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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

Y. Y. Huang and S. Gerber

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We would like to thank B. Stocker very much for his helpful suggestions and interests in our manuscript. We list our opinions point-by-point in response to his comments or suggests. Modifications to the manuscript can be tracked in the submitted MS.

Reviewer:

SUMMARY This paper describes the implementation of a model for inorganic soil nitrogen (N) dynamics within a Global Dynamic Vegetation Model that explicitly treats the interactions of the carbon (C) and N cycles. Results are presented from a simulation covering years 1970-2005 and for several sensitivity analyses (soil moisture, elevated CO2, warming). The model is assessed against observational data of N2O

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emissions from a set of observations that are collected for the present study. Apart from confirming global total N2O emissions are on the same order as previous studies suggested (the central estimate here is 6.82 TgN2O-N/yr), the authors conclude that "Improvement of soil hydrology is likely to significantly reduce the large un- certainties associated with soil N2O emission estimates".

Response:

Thank you for taking time reviewing our paper.

Reviewer:

This is a straightforward and honest model description and presentation of its performance and presents some valuable insights into the general model behaviour in response to basic environmental drivers (CO2, warming, combination of the two). This is essential for the interpretations of model results also in view of future studies addressing N2O emissions conducted with this version of the LM3V-N model. Benchmarking model performance and a concise description of implemented code should be considered best practice and the study presented here is a good attempt at this ideal.

Response: We appreciate the reviewer's positive comments on our study.

Reviewer:

But does it convincingly succeed at thoroughly describing the parameterizations and benchmarking the model performance? In this respect, I have some concerns which should be addressed in a revised manuscript. The present study may warrant publication if the authors address the issues raised below.

Response:

We have carefully considered all issues raised by this reviewer. Responses and revisions are provided accordingly.

Reviewer:

In summary concerns are: - Concerning difficulties of benchmarking a coupled system: Did the authors really look at the most important factors determining N2O emissions? - The authors did not attempt to decouple their new implementation of inorganic N dynamics from the behaviour of other model parts in which their "module" implemented. Therefore, results are subject to these other model parts.

Response:

We agree that N2O emission from our study is subjected to the performance of other model parts. Particularly the model is sensitive to processes that allow buildup of inorganic or mineral nitrogen (ammonium and nitrate), which happens if nitrogen (N) is limiting for decomposition and plant growth. The sensitivity to N limitation is due to the fact that denitrification is considered as a "weak" sink, where removal coefficients of plants and soils are much higher if there is sufficient demand for N. The second sensitivity is the fraction of N2O generated during nitrification. In this vein, we add sensitivity tests in sect. 2.2.3 and sect. 3.4 We now investigate model performance under altered N input from fixation, and changes in other N fluxes that affect inorganic N dynamics, the concentration of inorganic N in soils, and thus denitrification. These changes are associated with the following hypotheses: 1) The change in N fixation from a dynamical model that responds to N limitation to a static model based on reconstruction has the potential to add N critically above what is needed and remove a negative feedback that is inherent to LM3V-N. This addition also moves the dynamics of LM3VN towards schemes used in other models, where N fixation is scaled to net primary productivity (such as CLM), or transpiration (such as ISAM). 2) The sensitivity of excluding dissolved organic nitrogen reroutes some of the N that would be lost as organic form (DON, fire) through the mineral pool, and can therefore increase N2O emissions 3) Reduction in plant uptake strength leaves more available N for leaching and denitrification, or in other words increases the relative sink strength of denitrification vs. plant removal. 4) The parameter that determines the gaseous loss during nitrification can ultimately shift the competition for the overall available N because it removes N before

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it becomes available as nitrate. 5) allowing all N from fire to remain in the plant soil system also reduces unavailable losses, and increases potential denitrification losses due to resynchronization of plant demand and mineralization, and the overall fact that more N is retained in the system. Overall, these sensitivity experiments test how denitrification plays out within the larger soil-plant system and how the larger N cycle is linked to denitrification.

Reviewer

Presentation: For a model description and benchmarking exercise like the present study, the journal Geoscientific Model Development would suit even better than Biogeosciences. - The authors implemented a "module" for inorganic N dynamics, but the paper focuses only on N2O emissions. However, N2O emissions are governed by the inorganic N dynamics. Regarding the aim of this paper (model description/benchmark) these other processes warrant equal weight.

