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Interactive comment on “Multiple-factor controls on terrestrial N₂O flux over North America from 1979 through 2010” by X. F. Xu et al.

Anonymous Referee #2

Received and published: 11 January 2012

Title: “Multiple factor controls on terrestrial N₂O flux over North America from 1979 through 2010” Authors: Xu et al.

I have tried to do a proper review of this paper. Although the topic of the impact of temperature, atmospheric CO₂, deposition of nitrogen, radiation, ozone and N inputs is interesting and important, information on the model and its validation is lacking; interpretation of the results is therefore very difficult, especially it is difficult to understand what kind of experiments the authors have actually performed.

The model that describes N₂O processes apparently consists of one equation with 6 factors, four of which are lumped into one, and the factors ozone and N input are additional factors. Hence, the equation is a multiplication of three factors. Information on how the various factors are incorporated in the model is not provided. For example,

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denitrification and nitrification are modeled, but I see no relationship between these processes and N₂O production, or is it the V_{max}, the maximum rate of N₂O production via nitrification and denitrification? Are there two V_{max} values, one for each process. And is the value for V_{max} a global one, or is it variable in space and time? Atmospheric CO₂ concentration, ozone and absorbed photosynthetically active radiation influence N₂O emissions directly. In other models such as DNDC and Daycent the effects of such factors would be dealt with in the ecosystem/carbon cycle model in which the N₂O equations are incorporated, so it is peculiar that in the approach of this paper there is a direct influence. This requires explanation. The factor air temperature will have a direct effect on denitrification, but also indirectly through its influence on evapotranspiration and growth. There are many more questions that come up, but I guess the authors need to provide a better description of their model, including how the N₂O calculations are built in the ecosystem model.

Further lacunae in the paper are the various terms in the N cycle. The model apparently does not account for other N inputs like biological N fixation, and recycling of animal manure. There will be reasons for this, but it needs to be discussed at least. It is also unclear how N inputs influence growth of plants in agricultural and natural ecosystems.

Some discussion on how the model performs should be included. A remark that the spatial patterns are similar to other models is not sufficient. So a comparison with measurements and with other models is needed, if possible at the scale of individual grids, or counties, states and the total for North America. That should give some more confidence in the results, before starting to think about model experiments.

The section describing the model experiments is completely unclear. I read it three times and still have no idea what exactly has been done.

So I did not try to understand the results and discussion sections.

My recommendation is to reject, and invite the authors to submit a revision with a better description of the model and a full validation.

Interactive comment on Biogeosciences Discuss., 8, 10935, 2011.

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