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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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Our previous response was mistakenly copied and paste from an older draft. There is little change here compared to our earlier post, but we think that some responses are clearer and hopefully provide more information. We apologize for the double posting.

Reviewer:

This MS presents a module for simulating N2O fluxes at the global scale based on equations for denitrification and nitrification and considering N2O and NOx as fractions of the nitrogen that is processed. Most model elements were borrowed from other models. I have a number of serious problems with this MS:

Response: Thank you for taking time reviewing our paper.

Reviewer: The model description in Appendix A is not complete as the units are not provided.

Response: Agree. Appendix A is rewritten and moved to sect. 2.1.2 Soil N2O emission. We have further added more explanation on the overall plant-soil nitrogen cycle. We believe we caught all instances where we missed the units.

Reviewer: It is not clear how model calculations at a resolution of 3.75 by 2.5 degrees can be meaningful, since all data such as weather, soil and vegetation are kind of aggregates for that resolution, and how can this be compared with point measurements.

Response: We agree with the reviewer that evaluations of global simulations against point measurements result in scale mismatches. Soil N2O fluxes are highly variable even at the scale of a single stand. Currently, a higher resolution that is feasible for global simulation (e.g. 0. 5 by 0. 5 degrees) is still much larger than the area where field measurements take place, and benchmarking global simulations against field measurements still faces scale mismatches. In this way, any model data comparison is incomplete. In the original manuscript, we focus on capturing the average of annual mean emissions across different observations instead of one-by-one comparison. This overall average emission aggregates measurements and provides some information on model performance. We make reference to scale mismatches in the text. We further amend our manuscript with comparison against single points (although these points still represent grid-cell averages). We believe this is still informative, for example, our analysis shows that, while we somewhat capture the means of the fluxes across sites. However, our model has trouble simulating peak emissions. Further studies may elucidate whether a better representation of these hot moments are more sensitive to processes emitted (freeze-thaw cycles, pulses), or the aggregation of vegetation and climate.

Reviewer: With this spatial resolution, the time step is 30 minutes, but the authors provide annual and seasonal numbers only. It is probably more interesting to compare the model results with temporal distributions from field measurements. This could be done for a number of test sites in a variety of climate and soil conditions. If not available, perhaps seasonal estimates from experimental sites could be used to validate the model. Response: We have now added monthly and daily measurements to sect. 2.3 and 3.3, which show, that the model agrees with the general trends in N2O emission, but measured emission peak are not realized in the model.

Reviewer: To assess model quality, it is much more interesting to analyze the functioning of the soil-plant nitrogen cycle. How is denitrification compared to field measurements, and leaching, plant uptake, ammonia volatilization, etc. If the large flows in the system are correct, the authors will also be more confident about the small fluxes like N2O and NOx.

Response: Assessing model quality is a challenge for all models, and several reviewers have pointed out the effect of the larger N cycle on nitrification, denitrification and associated N2O fluxes. We now present an extensive sensitivity analysis that evaluates N2O fluxes in response to variation the relative strength of plant uptake, the overall supply of N to mineralization (by way of modifying biological N fixation), and by specific parameters that deal with nitrification and denitrification. We discuss the hierarchical system of plants and soils N demand are strong sinks for ammonium and nitrate under limiting condition, resulting in leaching and denitrification very small amounts. Excess nitrogen (if plants and soils have sufficient N) leads to leaching and denitrification. There, the partition coefficients and the parameters that determine the fate of the excess N become very important. The sensitivity analysis targets exactly that question. We further point out, that we do not invent a new model but put existing formulation (and parameter choices) in a new model. Currently, we do not have large scale observation data such as denitrification and ammonia volatilization in relative pristine ecosystems to support the benchmarking of the global model. Detailed analyses addressing this are now in sect. 2.2.3, 3.4 and the discussion part of the revised manuscript.

Reviewer: Finally, a true sensitivity analysis will also show what the major variables

and parameters are. For example, the N2O and NOx fractions will probably pop up as important coefficients.

Response: We add a series of sensitivity tests with regard to plant N uptake, nitrification rates, denitrification rates and the fraction of N2O lost from net nitrification in sect. 2.2.3 and sect.3.4 of the revised manuscript, which shows effects of the larger N cycle on the availability of N for nitrification and denitrification as well as direct parameter uncertainty of the added module. We found that the fraction of N2O lost from net nitrification is the most sensitive parameter. 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 embraces large uncertainties in our modelled results.

Reviewer: I fully agree with one of the other reviewers who states that this work is not ready for publication, and I also agree that perhaps Geoscientific Model Development is a more appropriate journal for submitting a revised MS.

Response: Thanks again for the helpful suggestions. We carefully considered the possibility for Geoscientific Model Development. As pointed out in the response to Beni Stocker's review, we highlight that this is not a new development, but the addition of an existing nitrification/denitrification module to LM3VN. We would like to emphasize that the basic goal of this paper is thus not presenting the new module per se, but how it performs in the context of the larger plant-soil N cycle. The reviews have helped to sharpen this focus, which we think makes the manuscript a nice fit for Biogeosciences.

References

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