Response:

We appreciate the reviewer's suggestions for Geoscientific Model Development as a better choice. We have carefully considered the reviewer's suggestions, but we would like to emphasize that model development and benchmarking is not the only focus of our study. In particular, the denitrification scheme we implemented is used in other models. We think the question we are asking are more of what is the result of established denitrification routines into a different model knowing that each model has a different "philosophy" of the larger plant-soil N cycle. We also think that the comparison against different site should not be the central part, but we would like to put emphasis on the question what happens if one implements established denitrification routines into a different model. How does it impact N2O budget, N2O fluxes at different sites, the response to global change factors, and to how the water cycle is treated? Emphasizing that our model is not new, we think our attempt to answer these questions fits into Biogeoscience. In that sense we appreciate Beni's suggestion to evaluate N2O

fluxes with the larger N cycle in mind.

Reviewer

GENERAL COMMENTS WHY BIOGEOSCIENCES? The present study would fit the scope of Geoscientific Model Development (another open-access Copernicus journal with a high impact factor) perfectly. This would allow for a better reproduceability, re-usability and tractability of code developed here. GMD requires model code to be made public. Of course, making the entire LM3V-N code public may not be practical here and I am aware of the challenges of de-coupling individual model parts that are usually run in tight coupling with other model parts. However, this should not prevent development of parts of larger models to be published in GMD. A practical solution may be found to provide developed code as a module and some overhead to drive that module in a "demonstration mode". Could that be achieved? In this case, I strongly recommend publication in GMD. This is the best way to share innovations, advance science (and even get more citations). Also the data in Table B1 could be made publicly available in a convenient format. GMD provides a great platform to share such data.

Response:

We share the reviewer's opinion that public code ultimately is a great tool to advance science. We also have the reservation he mentioned, with respect to the practicality of doing this work. LM3V-N has a significant overhead and its implementation on a new platform is complex. The solution of a demonstration code could be an alternative, but since the nitrification-denitrification code is not new per se (just implemented in a different model) we feel there is little to be gained from a demonstration module. We appreciate the reviewer's suggestions for Geoscientific Model Development as a better choice. We have carefully considered the reviewer's suggestions, but think Biogeo-science fits our study also.

Reviewer

CHALLENGES OF BENCHMARKING A COUPLED SYSTEM Paper deals with a process (N2O emissions) that is very challenging to model. This is because of the C-N cycle system dynamics with "circular coupling" where response time scales of individual processes determine the system response on different time scales. It is inherently difficult to thoroughly benchmark such a coupled system. The challenge is that N2O emissions are dependent on all aspects of the C-N cycle.

Response

We appreciate the reviewer's acknowledgement of the challenges associated with this study, we have amended the text with insights how LM3V operates with respect to other parts of the N cycle in the method section. We expanded sensitivity tests that include now fixation, fire, DON losses, and plant uptake, next to the "classical" nitrification-denitrification parameters.

Reviewer

The study presented here appears to be subject to these problems as well. Benchmarking individual processes in a coupled system without actually de-coupling separate model parts may be misleading. In some instances (e.g., correlation analysis, Sect. 3.4; strong focus on sensitivity to WFPS) the analysis presented here is subject to this problem and it is confusing in what insight some analyses really provide.

Response:

We agree that correlation analysis does not provide much information and is removed from the revised version of the manuscript. However, we think WFPS is an important factor contributing to the uncertainty of terrestrial N2O simulations and is one of the focuses of this study. Because nitrification-denitrification requires spatially or temporal conditions alternating aerobic and anaerobic conditions, which is parameterized via WFPS, this is probably the single most important factor, after the N requirements of plant and soil have been taken care of. With the improvement of the manuscript, by

considering other factors, we can put the sensitivity to WFPS in a much better context.

Reviewer

In my understanding, N2O emissions are determined by two (largely independent) aspects:- denitrification/nitrification throughput; This scales linearly with substrate (nitrate and ammonium) pool size (their Eq. A1 and A4) which in turn this is governed by the balance of net mineralisation, plant N uptake and losses. It is thus affected by the whole system of C-N interactions. Benchmarking this aspect of N2O emissions thus requires a wide focus of benchmarked quantities. - fraction of N2O lost with denitrification/nitrification. This is determined by soil oxidation availability (their Eq. A8 - A11). This fraction is relatively uncertain.

