Responses to Reviewer 2

We thank the reviewer for her/his constructive review of our manuscript. We found the comments very helpful, as they revealed that in the revised manuscript we must revise our language to show the insights gained in understanding how the mean annual cycle of CO_2 changes in response to climate and environmental drivers in a fully coupled ESM. The review underscored that the rationale for our study was not made clear, and we will remedy this in the revised manuscript. Here, we respond point-by-point to the reviewer's comments (Times New Roman font) with our rationale and proposed modifications to the revised text (Arial font).

The paper describes an analysis of potential drivers of multi-century trends in the seasonal cycle amplitude of the atmospheric CO_2 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 CO_2 seasonal cycle amplitude at high northern latitudes over the past 60 years; In a series of studies trying to disentangle the drivers of the observed increase in atmospheric CO_2 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.

While our study follows from several papers [e.g., Randerson et al. (1997) and Graven et al. (2013)] that showed that the mean annual cycle of CO_2 at high northern latitudes has increased steadily since measurements began in 1958, the goal of our paper is less to attribute drivers of the observed increase and more to test the abilities of a prognostic ESM to simulate the increase and to explore whether the ESM predicts nonlinearities or tipping points in the long-term increasing trend as climate in the model continues to evolve past the present-day. The climate and biogeochemical communities have invested tremendous time into the development of fully-coupled, mechanistic models, and rarely has a multi-decadal phenomenon such as the long-term CO_2 amplitude increase been observed in nature and therefore provided an opportunity to test a multi-decadal model in a fully-coupled, prognostic model.

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 CO_2 seasonal amplitude). The coupled climate–carbon cycle 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 CO_2 cycle) requires more caution when discussing the relative contribution of all potential drivers of the observed amplitude change (CO_2, climate, agricultural changes, . . .).

We agree with the reviewer that more discussion of biases in the version of CESM run for this study requires additional attention in the reviewed manuscript. We also recognize that we need to reframe discussion away from "what drove the observed amplitude" and more toward "what nonlinearities are present in a prognostic ESM that influences its ability to simulate multi-decadal through multi-century trends in coupled climate-carbon cycling?".

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...

i) We thank the reviewer for her/his helpful comments here that prompt us to recognize the need for us to provide better framing for our study's motivation and results. The use of a coupled ESM is crucial for our major goal, which is to explore whether there are changes in drivers of the mean annual cycle amplitude in a future climate change. We will add the following text to the introduction: "The use of a coupled model allows us to simulate the co-evolution of physical climate and biogeochemistry using a self-consistent framework. This is crucial since carbon fluxes are inherently linked to the physical climate; for example, a

change in GPP will be associated with changes in evapotranspiration, which feeds back on metrics such as humidity, cloud cover, and precipitation. Moreover, in a fully prognostic model, both climate and carbon cycle diagnostics are free to evolve rather than being tied to input data sets that reflect the contemporary climate."

... 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.

ii) In response to this comment, and some comments from Reviewer 1, we plan to add additional figures to demonstrate the pulse response methodology and validation against the full-transport land CO_2 field simulated by CESM. We will also add the text included in our response to Reviewer 1 to better explain and validate the method.

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 CO_2 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).

We address these drivers in our responses to the reviewer's individual comments below.

I thus recommend major revisions prior to consider that such work brings new information for the understanding and the prediction of the atmospheric CO_2 seasonal amplitude changes.

Main comments

* Introduction:

- The authors provide a nice literature review of articles that have tried to explain the increase of atmospheric CO_2 amplitude. However, they lack the recent study by Hakihiko Ito et al., 2016 in Tellus " Decadal trends in the seasonal-cycle amplitude of terrestrial CO_2 exchange resulting from the ensemble of terrestrial biosphere models" Note that such study is using an ensemble of process-based land surface models, including two versions of CLM (CLM4 and CLM4VIC) which are probably close to CLM4CN 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.

