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Biogeosciences Discussions

Interactive comment on "Impacts of Nitrogen Addition on Nitrous Oxide Emission: Model-Data Comparison" *by* Yujin Zhang et al.

Anonymous Referee #3

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The aim of this paper is to investigate the performance of biogeochemical models (of different complexity) on nitrate and ammonium additions in terms of N2O emissions, mimicking the seasonal N depositions over a forest. I found this paper poor and lacking on multiple aspects, from the method to the discussion of the results.

Major points:

1. The calibration procedure of these models is not well reported in the text whilst it represent a pillar in this research field (section 2.4). I assume that no one of these models was previously calibrated, since most of them have not the possibility to simulate very complex systems as forest, neither plants. If this procedure has been performed, I would suggest the authors to detail it for another submission.

2. The models selected by the Authors present large differences in complexity and are constitutionally different. This could produce difficulties in using and comparing these models since some of them are just a module and are not able to consider important processes as the plant uptake, water dynamics in the soil, interaction with the biogeochemical cycle of C, or other losses of N than N2O as ammonia, as reported in the conclusions, but never discussed in the paper.

3. Some of the selected models is already set to add automatically the atmospheric deposition as a source of N to the system (wet and dry). A specific treatment by the Authors regarding the parameterization of these models to reduce this default addition were not addressed or discussed.

4. The title is not appropriate, since it does not circumscribe the domain of the investigation and does not uses specific words.

5. The abstract is very confusing and not well written in English, seems very different from other part of the text that appear much better written.

6. Introduction is lacking and not well shaped, related to global estimations, when the study in on another scale. Figure 1 that report only some example and is not exhaustive.

7. Materials and method par has to be improved, since there is not a clear explanation of the experiment, e.g. the role of the control plot, the repetitions (number and disposition), the measurement performed by the flux tower and the characteristics of the chambers, or soil depth of the measurements. In the simulation protocol there is no a detailed procedure on how the statistical procedure was applied.

8. Discussion section should be focused with the interpretation of the data, objectively and inter-subjectively, in light of the evidences brought by other scientists or backgrounds. In this section, there are part that are more suitable for the introduction section (i.e. L174-185). Some discussion is not directly in support of the obtained results.

Furthermore, the discussions are poor and are not addressing the aims of the paper.

9. Supplementary results. I appreciated the efforts of the Authors to collect the equations behind the N2O emissions in the explored models, but in some of them there are relations that have nothing to do with it e.g., Rg and Rd in DNDC. These equations and the parameters or variables used by each model could be the base to discuss the performances of each model.

10. Furthermore, I really suggest rephrasing everything avoiding plagiarism (that is at 25%).

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