Response:

We agree that N2O emission is affected by the whole system of C-N interactions, and is one of the reasons or advantage for us to analyze N2O emissions within a global C-N model. The fraction of N2O lost during nitrification is set as a constant of 0.4%. We test the sensitivity of N2O emissions to this fraction by setting this value to 4% or 0.04%. The model is most sensitive to this fraction compared to parameters regulating plant N uptake, nitrification and denitrifiction rates. However, this fraction is very uncertain based on limited field or laboratory studies. Goodroad and Keeney (1984) suggested a value of 0.1-0.2% , while Khalil et al. (2004) reported a range of 0.16%-1.48% depending on the O2 concentration. We applied a value of contant 0.4% in the default run which might cause large uncertainties in our results. The fraction of N2O lost from denitrification is taken from the empirical estimation from the DayCent model (the daily version of the CENTURY model), and has been assesed under different conditions (Del Grosso et al., 2000). DayCent has been widely applied in trace gas studies across terrestrial ecosystems. This fraction also embraces large uncertanties. We acknowledge this fact in our improved manuscript.

Reviewer

Thus, the challenge is that N2O emissions are dependent on all aspects of the C-N cycle. Soil moisture affects the amount of inorganic N subject to denitrification and nitrification. The strong focus of this study on assessing the model sensitivity to soil moisture (_water-filled pore space, WFPS) is thus questionable.

Response:

We agree with the reviewer that N2O emissions are dependent on various aspects of implementation of C-N cycle in our model. Based on the reviewer's suggestion, we add a comprehensive set of sensitivity tests in sect. 2.2.3 and sect. 3.4 (Sensitivity to N cycling processes and parameterization) to analyze the influence of N cycling processes other than nitrification and denitrification on N2O emissions. These tests include effects of biological N fixation, DON losses, fire, and plant uptake capacity.

Reviewer

The authors implemented a full representation of inorganic soil N dynamics (p.3106, l.1: "Here, we add a soil nitrification–denitrification module"). However, this paper puts a very strong focus on N2O emissions. As mentioned above, N2O emissions are governed by the inorganic N dynamics. I think, benchmarking N2O emissions would be more powerful, if observational constraints on other quantities determining the inorganic N dynamics of different levels be included. Examples of such quantities are: -inorganic N pool size (given net mineralisation rates) - N loss rates (given inorganic N pool sizes) - nitrification/denitrification rates (given inorganic N pool sizes) – sensitivity of nitrification/denitrification rates to different soil conditions.

Response:

We agree. The focus of this manuscript is on N2O emissions. However, N2O fluxes are strongly regulated by inorganic N dynamics. It is beneficial if other quantities regulating inorganic N dynamics are validated. One of the biggest factor that sets inorganic pool sizes are the sink strength of plant uptake (under N limitation), and the

presence/absence of N limitation (or in other models, how plant N status affect uptake). Soil N pool measurements can be helpful as an additional benchmark. However, as the author noted, it is the larger plant soil N cycle, including that sets plant N demand, and N limitation. These questions are great challenges for all models. Unfortunately, we do not have large scale observation data available for benchmarking the global model with regard to quantities such as inorganic N pool size, nitrification and denitrification rates.

Reviewer

SUBJECT TO PERFORMANCE OF LM3V-N This is in some respect related to the comments raised above. The authors test the model part representing inorganic N dynamics, as implemented in the LM3V-N model. However, some sensitivity analyses presented here are tightly dependent on the sensitivity of the LM3V-N model (Sect. 3.5). This requires at least a description of the general functioning of that model (How are major N input and loss fluxes represented? What leads to N limitation? What governs N fixation?)

Response:

We agree. Further description of the general N cycle is added to sect. 2.1.1 Main characteristic of LM3V-N. And we discuss it with respect to our newly added sensitivity analysis. Adding an established nitrification-denitrification model is indeed subject to the overall "philosophy" of the entire biogeochemisitry model. We believe, it is thus useful to evaluate N2O emission in this sense.