We thank the reviewer for bringing this article to our attention, and will include discussion of this article in the revised manuscript. We note that this article was published after our manuscript was published in *Biogeosciences Discussions*, so we did not have the opportunity to include it in our initial submission. Likewise, another relevant paper was published in *Biogeosciences Discussions* a few days after our initial submission. These papers are valuable in that both us multi-model ensembles (MsTMIP and TRENDY, respectively) to consider changes in the mean annual cycle of land-atmosphere carbon fluxes.

An important difference between these papers and our manuscript is that these papers focus on the seasonal cycle of fluxes, rather than propagating those fluxes to atmospheric CO_2 concentration, which we focus on in our paper. The propagation of fluxes to atmospheric CO_2, even using a simple method such as the pulse response code that we use, is important since atmospheric transport plays a major role in the spatial gradient in atmospheric mean annual cycle trends. We also note that in terms of

understanding future observations, we cannot directly observe GPP (although promising remote sensing tools such as chlorophyll fluorescence are being developed) or ecosystem respiration at large spatial scales. Thus, using a model such as CESM to develop hypotheses about how individual process might change the quantity that *is* directly observable (atmospheric CO_2) is a valuable exercise, in our opinion.

We will revise the introduction to include discussion of these manuscripts and to differentiate our approach from these papers. The following text will be inserted before the paragraph on p3, L10: "Several recent papers have considered how the amplitude of NH net carbon exchange has changed over the historical period. Ito et al. (2016) analyze MsTMIP terrestrial ecosystem models to determine how atmospheric CO_2, climate change, and land use affect the NH flux amplitude for the historical period, and Zhao et al. (2016) analyze the net terrestrial flux to the atmosphere in TRENDY models. Both of these studies find that CO_2 fertilization is the strongest driver of increasing ecosystem productivity and thus the amplitude of the net carbon exchange in the NH. The results from these ensemble-based analyses provide a useful basis for comparison for our analysis of a single, fully coupled ESM. A significant difference between the approach used by these papers and our study is that they consider the net flux amplitude, whereas we propagate fluxes using an atmospheric transport operator to determine the influence on latitudinally resolved atmospheric CO_2 fields. We hypothesize that fingerprints of climate change or CO_2 fertilization may be evident in different latitude bands in the CESM output."

This statement will be followed by the following text, to be included in the discussion section of the revised manuscript:

"CESM simulations show that the major drivers of the mean annual cycle amplification leave differential imprints on atmospheric CO_2 in different latitude bands. For example, CO_2 fertilization leaves the largest imprint in both absolute and relative terms on midlatitude CO_2, whereas climate change may amplify high latitude CO_2 while having a near-neutral impact on CO_2 annual cycle amplitudes south of 60°N (Fig. 10). These fingerprints may be useful for developing hypotheses regarding observed trends and determining future observational strategies to monitor carbon-climate feedbacks."

- 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.

We thank the reviewer for this constructive comment. An implicit premise of our study was that the drivers of changes in the mean annual cycle between 1958 and 2013 need not continue to drive changes in the mean annual cycle of CO_2 into the future. Fertilization impacts could saturate, while further increases in temperature or related changes in drought conditions may actually reverse trends in seasonal productivity. We recognize that we need to make this premise more explicit in the revised manuscript. The Forkel et al. (2016), Ito et al. (2016), and Zhao et al. (2016) studies all focus on explaining only the historical trend, not future projections. We choose to study this topic in a single climate model so that we can in more detail analyze the regional contributions by driver to future changes in the mean annual cycle amplitude.

