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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 #2

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The paper describes an analysis of potential drivers of multi-century trends in the seasonal cycle amplitude of the atmospheric CO2 concentration with a Prognostic Earth System Model. The study follows from the paper of Graven et al. 2013 that analyzed in detail the large increase of atmospheric CO2 seasonal cycle amplitude at high northern latitudes over the pas 60 years; In a series of studies trying to disentangle the drivers of the observed increase in atmospheric CO2 seasonal amplitude, this paper propose a first attempt with a prognostic coupled carbon-climate cycle model and an investigation of the amplitude changes up to the horizon 2300.

The paper is clearly written and relatively easy to follow. However it seems to me that the simulations performed in this study with the chosen model does not completely allow to investigate some of the questions (for instance, what are the drivers of the increased atmospheric CO2 seasonal amplitude). The coupled climate – carbon cycle

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model helps to understand the potential feedback between the land surface processes and the atmosphere and to investigate long term prediction; but the chosen model with its biases (i.e., the too low amplitude of the mean seasonal CO2 cycle) requires more caution when discussing the relative contribution of all potential drivers of the observed amplitude change (CO2, climate, agricultural changes, . . .).

It is not clear (at least to me) what the study brings in comparison to previous studies as I feel it does not focus enough on the "potential novelty" linked to i) the use of a coupled ESM as well as ii) the use of "regional atmospheric influence functions" to analyze the regional and temporal contribution of the potential drivers. Note that this last part is poorly valorized and not discussed in detail enough. I also find that on average the results are exposed but not analysed enough in terms of processes (GPP versus the different Respiration terms; contribution of different PFT; which are the key processes in the model that are responsible for the modeled trend and CO2 amplitude (water versus temperature limitations, . . .)). The limits of the model are also not discussed enough in terms of which scientific results are "robust" versus those that are likely not very uncertain (especially when discussing the time frame 2100 – 2300).

I thus recommend major revisions prior to consider that such work brings new information for the understanding and the prediction of the atmospheric CO2 seasonal amplitude changes.

Main comments

* Introduction:

- The authors provide a nice literature review of articles that have tried to explain the increase of atmospheric CO2 amplitude. However they lack the recent study by Hakihiko Ito et al., 2016 in Tellus " Decadal trends in the seasonal-cycle amplitude of terrestrial CO2 exchange resulting from the ensemble of terrestrial biosphere models Âż Note that such study is using an ensemble of process-based land surface models, including two version of CLM (CLM4 and CLM4VIC) which are probably close to CLM4CN

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used in this study? Although such study was just published, it would be now crucial to include it in the literature review, given how comprehensive it is.

- Secondly and more importantly, we miss after such review what are the remaining critical uncertainties around the drivers of the seasonal CO_2 increase? For instance Forkel et al. (2016) claimed that they could reproduce reasonably well the observed CO_2 amplitude increase. What is thus missing or what is uncertain from their study? A critical analysis of the past literature in order to define the “niche” for this paper is missing. It would be good to have a set of more precise questions that the paper will target.

- Page 3, l13: The justification for the need of a full land-atmosphere-ocean coupled model is not provided, at least given the scientific questions that underlines the study? You need to justify why using the full ESM is beneficial and what can it bring compared to others studies (for instance, Ito et al. (2016) have used an ensemble of land surface models and similar experimental set up to separate the effect of potential drivers)? You could have envisaged forcing the CLM4CN model with climate predictions with a bias correction. What do you gain from your coupled approach ?

- It seems strange to me to emphasize the period 2100 – 2300 with a model that does not include Permafrost modeling and other critical processes linked to land management (no crop specific module, or no vegetation dynamic); while these may be more crucial in very long term simulations. You have at least to justify that the model is suitable to answer the question you pose.

- In general the introduction should propose a set of questions that follow from points that have not been treated by previous studies or based on the uncertainties that are still prevailing? And your approach (i.e. the use of CESM1) should be justified or at least explained with respect to the objectives.

* Model section:

What does CLM4CN do for natural vegetation shift. This will be crucial in the boreal zone with possible tree migration northward especially with such long time frame investigated (2300). Few word on this aspect would be beneficial.

* Experiement:

- The authors mention using “impose CO2” for the different experiment while in the result sections they say “The imposed emission scenario” (page 7, l10). The procedure became only clear to me when reading the note page 7, l15: “We note that the atmospheric CO2 moles fraction values were diagnostic only. . ..”. I thus think that the “experiment section” should describe more precisely what was done and differences between imposed CO2 and diagnostic CO2.