Reviewer

In my understanding, with inorganic N dynamics represented broadly equally (which is the case for all global vegetation models that simulate C-N dynamics and N2O emissions: DyN-LPJ, Xu-Ri et al., 2012; LPX-Bern, Stocker et al., 2013; O-CN, Zaehle et al., 2011), N2O emission sensitivity to CO2 and warming primarily depends on

the degree of progressive N limitation under environmental change (less N2O emitted in a N-scarce system). Here, these models' predictions diverge substantially. On one side,O-CN generally more N limitation under elevated CO2 (=increased plant demand), on the other side DyN-LPJ and LPX-Bern (pretty much the same) does hardly generate N limitation on a decadal time scale. This model behaviour is contingent on how N inputs into the system are simulated (we know that losses are broadly equal as they all rely on a DNDC-type model for inorganic N dynamics). O-CN simulates BNF using an empirical relationship with evapotranspiration. DyN-LPJ implies a BNF flux by holding soil C:N ratio constant, i.e., higher litter-to-soil C flux implies additional N brought into SOM, which is ultimately made available for plant N uptake after mineralisation. To interpret the results presented here, it is crucial to understand where in this spectrum of O-CN and DyN-LPJ this model is. The information provided in Sect. 2.1 ("BNF in LM3V-N is dynamically simulated on the basis of plant N availability, N demand and light condition.") doesn't provide sufficient insight to understand this crucial model characteristic.

Response:

The reviewer is spot on. BNF in LM3V-N is different from that of O-CN, LPJ-DyN and LPX-Bern. Further description related to BNF is added to sect. 2.1.1.5. And more details are available in Gerber et al. (2010). BNF in LM3V-N is active only when plant N requirement is failed to be satisfied by root uptake, and is adjusted according to plant N demand. LM3V-N assumes a tighter (smaller input and smaller losses) in preindustrial N cycling compared to O-CN, LPJ-DyN and LPX-Bern with smaller amount of BNF (72 in LM3V-N vs. 104 TgN yr-1 in O-CN) (Zaehle et al., 2010). The adaptive BNF also contributes to the tighter N cycling. However, in conditions of N limiation, there is considerable adjustment of BNF in response to progressive N limitation, as illustrated by ca. 2 times increase averaged over 100 years (Panel (3), Fig.8) under doubling of atmospheric CO2 level. This strong negative feedback via BNF alleviated N limitiation intially faced by tropical forests, and turns the negative N2O response to positive after

several decades. Deference in BNF is one of the major causes of divergent responses to CO2 fertilization between those models. Results related to BNF is added to sect.3.4 , 3.5 and discussion of the revised manuscript.

Reviewer

CORRELATION ANALYSIS IN SECT. 3.4 Are correlations derived from regressing the corresponding time series of the historical run? Temporal resolution (daily/monthly/annual)? I'm a bit confused about what such a correlation actually represents. Short term correlations don't necessarily represent the system's sensitivity to a certain input. I guess that's really what you are after here: understand the characteristics of the model - its sensitivity to different driving variables. Isn't this better covered by your analysis of step changes? The analysis presented here is particularly confusing in the case of the correlation between N2O emissions and Ammonium. I'm pretty sure that, if you would add a certain amount of Ammonium everywhere (N fertilization experiments), N2O emissions would increase not decrease - also in the model presented here. The temporal correlation presented here thus does not provide direct insights into the model sensitivities. I think, the confounding aspect is that there is also a time-scale dependence of such correlations (delayed response of some variables in the system). Another aspect that is confusing about the analysis presented in Fig. 5 is that some correlations are with variables that are directly or indirectly external to C-N cycling (temperature, soil moisture, GPP), while others are intrinsic quantities (nitrate, ammonium, etc). Regarding the negative correlation of N2O emissions with ammonium concentrations: This is confusing as Eq. A1 says that nitrification (_N2O emissions) and ammonium are directly proportional. I suspect that this counter-intuitive result is due to the fact that ammonium levels are low in the tropics due to the high plant N demand. At the same time, also net mineralisation rates must be quite large (is that so?) and nitrification rates must be high as well which implies high N2O emissions. Is the result presented here really indicative of what's driving N2O emissions?

Response:

We agree. The correlation analysis may provide some insights, but confuse when discussing mechanisms. The response to ammonium availability is such an example. On a side note, because nitrification is strong in LM3V compared to other sinks, any increase in nitrification strength will draw down ammonium concentration.

