Biogeosciences Discuss., 12, C3307–C3313, 2015
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Biogeosciences
Discussions
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Interactive comment on "Global soil nitrous oxide emissions in a dynamic carbon–nitrogen model" *by* Y. Y. Huang and S. Gerber

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Received and published: 3 July 2015

Reviewer:

The authors added a new soil N2O emissions module to the dynamic global land model LM3V-N, and tested its sensitivity to soil moisture regime, as well as its responses to elevated CO2 and temperature. However, I am not sure what the main objective of the paper is – whether this was mainly a model development paper or whether they wanted to conduct different sensitivity analyses. As noted by the other two reviewers, I think this paper needs major revisions before it can be published. In my opinion, the most important is to: 1) include more analyses instead of the speculations presented in its current status; and 2) highlight the original contributions in this paper, specifically illustrating what is different from what already has been published in Xu-Ri et al., 2012.

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Response:

In the new manuscript we hope that we clarify that we are building on existing nitrification/denitrification modules and discuss how this implementation bears out in this specific coupled carbon- nitrogen cycle model. As pointed out by Beni Stocker, these processes are subject to how the larger plant-soil cycle is implemented. To achieve this goal, we added a set of new analysis and tested the response of N2O emissions to assumptions related to N availability for nitrification-denitrification through altering biological N fixation fluxes, limiting dissolved organic N and fire volatilization N losses and changing plant N uptake strength. The corresponding analyses are in presented in sect. 2.2.3 and sect. 3.4 (Sensitivity to N cycling processes and parameterization) and the discussion in the revised manuscript. Our modeled response to CO2 fertilization is different from Xu-Ri et al. (2012). Xu-Ri et al. (2012) suggests a positive response globally or from tropical forest based on histroical simulations and combining the interaction with climate change, while we argue for a negative response from tropical forest in the first three decades of imposing a doubling of atmospheric CO2 (568 ppm).

Reviewer: I first list some major concerns, followed by minor comments. Major points: The authors argue in the abstract ln. 7-9 on p. 3102 that “[t]he model was capable of reproducing the average of cross-site observed annual mean emissions, although differences remained across individual sites if stand-level measurements were representative of gridcell emissions.” It is not obvious how they concluded that the model was indeed capable of reproducing the observed emissions. From the Figure 3, it is also not clear if the model is capable or not.

Response: We now add more comparison against data, specifically we also compare time series for a suite of site against the model. This allows us to discuss the model results in much more detail. For example, we show and discuss in the text, that we do not capture the entire breadth of N2O emissions across sites, and also within particular sites. Our abstract reads now “Results extracted from the corresponding gridcell (without site-specific forcing data) were comparable with the average of cross-site observed annual mean emissions, although differences remained across individual sites if stand-level measurements were representative of gridcell emissions.". While mismatches uncover model deficiencies, a point by point evaluation also bears the problem of scale mismatches, and issue raised by other reviewers and discussed in the text.

Reviewer: I would expect to see more rigorous model-obs comparisons, if this is a model development/validation paper. As the second reviewer suggested, I would also like to see hourly/monthly comparisons at multiple sites, and I find it odd that the model is run at "an annual time step" as they state on ln. 8-9, p. 3113. Why don't they get the annual average from their half-hourly simulation?

Response: The model is run with the fastest time-step of half an hour. The results state on ln. 8-9, p. 3113 (from original manuscript) is the annual average from the half-hourly simulation. To clarify, we reworded to "Modelled N2O emissions capture the average of cross-site observed annual mean emissions" (ln. 4-6, p. 16, revised manuscript). We further added comparison based on monthly and daily site measurements to sect. 2.3 and 3.3 of the revised manuscript..

Reviewer: Also, I recommend that they at least add their modeled values in Table B1 as well, so that the reader can directly compare their modeled values to the observations.

Response: We followed the suggestion:. Table B1 is moved to Table A1, and modeled values are added.

Reviewer: With regard to soil moisture, why does Figure 3 use different methods for the different data sets? I understand that there are three methods that the authors used for each of the three different data sets but it does not make much sense to do a model-obs comparison in a panel, using method 3 for part a and method 2 for parts b and c. Why not use one of the methods for all parts? If the authors agree that soil moisture values larger than 0.6 are not reasonable, what about the validity of the maximum water method that leads to a global mean WFPS higher than 0.6 (Figure 4)?

BGD

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Response: We have three methods for each of the three different data sets. LM3V-SM does not allow soil water to accumulate beyond field capacity. Meanwhile, the other two data sets (NOAH-SM and ERA-SM) are products to emulate observed soil moisture where soil water can transiently be stored above field capacity. Based on our understanding of these soil water data sets, we believe WFPS is more accurately represented by method 3 for LM3V-SM (part a), and by method 2 for NOAH-SM (part b) as well as ERA-SM (part c). Therefore, we use different method for different soil moisture data set. WFPS higher than 0.6 are generated by the two external soil moisture data sets (NOAH and ERA) through the maximum method, which we mention in the text is less appropriate to use for these data, nevertheless, they provide useful information in terms of the sensitivity to the soil moisture and its parameterization.

Reviewer: I also found that there are some statements in this paper that should be better justified. First, on ln. 5, p. 3115 authors state that "[t]he negative impacts (reduced N2O flux), which are also reported from manipulative experiments, are likely from increased plant N and immobilization demand under CO2 fertilization, reducing N availability for nitrifiers and denitrifiers" but is this what they see in the model? I believe they can also draw a similar graph, illustrating plant N and immobilization rate in time-series to see if this is indeed the response they are seeing in the model. The same goes for the positive impacts. I think it is important to see if the litter production and soil moisture have been increased, as well as stomatal conductance and leaf transpiration reduction, as they imply in the paper.