In the Ito et al. (2016) paper, a fair amount of attention is given to the idea that the mean annual cycle strength correlates with the net terrestrial sink strength. Because the simulations were run to 2300, we are able to determine the time period in CESM where this statement is no longer true. In the extended concentration pathway simulations, the mean Northern Hemisphere CO_2 amplitude is correlated with increased Northern Hemisphere carbon uptake (using NEP and neglecting land use change, disturbance, and harvest fluxes) through ~2150, at which point NEP shows significant declines in the Northern Hemisphere while there are only small changes to the amplitude (Fig. SB1).
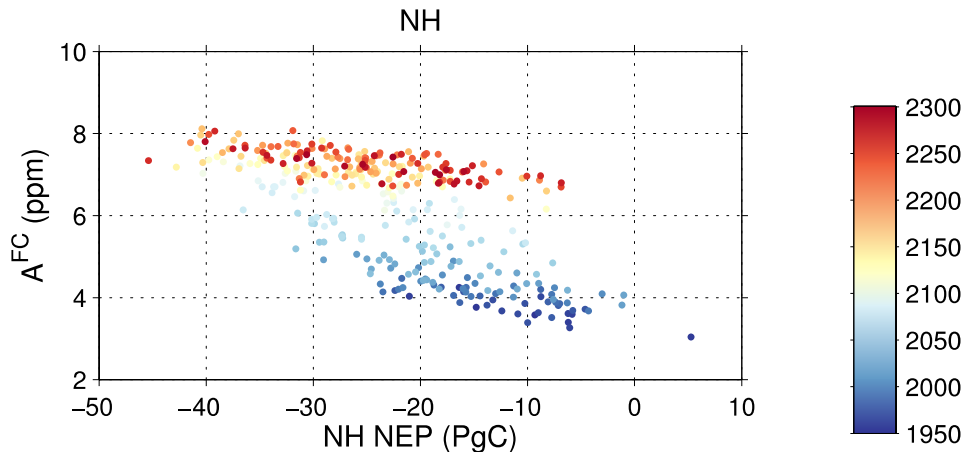
Fig. SB1: FullyCoupled atmospheric CO_2 annual cycle amplitudes (A^{FC}) versus NEP averaged over the NH and shaded according to simulation year. Negative NEP values indicates net carbon uptake by the land surface.

Based on the reviewer's comment, we plan to add a section on " Uncertainties and future model needs" to the discussion section to the paper, in which we explicitly discuss how lack of permafrost parameterizations, vegetation successional patterns, active human management, etc. affect the simulation results. It is exciting that CESM2, in preparation for the CMIP6 experiments, has much improved representation on frozen soil carbon and temperature interactions (Koven et al., submitted) as well as land management representation (P. Lawrence et al., BGCWG February Meeting presentation). Moreover, a version of CLM-ED will be released this fall. These new developments present opportunities for follow-on studies to explore the impact of these "missing" interactions. However, we feel that these comparisons are outside the scope of the current paper and are best reserved for a future study. Our paper, instead, provides a baseline analysis of the CESM1.

We also thank the reviewer for the suggestion of explicitly including questions that the paper will address. We will include the following questions at the close of the "Introduction" section of the revised manuscript:

"The questions guiding our analysis of CESM extended concentration pathway simulations are as follows:

1. Does the relative importance of drivers of the CO_2 amplitude trend change after 2100? For example, do we see evidence of saturation of the CO_2 fertilization effect or evidence of a climatic tipping point after which the CO_2 amplitude declines?

2. Do the regional contributions to CO_2 mean annual cycle trends change in response to large changes in climate?

3. Does the CO_2 annual cycle amplitude scale with the hemispheric carbon sink from NEP as climate and atmospheric conditions evolve in the future?"

- 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?

Since the goal of the paper is to explore future trends the use of a prognostic climate model is crucial since we do not have some bias-corrected estimate for climate change. Given the scientific questions we have added to the paper, and our response (above) for why CESM is a good tool for this analysis, we think that our approach has now justified in the manuscript text.

- 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.

We agree with the reviewer that there are limitations to the CESM configuration for the science questions we address in our paper, including the lack of permafrost modeling and land management. We note that ESM development is a slow and steady process, and that there is value to fully exploring processes in CESM1–the first fully coupled version of this model. Moreover, comparisons among different model versions are crucial, so careful analysis of CESM1 will provide better insights and science questions for subsequent analysis of CESM2.