Reviewer

MODEL DESCRIPTION IN APPENDIX Appendix A contains "the heart" of this paper. This paper is primarily a model description and benchmarking exercise. The model is not applied to address a specific question or a particular period. I find it inconsistent with the scope of the paper, to put the actual model description (the "heart") into the appendix.

Response:

We gladly follow the reviewer's suggestion and move the appendix into the method section in the main text (sect 2.1.2 Soil N2O emission).

Reviewer

"Our simulation of N2O losses during nitrification–denitrification generally follows the "hole-in-pipe" concept". To my understanding, this concept refers to models that assume that gaseous N losses are proportional to net mineralisation rates. The model presented here assumes that N losses are scale with inorganic pool sizes (proportionally for nitrification - not really a loss term though) and with Michaelis-Menten kinematics for denitrification (not mineralisation rates). In my understanding, the model presented here is thus not a hole-in-the-pipe model.

Response:

The original "hole-in-pipe" model assumes that gaseous N losses are proportional to net mineralization rates. It describes the rate of nitrogen cycle as the amount of nitrogen flowing through the pipes. N2O leaks out of the pipes depending on nitrogen cycling rate as well as the size of the holes, determined largely by soil water

content(Firestone and Davidson, 1989). Our understanding is that this metaphor of "nitrogen flow through the pipe" is not constraint to net mineralization. Instead, it is generalized to nitrogen availability and can be indicated by various indices such as N mineralization, nitrification potential and the inorganic pool sizes (e.g. Davidson et al. (2000)). However, we agree with the reviewer that our expression is confusing. And "Our simulation of N2O losses during nitrification-denitrification generally follows the hole-in-pipe concept (Firestone and Davidson, 1989) with more detailed treatment of the N flux pipes and the leaky holes (gaseous losses) in the pipes" is deleted from the revised manuscript.

Reviewer

As a further remark on the "hole-in-the-pipe": Can't we say that the "hole-in-the-pipe" concept is simply wrong? In such a model, N losses are not affected by N demand. That is, if net-mineralisation is increased, losses are increased irrespective of whether demand for N uptake is increased. Hence, warming may not stimulate plant growth (in contradiction with observations) and elevated CO2 will tend to lead to a state of progressive N limitation as N losses are not reduced. Both are not match observational findings (Melillo et al., 2011; FACE results). Further, Davidson et al. (2007) present evidence that N2O emissions are indeed reduced when demand outweighs net mineralization and leads to depleted inorganic N pools. Maybe add this to discussion.

Response:

We agree. The mineralization based approach is added to Discussion, which states "If gross mineralization is used as an indicator of the rate of N flow in the "hole-in-the-pipe" concept and gaseous losses are propotional to mineralization, the inital negative response is unlikely to be detected. We found increased mineralization rate with increased litterfall under elevated CO2, while N availability is reduced from LM3V-N. The mineralization based approach is likely to predict an inrease of losses regardless of N limitation".

Reviewer

SPECIFIC COMMENTS p.3102 l.3-5: "With high temporal and spatial heterogeneity, a quantitative understanding of terrestrial N2O emission, its variabilities and reponses to climate change is challenging." âĚŸAĚĞTre wording to "Due to its high temporal and spatial ..."

Response: Agree. Change is made to P1 line 10 of the revised manuscript.

Reviewer: l.9: state explicitly if you applied the model to sites pecific driving data or extracted the corresponding gridcell's output

Response: Correct. We extracted the corresponding gridcell's output. Explicit explanation is added to p.1 lines 16-17 of the revised manuscript, which says "Results extracted from the corresponding gridcell (without site-specific forcing data) was comparable with the average of cross-site observed annual mean emissions"

Reviewer: l.11-15: State the response of N2O to elevated CO2.

Response: Corrected. The revised manuscript states: We found that the global response of N2O emission to CO2 fertilization was largely determined by the response of tropical emissions with reduced N2O fluxes in the first few decades and increases afterwards. The initial reduction was linked to N limitation under higher CO2 level, and was alleviated through feedbacks such as biological N fixation. The extratropical response was weaker and generally positive, highlighting the need to expand field studies in tropical ecosystems.

Reviewer: p.3103 l.1: You may state the contribution of N2O to total anthropogenic radiative forcing.

Response: Agree. Add to p.2 lines 14-15 of the revised manuscript.

