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Biogeosciences Discussions

Interactive comment on "Spatial and temporal patterns of CH_4 and N_2O fluxes in terrestrial ecosystems of North America during 1979–2008: application of a global biogeochemistry model" *by* H. Tian et al.

H. Tian et al.

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Comments The manuscript by Tian et al. represents a well-written paper regarding a continental analysis of how CH4 and N2O fluxes vary in space and time for North America. I think this is a worthy attempt at a large goal, and I enjoyed multiple aspects of the manuscript. However, I feel that there are a variety of changes that should be made to enhance the clarity and utility of the paper.

[Response: Thanks for the positive comments.]

Comments In particular, I feel that a more in depth discussion of (1) the information that went into the model and (2) possible controlling factors behind the observed patterns is warranted. Please find my general and specific comments below.

[Response: We have made little revision on the manuscript putting put more discussion on the model. The controlling factors on the CH4 and N2O fluxes were analyzed and reported in our other two manuscripts; so we did not put too much effort on these in present manuscript.]

General comments Comments Page 2836, lines 24-25: You talk about comparisons being made between modeled results and a variety of field data (e.g. from CERN and LTER) but you do not discuss these further. Are there comparisons that were made at this phase besides the two sites you discuss explicitly?

[Response: It has been revised a little; the comparisons have been done in our previous work, so we just put the citation, and did not go further for discussion.]

Comments Page 2837, lines 15-16: you say that the DOC is either used as a substrate for methane or that it is oxidized to CO2, but DOC could also be leached 'out the bottom', stabilized within soil organic matter, or used to form heterotrophic microbial biomass. Does the model not account for these other pathways or is 100% of the DOC in the modeled system converted either to CO2 or CH4? Or, do these other pathways not matter, because it is simply based upon a relationship between absolute DOC concentration and CH4 efflux? Also, how are DOC and N concentrations determined / estimated? Are you using biome/ecosystem definitions to do this? If so, what are the definitions and where are the data coming from? Are you using information from a single site and extrapolating to whole biomes?

[Response: In the DLEM model, leaching is considered as a sink of nutrient in system. So, the leaching is accounted for DOC loss. We have revised the manuscript to show this. The DOC and N concentration are determined through the all major C and N processes in the DLEM. All the parameters for these processes are different for each

ecosystem. The optimization of these parameters for each biome might be conducted for one or more site, depending on the availability of the field observations. All these could be found in our previous publications for the DLEM model.]

Comments In your formulas describing the controls over CH4 and N2O efflux to the atmosphere there is no term for O2 availability. Is it because O2 is unnecessary due to O2 linkages with soil moisture, or simply that soil moisture vs CH4 or N2O relationships are so strong? Different ecosystems with the same soil moisture can have different soil O2 concentrations (soil texture, root and soil heterotrophy respiration, etc can help regulate the relationship between soil moisture and soil O2), but perhaps at this scale soil O2 is not required?

[Response: We agree that O2 availability is critically important in controlling CH4 and N2O processes. However, given the difficulty in well simulating O2 availability in all ecosystems, and the strong relationship between soil moisture and O2 availability, we use soil moisture to show the effects of O2 availability on CH4 and N2O processes.]

Comments Page 2843, line 5: you say that you use a Q10 of 2.5, while Huang et al. used a Q10 of 3. Though this seems like a small difference, changing Q10 this amount can have very large effects on the results. Please include the rationale for making this change.

[Response: This change is made based on one of our field work in natural wetland (Song et al., 2009). Given that Huang et al's work is for rice paddy which might be influenced by human activity, and the natural ecosystems are the major domain in our study; we used the Q10 as 2.5, rather than 3.0. Reference: Song, C., X. Xu, H. Tian, and Y. Wang (2009), Ecosystem-atmosphere exchange of CH4 and N2O and ecosystem respiration in wetlands in the Sanjiang Plain, Northeastern China, Global Change Biology, 15, 692-705.]

Comments I find it interesting that Mexico accounts for so little of the CH4 flux but maintains a significant proportion of the N2O flux. I think this decoupling is worth

discussing.

[Response: We have added some discussion for this phenomenon.]

Comments In the same vein, I think you should more fully discuss Canada's very large increase in CH4 efflux. What is causing this?

[Response: Thanks for the comments; we have revised the discussion section by highlighting to the Canada's large increase in CH4 emission.]

Comments In general, I think a much deeper discussion of the controlling factors over the temporal and spatial patterns observed is needed. This receives little attention in the Discussion, with only a single paragraph that superficially mentions things like ozone and nitrogen input, but a deeper consideration of how these factors are regulating the patterns would be very useful. What is the role of climate (discuss Figure 9), of substrate concentration, etc and how do these important controls vary across space and time?

[Response: We have expanded our discussion on the controlling factors over the temporal and spatial patterns observed.]

Comments Also, Figure 6 suggests notable increases in efflux through time and I would be interested to hear more Discussion about the controlling factors regulating this pattern. In the first panel of Figure 4, you do see spikes in CH4 production, yet in the text you discuss a lack of spikes and fine scale (e.g. hourly) temporal resolution. With that in mind, why do you think you see spikes here? This should be discussed.

[Response: We have expanded discussion for Figure 6 and Figure 7. The spikes in the Figure 4 are observational data; we in text said that out model results are lack of spikes in CH4 flux.]

Specific comments Comments Page 2833, line 2: I would consider changing 'superhigh' to something like 'very high'.

[Response: It has been revised.]

Comments Page 2833, line 8: put a ‘the’ before ‘scientific community’

[Response: Thanks, mistake corrected.]

Comments Page 2835, line 16: when you say ‘mineralization/immobilization’ do you mean mineralization and immobilization of nutrients? Please be more specific.

[Response: Thanks, it is nutrient mineralization.]

Comments Page 2846, line 1: do you mean Landsat?

[Response: Thanks, misspell corrected.]

Interactive comment on Biogeosciences Discuss., 7, 2831, 2010.