We will add the following text to the introduction P3, paragraph ending on line 22: "The CESM provides a unique platform for exploring these questions in that it is one of the few prognostic ESMs to include coupled carbon-nitrogen biogeochemistry and diagnostic atmospheric CO_2 variability."

- 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.

Per the reviewer's suggestion, in the revised paper we plan to introduce the following questions:

1. Does the relative importance of drivers of the CO_2 amplitude trend change after 2100? For example, do we see evidence of saturation of the CO_2 fertilization effect or evidence of a climatic tipping point after which the CO_2 amplitude declines?

2. Do the regional contributions to CO_2 mean annual cycle trends change in response to large changes in climate?

3. Does the CO_2 annual cycle amplitude scale with the hemispheric carbon sink from NEP as climate and atmospheric conditions evolve in the future?"

* 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.

CLM4CN does not include dynamic vegetation. We will include the following text in the model description (Section 2.1): "These simulations were run without dynamic vegetation, which potentially also damps feedbacks that could contribute to changes in the CO_2 annual cycle through 2300."

We will also add text to the "Uncertainties and future model needs" section that will be added to the discussion: "The lack of dynamic vegetation in this version of CESM contributes some uncertainty to these results. Tree cover is expected to expand further northward with climate change (e.g., Lloyd et al.,

2005), which may contribute to the long-term increase in NEP flux amplitude within high latitude ecosystems. In contrast, drying at lower latitudes may lead to replacement of trees with grasses and subsequent decreases in NEP amplitude. Thus, the balance of these processes on the overall flux amplitude and spatial variability in the atmospheric CO_2 trend is uncertain. An ecosystem demography version (CLM-ED) is currently being developed that would permit successional patterns in response to environmental change. We consider the documentation of trends in the static-vegetation configuration presented in this manuscript to be a crucial first step toward eventually determining the sensitivity of land-atmosphere biogeochemical couplings in more sophisticated, future configurations of the CESM model."

* Experiment:

- The authors mention using "impose CO_2" 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 CO_2 mole fraction values were diagnostic only. ...". I thus think that the "experiment section" should describe more precisely what was done and differences between imposed CO_2 and diagnostic CO_2.

We will include this description in Section 2.2 "Experiments". The revised text will read: "The mole fraction of CO_2 in the atmosphere is prescribed according to the RCP8.5 and ECP8.5 scenario described by Meinshausen et al. (2011), and it is this value that controls radiative forcing as well as CO_2 fertilization. However, the CESM retains a separate, spatially-varying CO_2 tracer that is a diagnostic passive tracer of land, ocean, and fossil fuel carbon fluxes; the additional carbon exported from the surface to the atmosphere does not exert any forcing on the climate."

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

We will revise section 2.4 to state we used monthly mean NEP from the CESM to derive atmospheric CO_2 from the pulse response function.

- 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 CO_2 amplitude rather than for the amplitude itself.

We have included revised text and figures in the response to Reviewer 1 to address these points. We anticipate that surface pattern differences are a minor source of disagreement since Nevison et al. (2012, GMDD) tested a similar pulse-response framework for fossil fuel emissions. The fossil emissions were distributed according to NEE, which represents a gross mismatch, but still had an r^2 value of 0.8 compared to a full transport simulation. Mismatches between CESM and CASA terrestrial fluxes are likely much smaller, although we have mentioned this factor as an additional source of error in the revised text.

* 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 occurred (missing sink). This would need to be clarified so that we see more how much is he missing sink per year?

Previous results have shown that the missing sink for atmospheric CO_2 in CESM is attributable to weak

uptake in both the land and the ocean, and that this sink is relatively smooth with time.

