, the author does not attempt to prove in any way his proposed hypotheses. It is not clear at all that the described mechanisms are actually represented in the model, and how significant or insignificant they might be. As the author himself suggests, what one really needs to do is to run the coupled ocean-atmosphere model with an imposed perturbation on the CO_2atm seasonal cycle. The resulting air-sea CO_2 fluxes could then be compared with the air-sea CO_2 fluxes in the control simulations (with no imposed perturbation).

While this would be ideal, I understand that detecting the proposed mechanisms in model simulations is nontrivial and new simulations take a long time. However, even without running new simulations, the author could have made the effort – for example - to sample the historical (or even the preindustrial control) simulation outputs of the HadGEM2-ES model to find interesting natural or anthropogenically induced variability in the CO_2atm seasonal cycle and at least partially demonstrate the validity of these hypotheses.

As stated by the author (from p. 8308, line 23 to p. 8304, line 2) the net effect of a shift in air-sea CO_2 exchange would arise from a combination of factors that are not considered here and that could only be addresses using coupled earth system models. HadGEM2-ES is exactly one of such highly reputable models. It is therefore not clear why the author did not explore the set of simulations already available to attempt to validate his hypotheses.

On p.8309 (lines 11-16) the author cites the last IPCC report to give evidence of projected changes in the CO_2atm seasonal cycle magnitude and decrease in sea-ice in the near future. Clearly the ensemble of Earth System Models used for the report show such changes. Again, it's not clear why the author did not bother to analyze these (available) outputs to find evidence of the hypothesized mechanisms described here.

More specific Comments:

The author never shows the actual seasonal cycle of atmospheric CO_2 simulated by the model, nor does he discuss the reasons why the amplitude of this seasonal cycle is changing with time, and the relative effects of terrestrial (e.g. changes in land productivity) and oceanic effects. It would have been best if the author actually showed that the seasonal cycle of atmospheric CO2 increases with climate change in 21st century simulations of this model (for example), and discussed those mechanisms briefly.

Note that over the global oceans, the variation of surface-water pCO2 is actually much greater than that of atmospheric pCO2, and the direction and magnitude of the sea-air CO2 flux are hence mainly regulated by oceanic CO2 (e.g. Takahashi et al. 2002). Will this have any relevance for the proposed mechanisms?

You need to put in the actual equation you used to calculate the air-sea CO2 fluxes under Methods (line 4, page 8306), it would make things easier for the readers.

How is the 1xCO2 calculated exactly? What are the monthly values of CO2 used, and how do they vary globally? This is completely unclear from the paper.

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 seasonal cycle.

Page 8306 – line 20: I think you need to replace “annual” with “monthly”.

In your Methods section, please describe your carbon biogeochemistry and ecology subroutines for the ocean and land components. Do you have actual ecological modules (e.g., phytoplankton groups, etc.) and what is the land biogeochemistry used? These technical details need to be mentioned for those not familiar with your model, especially since the choice of your biogeochemistry subroutine might have an impact on the seasonality of ocean CO2 and your CO2 air-sea fluxes.

The paper does not contain enough discussion of the geographical patterns and mechanisms behind the seasonal variation of air-sea CO2 fluxes. The discussion is therefore often overly simplistic. For example, some basic background is needed to explain the patterns in Fig 2 (dotted line) and setup the stage for your proposed mechanisms. Why is there annually averaged ingassing north of 40N, while there is both ingassing and outgassing in the Southern Hemisphere (Fig 2, dotted line), and how do these annually averaged patterns result from highly seasonal fluxes?

Air-sea CO2 fluxes are affected by seasonal changes in temperature, biological uptake and gas exchange efficiency (hence winds), all of which (not just temperature!) will change in a future warmer climate. Takahashi et al (2002) showed that the seasonal amplitude of surface water pCO2 (and accordingly the resulting CO2 fluxes) is dominated by biological effects in high latitudes above 40o latitude, and by the temperature effect in the temperate gyre regions. The temperature and biological effects are about 6 months out of phase, such that, along the boundaries between these 2 regimes they cancel each other, forming a zone of small ocean pCO2 amplitude. Will these more detailed mechanisms have any relevance for your proposed hypotheses? For example, your “mid-to-high latitude” mechanism (Figure 5) is based on considerations of solubility/temperature, but this is a region where according to Takahashi (2002) biological effects actually dominate the seasonality of your air-sea CO2 flux. Thus, while your proposed mechanism might indeed take place, it might be dwarfed by concurrent seasonal changes in biology or by changes in wind. I cannot tell (without analyzing the full climate model simulations) whether your proposed mechanisms (based on sea ice and solubility effects) will be major or minor players in this complex system.

Conclusions: In conclusion, this paper contains only an interesting motivation for a more complete ocean-atmosphere modeling study that has not been carried out yet. The paper lacks the quantitative analysis/research that demonstrates (or invalidates) the proposed mechanisms and hypotheses. This is not acceptable for a research paper. Earth System Models may be a simplification of reality and may contain significant biases in the representation of climate but they provide a synoptic and quantitative description of many processes not easily measurable in reality. Given the lack of analysis of readily available model output, this paper is not worth publishing. I recommend the author resubmit this paper at a later time once he manages to validate (or invalidate – negative results are also valuable addition to our knowledge!) the presence and significance of the proposed mechanisms in the Hadley model simulations.

BGD

8, C4512–C4517, 2011

Interactive Comment

BGD

8, C4512–C4517, 2011