* Mapping atmospheric CO2 (section 2.3)

- Page 5, L 13: you should precise which patterns of the monthly CASA fluxes was used to prepare the pulse functions: GPP, NEP, NEE?

- Page 5, l25-28: There are potentially large differences between the CASA NEP spatial patterns and the CLM4CN ones so that it is not at all obvious that the “mapping approach with GEOS-Chem” will not be biased through differences in these spatial patterns. Discussion of Figure 2c brings a first insight but the authors should discuss more the impact of “surface pattern differences” and “transport differences” for the trend in the atmospheric CO2 amplitude rather than for the amplitude itself.

* Results

- Page 6, 30: It is not clear when you compare the 425 ppm simulated by CESM to the observed 391 ppm in 2010, over which period the drift occured (missing sink). This would need to be clarified so that we see more how much is he missing sink per year ?

- Page 7, L11: As I said above, you mention the “imposed emission scenario” but this is not detailed in the experiment section ?

- The change in surface temperature of 6K in 2100 and then 11K by 2300 makes me wonder about the prediction of the CO2 amplitude increase. With such large temperature change after 2100, neglecting permafrost melt and potentially large natural vegetation change in the artic may be severe limitation ? At least this should be discuss to gain confidence that the other effect accounted for are the primary ones ?

- More generally the fact that the model simulate only $\frac{1}{2}$ of the seasonal cycle atmospheric amplitude at high latitude is probably a strong limitation to study the “drivers of the amplitude increase”. This should be discussed in more detail. Such a bias has probably large implications on the relative contribution of atmospheric CO2 increase, versus climate and land use change ?

- Page 8, L28: Why do you think that you still obtain a strong fertilization effect on the amplitude increase even given that CLM4CN has the lower fertilization effect of last CMIP5 models? Maybe you should explain a bit more which processes are contributing? Only the GPP increase? or other effects linked to autotrophic and heterotrophic respirations ?

- Climat change effect (section 3.3.2): I feel that not enough insight on the processes that lead climate change to impact the changes in atmospheric CO2 amplitude are given ? What is the role of the different respiration terms versus photosynthesis ? Do you see different contribution between grass and tree PFTs? What are the mechanism in CLM4CN that explain the contribution (sensitivity of the maximum photosynthetic uptake to temperature ?)

- LUC effect (section 3.3.3): It seems strange to mention that “the model is providing contrary results to previous studies” with the explanation that it does not properly treat cropland! At least we need a discussion to show that the current LUC effects are not completely wrong given such “model shortcut”. The authors should detail why they think the other component of the LUC effect may be important and why “their message about LUC effect” is still a valuable one ?

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- Page 9, l34: Precise over which period the growing season length increased by 1 month. 0verall the section 3.3.4 on the growing season length is not bringing much information. You could explain what contributes in the model to the change in growing season length (earlier starts or later end of the season). As for the tropic and the argument on the water use efficiency, you could provide more support by discussing how the soil moisture has evolved in the simulation with climate change.

* Discussion:

- Page 10, l22-23: You mention that the CESM has probably a too strong CO2 fertilization effect. This is not intuitive as you previously mentioned that CLM4CN has the lowest fertilization effect from the CMIP5 suite of model and that it provides a too low mean amplitude and mean amplitude trend for high latitude. The reasoning and conclusion should be more detailed as it is not intuitive. You can have several compensating effects so that the fertilization in the model is not too strong. Also, what is potentially missing is a discussion of the fertilization effect in CLM4CN with respect for instance to "FACE" experiment to put in perspective the results and conclusion drawn for the 23 century.

- Page 10, l28: You mention that LUC reduced the amplitude of atmospheric CO2 seasonal cycle, contrary to previous studies. You should indicate why the simulation with CESM provides new plausible information, given that you have mentioned that "treating crop as grassland" is a severe limitation (see my comment above). You have to provide some explanation on why you think the results with CESM provide a new perspective with respect to LUC. Basically what was the typical LUC that is contributing to such decrease, through which processes, . . . ?

- Page 11, last paragraph: Further discussion on the fact that "the results indicate that there is no high-temperature tipping point at which terrestrial productivity declines" would be valuable. Does this mean that the temperature dependence of the maximum photosynthesis peaks at high enough temperature threshold? or is it linked to the

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nitrogen cycle ?

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