

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

Anonymous Referee #1

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General consideration: This manuscript presents a model intercomparison for assessing models response to N₂O emissions using different level of N deposition in a subtropical forest in China. Despite the huge potentiality of the work, mainly thanks to the availability of instruments which allows to retrieve information on several control factors of process-based models, the global work is not well structured. The paper does not fluent and the explanations of all the modelling aspects (i.e. initialization, sensitivity, calibration and validation) are missing. Also, English is poor. I recommend rejection for this paper.

Critical points

There are many critical points which were here briefly reported. In general, the paper lack of all basic aspects which should be considered in a publication focused on

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models.

1. The paper does not provide any formation about how models have been initialized: What about model spin-up (pools' equilibrium)? Which are the main input parameters (climate, soil, vegetation and management)? What about climate (how the ini file is structured? what does it requires?)? From where these climate info were retrieved? ? What about soil? From where these soil info were retrieved? Do exists a meteorological station? Far or close to the experimental area?.

2. The paper does not provide any formation about how the model has been calibrated. Authors cannot only apply models. This option could be possible only if they are able to provide a strong background for each model. This can be done, however, only for the most commonly applied tools (i.e. DNDC and DayCent, but very difficult for the remaining). Authors have to be able to provide proofs that models are able to reproduce a specific ecosystem. To do that, they should calibrate the model, reporting results related to fluxes but also to biomass or other parameters (i.e. SOC dynamics, fruit pools, etc). Doing so, authors would proof that their models are able to reproduce all these parameters. Otherwise the different effect that a specific vegetation type (as example oaks or pine for forest, maize or rice for crop, warm or cold grass etc.) may play on N emissions is ignored. If this effect is ignored, this is means that models are not reproducing what does really happening in the field.

3. I don't think all these models are able to reproduce forestry systems. For instance, authors talk about DNDC. But does exist a specific version for simulating forest dynamics (namely forest DNDC). The common version (the latest one is DNDC95) is not appropriate for simulating forest systems. If authors have used this latter, they should explain much in detail what they do for simulating a forest system (i.e. how has been calculated biomass partitioning? From where they retrieved biomass partitioning info?).

4. The paper does not provides any formation about ecophysiological parameters used for reproducing a specific type of tree. Trees have different response to climate (i.e.

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xenophile species, different RUE and WUE, different root depth and length). All these characteristics affect N amount and its permanence in the soil. In this paper is not possible to understand if these info were used (how they were partitioned within the model?) and how were retrieved (i.e. literature? Experiments?).

5. There are no information related to climate scenarios. Authors write about about future scenarios of N deposition but they do not refer to any climate scenarios (SRES? RCP? Which scenarios? Which time slice? References?).

6. Only one year of data is not enough for representing fluxes dynamics, especially considering that fluxes variability is closely related to climate-soil interaction. More than one year of data is needed for proving that models well work. This time should be used for calibrating the different models.

7. Figures are not clear and discussion is very poor.

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