We will revise the first paragraph of section 3.1 to conclude with "We note that the drivers of the amplitude increase during 1985—2013 were simulated to different levels of fidelity: the NH atmospheric temperature increase over land was roughly equivalent (1.02 K vs 0.95 K in the NCEP-NCAR reanalysis (Kalnay et al., 1996)), but the NH atmospheric CO_2 mole fraction in CESM was too high (425 ppm vs 391 ppm derived from observations in 2010). Previous analysis of CESM shows that the high CO_2 bias is attributable to persistent weak uptake in both land and ocean (Keppel-Aleks et al., 2013; Long et al., 2013)."

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

We will clarify in the experiment section the details of the imposed emission scenario as described in our previous response.

- The change in surface temperature of 6 K in 2100 and then 11 K by 2300 makes me wonder about the prediction of the CO_2 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

We agree with the reviewer that more discussion of this limitation, beyond noting the absence of permafrost dynamics in Section 2.1, should be addressed in our paper, and will add the following discussion to the "Uncertainties and future model needs" section of the revised manuscript: "The lack of permafrost dynamics likely has a large impact on CO_2 annual cycle trends, especially later in the simulation when global mean temperature has increased by over 10 K in the fully coupled simulation. Ongoing model development in CESM includes improved representation of permafrost carbon (Koven et al., 2015), and thus future model configurations will provide an improved tool for investigating a process that may provide one of the tipping points we identify in our key science questions."

- More generally the fact that the model simulate only 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 CO_2 increase, versus climate and land use change?

Many CMIP5 models exhibit the same bias (e.g., Zhao et al., 2016) show that the mean TRENDY model shows a 40% deficit in the annual mean). Since we intend the study to be an examination of what a fully prognostic model can tell us about trends, tipping points, and our current ability to simulate these interactions, we feel there is still value in quantifying drivers of trends within CESM1.

We will add the following text to Section 3.1, Line 27: "Although the CESM simulates low mean annual cycle amplitude throughout the NH, we note that many land models have a low bias in their simulated fluxes. For example, TRENDY land models show a 40% deficit in the magnitude of the seasonal cycle (Zhao et al., 2016)".

- 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?

We will add the following text to clarify that GPP is the main driver of the trends:

"Enhanced GPP seasonality appears to drive to the amplification of the atmospheric CO_2 seasonal cycle over northern temperate and boreal regions during 1950—2300. In midlatitude temperate regions where CO_2 fertilization drives the CO_2 seasonal cycle amplification, the seasonal amplitudes of GPP, HR and AR increase from 1950 to 2250, but the magnitudes of and increases in GPP seasonal amplitude are larger than those of HR, AR, and NEP in the FullyCoupled, NoRad, and NoLUC simulations. For example, in eastern temperate North America (ETNA), FullyCoupled GPP seasonal amplitudes increase from 6.8 PgC in 1950 to 11 PgC in 2250, while HR amplitudes increase from 0.85 PgC to 1 PgC, and AR amplitudes increase from 4 PgC to 7.6 PgC. The absolute increases in the seasonal amplitudes of GPP and total respiration (AR+HR) are, respectively, 2.5 and 2.4 times larger than the increase in the NBNA NEP amplitude during this period. Moreover, we find that GPP in high latitude regions, where climate change is the dominant contributor to amplification of net fluxes, is highly correlated with temperature. In the pulse regions that comprise our broader Arctic and boreal zones, GPP continues to increase with temperature until surface air temperatures surpass ~300 K."

- Climate 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 CO_2 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 mechanisms in CLM4CN that explain the contribution (sensitivity of the maximum photosynthetic uptake to temperature)?

We address the reviewer's comment in the response/text above.