Reviewer: l.4: Unclear what you mean with "comparable to the combined anthropogenic emissions"

Response: Agree. We deleted "comparable to the combined anthropogenic emissions" to reduce confusion. See p.2 line 17 of the revised manuscript.

Reviewer: l.20: 'particularly' instead of 'particular'

Response: . Change made to p.2 line 31 of the revised manuscript.

Reviewer: p.3104 l.18: In my reading, LPJ DyN simulates a positive response of global N_2O emissions to CO_2 (blue line is above purple line in Xu-Ri et al. (2012), Figure 5).

Response: CO_2 plus interaction with climate result in a positive response of global N_2O emissions in Xu-Ri et al., (2012), but historical CO_2 change alone (single factor, from Fig. 7 of Xu-Ri et al., (2012)) causes a slight decrease in historical N_2O emissions. To clarify, we rewrote this part as: Simulations with O-CN demonstrated a positive response of N_2O emissions to historical warming and a negative response to historical CO_2 increase, globally. While CO_2 and interaction with climate change resulted in an increase in historical and future N_2O emissions from LPJ-DyN (Xu-Ri et al., 2012) and its application (Stocker et al., 2013), respectively, historical CO_2 change alone (single factor, from Fig. 7 of Xu-Ri et al., (2012)) caused a slight decrease in historical N_2O emissions.

Reviewer: You may also want to refer to Stocker et al., 2013: N_2O response from another implementation of Xu-Ri's adaptation of DNDC. Response: We added the omitted reference to p.3 lines 21-23,27-31 of the revised manuscript.

Reviewer: l.21: Xu et al., 2012 is usually referred to as Xu-Ri et al., 2012 (see references 'Xu-Ri & Prentice, 2008' in her own publication Xu-Ri et al., 2012).

Response: Corrected.

Reviewer: l.29: "data-overriding" Can you explain this differently - wasn't clear to my first reading.

Response: Agree. "Data-overriding" is changed to "replacing the model soil moisture"

Reviewer: p.3105 l.11: Does LM3V-N use fixed prescribed C:N ratios in different compartments?

Response: Yes. LM3V-N uses fixed prescribed C:N ratios in different compartments. In addition, LM3V-N has a N storage pool that buffers asynchronies in C and N dynamics. For more details, please refer to 2.1.1.1 C-N coupling in vegetation or Gerber et al. (2010).

Reviewer: Please clarify. Sect. 2.2.1.: Good, accurate description.

Response: We have trouble finding this expression in this section but stand by to make any further clarification.

Reviewer: p.3108 l.16: do you really mean "maximum"?

Response: No. Maximum is deleted and text is rewording to "LM3V-N uses the concept of plants available water, where the water that is available to plant varies between the wilting point and field capacity".

Reviewer: l.25: I'm confused, units don't add up. Also, it is unclear where other parameter values in Eq. 1 are derived from. Eq. 1 is the only equation presented in the main body of the manuscript, yet it describes a quantity of secondary (if not tertiary) importance (WFPS -> rates -> N2O emissions). This appears somewhat inconsistent with the presentation of more important equations only provided in the Appendix. Strong emphasis is put on assessing different formulations of WFPS, yet an function of WFPS is actually applied for determining denitrification/nitrification/volatilisation rates and NOx:N2O partitioning in the model, and this function contains parameters which are not described and assessed.

Response: Further explanation of parameter values and units are added to Eq.1 (Eq. 22 in the revised version). The formulation is revised as: WFPS=(theta/(rho * h_r)/(1-BD/PD) while WFPS is the water filled porosity, theta (kg m-2) h_r is the root zone soil water; hr (m) is the effective rooting depth of vegetation; rho is the density of water

(1000 kg m-3); PD is the particle density of soil (2.65 g cm-3); and BD is the bulk density of soil (in unit g cm-3) obtained from the Harmonized World Soil Database (HWSD) version 1.1 (Wei et al., 2014). We add more detailed description of the main characteristic of LM3V-N (sect. 2.1.1) and soil N2O emission (sect. 2.1.2) to the main text. WFPS is involved in nitrification/denirification/volatilisation as well as the partition of N gases as reported by various field and modelling studies. The NOx:N2O partitioning is taken from the empirical relationship derived by Parton et al. (2001) which is applied in the daily version of the CENTURY model. These constants are empirically derived based on field measurements. D/D_0 denotes the relative gas diffusivity in soil (D) compared to that in the air (D0). D/D_0 is calculated based on air filled porosity. The parameter represent the gas diffusion in air (D0) is not accually used in calculation. To clarify, we replaced the notation D/D_0 by Dr in the revised manuscript (Eq. 14-15).