Response: This is a great suggestion. We inserted a new figure (Figure 8) into the revised manuscript. The figure compares the global mean litter pool size, plant nitrogen uptake rate, transpiration and soil water content in the root zone between simulations without and with CO2 fertilization. Averages of global means over 100 years show an increase of plant nitrogen uptake rate, litter pool size and soil water content, and a decrease of transpiration due to CO2 fertilization effect.

Reviewer: I'm not sure I understand the reasoning behind the statement on ln. 14-

16, p. 3118: “Patterns of seasonality, and the correlates between N2O emissions vs. temperature and soil moisture suggest that moisture is the dominant driver of N2O emission in tropical regions and soil temperature critical elsewhere.” What does “dominant” mean in this case? I think that in order to make such a statement, one needs to show the impact of different variables that are important and how that affects their N2O emissions.

Response: According to other reviewer’s suggestion, the correlation analysis is removed from the manuscript.

Reviewer: The authors write on p. 3104 that “[s]imulations with LPJ-DyN and O–CN demonstrated a positive response of N2O emissions to historical warming and a negative response to historical CO2 increase, globally. This negative CO2 response seems to be in disagreement with one meta-analysis of manipulative field experiments showing an increase in N2O emissions at elevated levels of CO2 (Zaehle et al., 2011; Xu et al., 2012; van Groenigen et al., 2011). The discrepancy in response to global change factors needs to be addressed both in models and in the interpretation of manipulative field experiments.” It seems that authors are misinterpreting the work of Xu-Ri et al. (2012) (which authors write as Xu et al, (2012)). Xu-Ri et al. (2012) states that “[i]ncreasing CO2 generally enhanced the N2O emission in tropical and temperate moist forests, whilst reducing the N2O emission in some other regions (Fig. 6),” which is essentially the same as the argument made in the current paper. I think it would be helpful if the authors could clarify what it is that they are arguing that is different from the conclusions in the Xu-Ri et al. (2012), as this was not obvious to me.

Response: Fig. 6 of Xu-Ri et al., (2012) displays the simulated global 20th century trends of annual N2O emission in simulations with (a) CO2 and climate change and (b) fixed CO2 concentration. Xu-Ri et al. (2012) states “in many tropical regions, CO2 and climate change combined synergistically to increase N2O emission”, based on their Fig .6. However, the effect of CO2 alone cannot be derived from their Fig. 6. As further illustrated in their Fig. 7, CO2 plus interaction with climate result in a posi-

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tive response of global N2O emissions, but historical CO2 change alone (single factor) causes a slight decrease in historical N2O emissions. We agree our interpretation of their result is inaccurate without explicitly state whether it is CO2 effect alone or CO2 plus interaction with climate. In response to this and another reviewer's suggestion, we rewrote this part as: "Simulations with O-CN demonstrated a positive response of N2O emissions to historical warming and a negative response to historical CO2 increase, globally. While CO2 and interaction with climate change resulted in an increase in historical and future N2O emissions from LPJ-DyN(Xu-Ri et al., 2012) and its application (Stocker et al. 2013), respectively, historical CO2 change alone (single factor, from Fig. 7 of Xu-Ri et al., (2012)) caused a slight decrease in historical N2O emissions."

We do not think our argument for CO2 fertilization response is the same as Xu-Ri et al., (2012). Xu-Ri et al., (2012) argues for a positive response from tropical forest based on historical simulations and combining the interaction with climate change, while we produced a negative response from tropical forest in the first three decades of imposing a doubling of atmospheric CO2 (568 ppm). The negative response from tropical forests is the major cause of the global negative responses to CO2 fertilization. While Xu-Ri et al., (2012) conducted historical simulations, we focus on step changes of CO2 that mimic most of the field experiment of CO2 fertilization (e.g. FACE).

Reviewer:

Minor comments: I am a bit confused about the Figure 1. The MEI values (http://www.esrl.noaa.gov/psd/enso/mei/table.html) are higher than 0.6 on several occasions between 1975 and 1980 (1976 Jun-Oct, 1977 Jun-1978 Mar, 1979 Jul-1980 Jul) and yet, this figure is only showing a one gray zone during that period. Also, it is unclear which WFPS method was used for this calculation. It would be helpful if they showed the range in interannual emissions, based on the 3 different methods and datasets they used. The same goes for Figure 2.

Response: Agree. The revised Figure 1displays three sets of annual global N2O emissions corresponding to three soil moisture datasets. We used the average of 12 monthly values to represent MEI of a year. Grey zones indicate the years with mean MEI greater than 0.6. And grey areas do not incorporate any sub-annual information.

Reviewer: Are RNOx:N2O and RN2:N2O values calculated at every time step for every grid cell? Or how does it work?

Response: Yes. RNOx:N2O and RN2:N2O values are calculated at every time step for every grid cell, which we now expliclitly mention in the revised manuscript.

Reviewer: L. 4, P. 3102 – typo "reponses" L. 19, p. 3109 – typo "equalibrium". l. 1, p. 3117 – typo "exsit" l. 5, p. 3117 – "constraint" to "constrain" l.29, p. 3117 – typo "oringinal" l. 8, p. 3119 – typo "aboitic". l. 13, p. 3119 – typo "unstand" l. 9, p. 3120, typo – "speicies"

Response: Thank you for catching those typos.

Interactive comment on Biogeosciences Discuss., 12, 3101, 2015.