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Interactive comment on "Quantifying Global N_2O Emissions from Natural Ecosystem Soils Using Trait-Based Biogeochemistry Models" *by* Tong Yu and Qianlai Zhuang

Anonymous Referee #1

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General Comments: This study tried to improve their previous first-order module within the TEM model through incorporate microbe trait to simulate nitrous oxide (N2O) fluxes from the natural soils. The results indicated that a total emission of N2O was 8.7±1.6 Tg N yr-1 globally, and 42% of this emission was attributed to tropical forest. They found that the average N2O flux is 0.7 kg N ha-1 yr-1, with a minimum flux of 0.01 kg N ha-1 yr-1 in the dry season of African savanna, and a maximum of 5.7 kg N ha-1 yr-1 in tropical peatlands based on all observational sites (N=81). Compared with their previous version, the current microbial trait-based model shows a better performance, especially in rainforest, because of the consideration of N taken up by soil microbes. They concluded that new model captured more variations of N2O emission in response

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Discussion paper

to seasonal changes in climate. However, there are substantial weaknesses in this study that should be addressed for making an incremental advancement in modeling N2O emissions from soils.

Specific Comments: 1. The authors intent to estimate N2O emission from natural soils during 1990-2000. However, the terrestrial ecosystems have been extensively disturbed and managed. It's unclear about what the natural soils mean in this manuscript. There is no detail information on how they generate the natural ecosystem data across the global land surface. 2. Global map (Fig. 5) shows N2O emissions from the cultivated areas where crops planted during 1990-2000 according to my knowledge. Do you consider background emission from cropland as natural emissions? Or you treat cropland as other types of vegetation? 3. It needs to provide more explicit explanation on the role of microbe in N-containing gas formations and diffusions, mineralization/immobilization, nitrification/denitrification, etc. The figure 1 needs to include such information on microbial processes. 4. The description of major equations is barely understandable for readers. There is no connection between these equations listed in the manuscript. The authors should provide equations focusing on N2O fluxes. 5. The authors mentioned their previous model and used it to make comparison with the current version. They should have a description of their previous model and list the improvements in the methodology. 6. They emphasized site-level estimates and climate data sources, but not for global simulations. There is no detailed information on the climate data source or description on climate variability during 1990-2000. 7. The authors should provide the method on how to extrapolate site-level estimates to the global level. Also, I am curious with the uncertainty range (7.1-10.3 Tg N yr-1), but they did not give any explanations. 8. As also indicated in the manuscript, biological N fixation and denitrification can contribute a significant amount of N2O emissions, but these processes were not included in this study. A paragraph should be included in the discussion sector to address this ignorance and its impact on the entire estimates. 9. They claimed that CN ratio plays a significant role in N2O emissions, which is one of their objectives. They indeed mentioned CN ratio threshold in the methodology; however, nothing special has been described in the result or discussion sectors. 10. The improved trait-based model is actually a hybrid of first-order and second-order expression. According to Fig. 4, I cannot tell the advantages of this improved model. They should provide more evidence. 11. They found that tropical peatland has the highest N2O emission, up to 5.7 kg N ha-1 yr-1. When I go back to that article, they chose this site because the peatland was converted to cropland and induced a much higher N2O emissions. However, I guess this model is incapable to simulate land conversion and its impact. If you used this site, your estimates in Southeast Asia should be much higher than other previous studies. Thus, it is not appropriate using this site for model calibration. 12. In Fig. 2, I can only detect one site in the Congo Basin for model calibration. Based on my knowledge, this region may be a large source for N2O emission. Thus, I suggest the authors to collect more data to re-calibrate their model. 13. The microbial biomass data was not well explained. We need to see more details about these data. 14. It seems that the tables and figures can be further improved. For example, the table 1 and table 2 can be provided as a supplement file. The Fig.2 can be improved by removing the Antarctica regions. Fig. 3, 4 and 6 should be improved as the current resolution of figures is poor. In addition, there are two fig.4. 15. Table 3a conflicted with details provided in section 2.3 of the paper, and tables 3 and 4 should be swapped to match the order given in the methods section. There was also repetition within the methods sections. 16. Literature cited: Several new efforts in soil N2O modeling have been published recently. Literature review should include recent modeling efforts. Particularly, I am surprised that the authors did not recognize a major NO2 model intercomparison project- NMIP (Tian et al 2018). Tian, H., J. Yang, C. Lu, R. Xu, J. G Canadell, R. B. Jackson, et al. (2018) The global N2O Model Intercomparison Project, Bulletin of the American Meteorological Society (BAMS), https://doi.org/10.1175/BAMS-D-17-0212.1

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