Reviewer: p.3109 l.17: "...field scale." References?

Response: Agree. References "Dijkstra et al., 2012;van Groenigen et al., 2011" are added

Reviewer: l.22: At what point in the simulation does the CO2 doubling become effective?

Response: Here, we evaluate the model's response to step changes in form of a doubling of preindustrial CO2 level (284 ppm to 568 ppm) and a 2K increase in atmospheric temperature.

Reviewer: p.3110 title of Sect. 2.3: Could "... with environmental variables" be replaced by ": : : with observations"? This would make more sense to me.

Response: Agree.

Reviewer: p.3111 l.3: Did you get this value spot-on from blindly implementing the equations with parameter values described here or was there any tuning involved? Not that this would be problematic, but it should be mentioned here to provide clarity.

Response: We did not aim at this value. As the reviewers mentioned, this value is sensitive to the fraction of net nitrification lost as N2O. We highlight this now in the discussion section.

Reviewer: Where does uncertainty range stem from? Why is the uncertainty range not displayed in Fig. 1? Or is it just a range of values for different years. Please clarify.

Response: Based on another reviewer's suggestion, we supplied three budget values corresponding to the three soil moisture datasets. The simulated global soil N2O flux is 6.69+/- 0.32 TgN yr-1 (1970-2005 mean and standard deviation among different years) (Fig.1) with LM3V-SM (Method 3), 5.61+/-0.32 TgN yr-1 with NOAH-SM (Method 2) and 7.47+/- 0.30 TgN yr-1 with ERA-SM (1982-2005, Method 2). The uncertainty (+/-) stands for the standard deviation of N2O emissions from different years for each soil moisture dataset, which we clarified in our manuscript. Annual N2O values from different soil moisture datasets are also added to Fig.1.

Reviewer: p.3112 l.10: highly variable savannah emissions: when high/low? during wet season? confusing units (seasonˆ-1)

Response: High emissions are during wet seasons and low emissions in dry seasons. Units are changed to monthˆ-1 instead of seasonˆ-1 for Fig.2.

Reviewer: p.3115 l.5: Xu-Ri et al., 2012 suggests positive effect.

Response: Please refer to the answer to an earlier question in p.3104 l.18. To reduce confusion, we removed reference to Xu-Ri et al., 2012 in the revised manuscript.

Reviewer: l.13: "net effect depend on..." See my general comment "SUBJECT TO PERFORMANCE OF LM3V-N".

Response: Agree

Reviewer: p.3116 l.18: delete "knowledge from"

Reviewer: Response: Corrected.

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Reviewer: p.3117 l.10: Wouldn't such environmental gradients (along which primarily temperature and precipitation change) offer a great testbed for N2O model benchmarking?

Response: Agree. Environmental gradients provide us great opportunity to test the models. Although altitudinal changes result in temperature or precipitation gradients, these gradients are within one model grid cell and the model does not incorporate topographical information explicitly. It is difficult to make use of the altitudinal data.

Reviewer: p.3120 l.9: typo: "speicies"

Response: Corrected.

Reviewer: Appendix in general: Parameter values are presented in Equations without any further description and reference. Can this be improved?

Response: Appendix A is rewritten and moved to sect. 2.1.2 Soil N2O emission in the main text. We replaced the notation D/D_0 by Dr in the revised manuscript (Eq. 14-15). We also add corresponding refereces, units and further descriptions for parameters such as b_(N,ãĂŰNHãĂŮ_4^+), b_(NO_3^-),k.

Reviewer: Fig.5: I recommend to use a two-colour scale (e.g., blue-red) Response: According to reviewers' suggestions, we agree that Fig. 5 does not provide much information and delete Fig.5

References

Davidson, E. A., Keller, M., Erickson, H. E., Verchot, L. V., and Veldkamp, E.: Testing a conceptual model of soil emissions of nitrous and nitric oxides, Bioscience, 50, 667-680, doi:10.1641/0006-3568(2000)050[0667:tacmos]2.0.co;2, 2000.

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