In addition, we will include an analysis of the changes in PFT cover that contribute to the reduction FullyCoupled atmospheric CO_2 seasonal amplitudes from land-use change (LUC). As stated in the Methods section 2.2, PFT fractions vary on an annual basis from 1850—2100, then are held at 2100 values through 2300 in the FullyCoupled simulation. In the NoLUC simulation, PFT fractions are held at 1850 values. Crops (treated as unmanaged grass), needleleaf evergreen trees, and grass PFTs cover most of the NH boreal and temperate vegetated land. Between 1850 and 2100, boreal, temperate, and subtropical crop cover increase, while grass and needleaf tree cover decrease. Therefore, the decrease in the NH atmospheric CO_2 seasonal amplitude in response to temperate and boreal LUC reflects the fact that needleleaf evergreen tree and grass cover in these regions is lower in FullyCoupled than in NoLUC, resulting in lower GPP and smaller NPP seasonal amplitudes.

- 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?

We agree that prescribing LUC and treating crops as unmanaged grass may produce unrealistic responses in atmospheric CO_2 seasonality. The contrast between our results, which show that LUC reduces NH atmospheric CO_2 seasonality and other studies, such as Zhao et al. (2016), showing that LUC increases atmospheric CO_2 seasonality indicate that more sophisticated treatment of changes in vegetation cover and explicit representation of crop cover are likely necessary in the CESM.

- Page 9, l34: Precise over which period the growing season length increased by 1 month. Overall 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.

In section 3.3.4, we will add additional details to the text to state "The NH CO_2 annual cycle amplitude increase resulted not only from changes in the mean temperature affecting GPP, but also from lengthening of the growing season. We found that the growing season, defined as months with negative NEP (net terrestrial carbon uptake), increased for all NH terrestrial regions by about 1 month. The overall lengthened growing seasons accounted for 1—1.3% yr^{-1} of the high latitude net terrestrial carbon uptake after 2050, and up to 5% yr^{-1} of the midlatitude terrestrial carbon uptake after 2100. Thus, while this is an important contributor, it is secondary to increased mid-summer GPP."

We also expand our discussion to include analysis of soil water content in CESM, per the reviewer's helpful suggestion: "The driver of the increased growing season length was different for different ecoclimate regions. For regions north of 30°N, climate change was the driver of increased growing season length. In boreal and Arctic regions, climate change extended the growing season for an additional month in the fall. In contrast, midlatitude climate change facilitated an earlier start to the growing season in the spring (Fig. 11a). CO_2 fertilization was the major driver of changes in the growing season length in the subtropics, while climate change had the opposite effect. This result suggests that subtropical ecosystems in CESM are near a temperature optimum, but may be water-limited. In the FullyCoupled simulation, soil water content over the top three model layers, corresponding to 0.06 m depth, decreases in the Amazon and central America by 13% on average from 1950 to 2300. In the simulation without radiative forcing (but including CO_2 fertilization effects), soil water content increases by 1% on average in these regions, and suggests improved water use efficiency by vegetation. Thus, increases in water use efficiency associated with increased atmospheric CO_2 permit longer periods of carbon uptake."

* Discussion:

- Page 10, l22-23: You mention that the CESM has probably a too strong CO_2 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.

Upon further analysis, we have decided to remove this discussion from the paper as it is speculative and relies on relative, rather than absolute, trends in the amplitude.

- Page 10, l28: You mention that LUC reduced the amplitude of atmospheric CO_2 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, . . . ?

We will clarify the discussion to state that: "In contrast, land use change in CESM reduced the atmospheric CO_2 mean annual cycle amplitude throughout the NH, with the largest reductions over the mid- and high latitudes. Reductions in tree cover in the FullyCoupled simulation compared to the NoLUC simulation are associated with decreases in the net carbon flux amplitude and a negative trend in the CO_2 annual cycle amplitude. In the FullyCoupled simulation, croplands replace the lost tree cover. Several recent papers (e.g., Zeng et al., 2014; Gray et al., 2014) suggest that agricultural amplification, facilitated by irrigation and fertilization, may be an important driver of the observed mean annual cycle trend. In the CESM, however, crop cover is currently treated as unmanaged grass and thus these agricultural practices are not explicitly modeled, and thus do not mitigate the reduction in tree cover. These results

underscore that explicit consideration of human modifications may be necessary for prognostic models both to match observations and to provide realistic predictions of future changes. We note that in CLM, development is under way to represent irrigation and fertilization in croplands in future versions of the model."

