Biogeosciences Discuss.,
doi:10.5194/bg-2016-112-RC1, 2016

Interactive comment on "Drivers of Multicentury Trends in the Atmospheric CO_2 Mean Annual Cycle in a Prognostic ESM" *by* Jessica Liptak et al.

Anonymous Referee #1

Received and published: 17 June 2016

This study explores different contributors to the increase in the atmospheric CO2 seasonal amplitude, as predicted by the CESM in simulations that span 1950-2300. I am generally supportive of this paper. Clearly an impressive effort went into it and it is well organized and written. However, I have some major concerns, listed in order of decreasing priority:

1) There are some steps in the methodology that need more detail and justification – some of them could/should really be stand-alone papers. These include a) The pulse-response method. b) The documentation of mid-latitude trends in observed CO2 amplitude

2) The CESM does a poor job of reproducing the current CO2 amplitude and the historical observed amplitude trends, which undermines confidence in the results presented

here. Although I think the exercise is still worthwhile, some sort of well thought out rationale or statement is needed to explain why readers should believe or pay any heed to the future model results going out to 2300, e.g., are there certain results that are robust and insightful despite the model's poor present-day performance?

Expanding on 1a) The pulse-response method. This could really be a stand-alone paper (see, e.g., Nevison, C.D., D.F. Baker, and K.R. Gurney, A methodology for estimating seasonal cycles of atmospheric CO2 resulting from terrestrial net ecosystem exchange (NEE) fluxes using the Transcom T3L2 pulse-response functions, Geosci. Model Dev. Discuss., 5, 2789-2809, 2012, www.geosci-model-dev-discuss.net/5/2789/2012/ doi:10.5194/gmdd-5-2789-2012, 2012.)

While I support the method and realize that it would be prohibitively expensive computationally to break down the contributions to CO2 amplitude change from different regions and mechanisms without some sort of shortcut approach like the Pulse Response method, I think it needs more than a 1 paragraph explanation. For example:

i) Is there any IAV in the meteorology used to create the pulse fields? Also, what is the consequence of assuming those met fields will still apply in 2300? ii) How are the 60-month decaying pulses combined to create a model atmospheric CO2 cycle? iii) In figure 2, the pulse-response amplitudes at midlatitudes are 3ppm or more smaller than the fully prognostic tracer. This doesn't seem "broadly similar" and undermines confidence that this methodology can detect subtle trends, esp. in the midlatitudes. iv) The GMD Discussions paper above was never accepted for final publication, due to reviewers who thought adjoint methods were superior. While the current method is superior in that it divides land into a larger number of regions (20 v. 11), the GMDD paper on the other hand was applying the method to estimate mean seasonal cycles, which are easier to get right than the more subtle trends in amplitude over time examined here.

Expanding on 1b) I'm not sure there is any evidence that CO2 seasonal amplitude is increasing at midlatitude sites such as NWR or UUM, KZM/D. In fact, if anything, they may be decreasing – possibly due to drought effects. The most robust effects are seen at BRW, with the amplitude increase at MLO less than half that of BRW. I don't think Zeng et al., 2014 is an adequate reference to prove that midlatitude CO_2 amplitude is increasing, since they don't actually show this.

Minor comments: p.1,L8, The term "changing atmospheric composition" to encompass CO_2 fertilization and N deposition is confusing. These two don't really belong in the same category, in my opinion, since the N deposition is relevant mainly after it deposits on the soil, i.e., the authors are not looking at some sort of physiological response of plants to increased atmosphere NO_x or NH_3 concentration. p.1,L12 is confusing as written – in one case we have the end time (2300) and in the other we have the start time (after 2100). Please rewrite to clarify start and end times for both effects p.1,L15 "rather than the strength of the terrestrial carbon sink" please explain more clearly what is meant here. p.1,L17, suggest replacing "is not predicated on" with "does not necessarily imply" p.1, L20 I think it's more accurate to say "at some NH sites" rather than "over the NH" (see my comments above about midlatitude trends). p.2,L31 missing AND between citations. p.2, L35 suggest saying, "Model evidence suggests that the combined effects . . ." and delete "in simulations." P2.,L20 and p.3,L17 again I find the catch-all term "changes in atmospheric composition" confusing. p. 6, L30. It seems like a stretch to call 425 ppm and 391 ppm "roughly equivalent" p.7,L5 Please provide a reference for the observed mid-latitude trend of 0.04 ppm yr^{-1}. P8, L19 Please explain further. Why is this consistent with effects being proportional to GPP? P9L12, to avoid confusion, would suggest splitting into 2 sentences: "..simulation. These latter influences added 4.7 ppm . . ." P9, L27 The Zeng et al reference, in my reading, does not actually demonstrate that the spatial distribution of where atmospheric CO_2 amplitude increases are seen (mainly at high latitudes) are consistent with agriculture, which is large at mid-latitudes. P10, L23, "perhaps indicating . . ." Please explain further.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-112, 2016.