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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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 CO_2, warming). The model is assessed against observational data of N2O emissions from a set of observations that are collected for the present study. Apart from confirming global total N2O emisions 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".

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. But does it convincingly succeed at thoroughly describing the parametrisations 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.

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. - 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.

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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.

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. 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.

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.

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 ($\sim$water-filled pore space, WFPS) is thus questionable.

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 (moisture, temperature, . . .) - fraction of denitrification/nitrification lost as N2O

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?)

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 emis-

sions: 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.

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 fertilisation 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 ($\sim$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?

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.

"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.

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 mineralisation and leads to depleted inorganic N pools. Maybe add this to discussion.

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." âĂŤ re wording to "Due to its high temporal and spatial ..." l.9: state explicitly if you applied the model to site-specific driving data or extracted the corresponding gridcell's output l.11-15: State the response of N2O to elevated CO2. p.3103 l.1: You may state the contribution of N2O to total anthropogenic radiative forcing. l.4: Unclear what you mean with "comparable to the combined anthropogenic emissions" l.20: 'particularly' instead of 'particular' p.3104 l.18: In my reading, LPJ DyN simulates a positive response of global N2O emissions to CO2 (blue line is above purple line in Xu-Ri et al., 2012, Figure 5). You may also want to refer to Stocker et al., 2013: N2O response from another implementation of Xu-Ri's adaptation of DNDC. 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). l.29: "data-overriding" Can you explain this differently - wasn't clear to my first reading. p.3105 l.11: Does LM3V-N use fixed prescribed C:N ratios in different compartments? Please clarify. Sect. 2.2.1.: Good, accurate description. p.3108 l.16: do you really mean "maximum"? 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

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and NOx:N2O partitioning in the model, and this function contains parameters which are not described and assessed. p.3109 l.17: "... field scale." References? l.22: At what point in the simulation does the CO2 doubling become effective? p.3110 title of Sect. 2.3: Could "... with environmental variables" be replaced by "... with observations"? This would make more sense to me. 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. 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. p.3112 l.10: highly variable savannah emissions: when high/low? during wet season? confusing units (seasonˆ-1) p.3115 l.5: Xu-Ri et al., 2012 suggests positive effect. l.13: "net effect depend on ..." See my general comment "SUBJECT TO PERFORMANCE OF LM3V-N". p.3116 l.18: delete "knowledge from" p.3117 l.10: Wouldn't such environmental gradients (along which primarily temperature and precipitation change) offer a great testbed for N2O model benchmarking? p.3120 l.9: typo: "speicies" Appendix in general: Parameter values are presented in Equations without any further description and reference. Can this be improved? Fig.5: I recommend to use a two-colour scale (e.g., blue-red)

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