- 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 nitrogen cycle?

We have removed this statement from the paper given that in individual pulse regions, there is a clear turnover in GPP with temperature. At high latitudes, this occurs above 300 K, and in the tropics, this occurs above 305 K. We note that temperature acclimation of GPP has recently been incorporated into CLM (Lombardozzi et al., 2015), but for CLM4, which we use, there is a clear decline. Instead, we close the paper with the "Uncertainties and future model needs" section prompted by the reviewer's comments.

"Although the results presented in this paper provide a useful look at the co-evolution of climate and the carbon cycle beyond 2100, several components of the model configuration induce substantial uncertainty into the results presented here. The lack of dynamic vegetation in this version of CESM contributes some uncertainty to these results. Tree cover is expected to expand further northward with climate change (e.g., Lloyd et al., 2005), which may contribute to the long-term increase in NEP flux amplitude within high latitude ecosystems. In contrast, drying at lower latitudes may lead to replacement of trees with grasses and subsequent decreases in NEP amplitude. Thus, the balance of these processes on the overall flux amplitude and spatial variability in the atmospheric CO_2 trend is uncertain. An ecosystem demography version (CLM-ED) is currently being developed that would permit successional patterns in response to environmental change. We consider the documentation of trends in the static-vegetation configuration presented in this manuscript to be a crucial first step toward eventually determining the sensitivity of land-atmosphere biogeochemical coupling in more sophisticated, future configurations of the CESM model.

The lack of permafrost dynamics likely has a large impact on CO_2 annual cycle trends, especially later in the simulation when global mean temperature has increased by over 10 K in the fully coupled simulation. Ongoing model development in CESM includes improved representation of permafrost carbon (Koven et al., 2015), and thus future model configurations will provide an improved tool for investigating a process that may provide one of the tipping points we identify in our key science questions."

Additional References

Ito, A., Huntzinger, D. N., Schwalm, C., Michalak, A. M., Cook, R., King, A. W., Mao, J., Wei, Y., Mac Post, W., and Wang W.: Decadal trends in the seasonal-cycle amplitude of terrestrial CO_2 exchange resulting from the ensemble of terrestrial biosphere models, Tellus B, 68, 2016.

Lombardozzi, D. L., Bonan, G. B., Smith, N. G., Dukes, J. S., and Fisher, R. A.: Temperature acclimation of photosynthesis and respiration: A key uncertainty in the carbon cycle-climate feedback, Geophys. Res. Lett., 42, 8624–8631, doi:10.1002/2015GL065934, 2015.

Long, M. C., Lindsay, K., Peacock, S., Moore, J. K., and Doney, S. C.: Twentieth-Century Oceanic Carbon Uptake and Storage in CESM1(BGC), J. Climate, 26, 6775–6800, doi:10.1175/JCLI-D-12-00184.1, 2013.

Lloyd, A. H.: Ecological histories from Alaskan tree lines provide insight into future change, Ecology, 86 1687–1695, doi:10.1890/03-0786, 2005.

Zhao, F., Zeng, N., Akihiko, I., Asrar, G., Friedlingstein, P., Jain, A., Kalnay, E., Kato, E., Koven, C. D., Poulter, B., Rafique, R., Sitch, S., Shu, S., Stocker, B., Viovy, N., Wiltshire, A., and Zaehle, S.: Role of CO_2, climate and land use in regulating the seasonal amplitude increase of carbon fluxes in terrestrial ecosystems: a multimodel analysis, Biogeosciences Discuss., doi:10.5194/bg-2016-121, in review, 2016.