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Biogeosciences Discussions

Interactive comment on "Sensitivity of wetland methane emissions to model assumptions: application and model testing against site observations" *by* L. Meng et al.

Anonymous Referee #1

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General Comments

This study couples a methane BGC model (CLM4Me) into global land model (CLM4CN) and explores how the modeled fluxes vary with several of the model parameters. CLM4Me (including updates included in this ms) is comprehensive and has several features beyond previous methane BGC models. The model produces a very high global methane flux 150-346 Tg CH4/yr, with again a very high proportion (79%) from tropical wetlands even though tropical inundated areas used here are lower than previous estimates. The modeled emission from northern latitudes is very low - only 12 Tg CH4/yr.

The sensitivity calculations demonstrate the sensitivity of methane flux to aerenchyma transport (in a model where no vegetation has aerenchyma), to soil pH, and to redox inhibition. The sensitivity calculations are informative at the site level, and not at the model gridcell level. This immediately raises the question how the site-level model can be scaled to the gridcell and to the globe. The authors could have and should have confronted the scaling issue and carried out additional sensitivity calculations to assess the global and regional emission estimates - the ms gives me no reason to have confidence in the estimates reported here.

Specific Comments:

The land model CLM4CN does not include plant function types (pft's) associated with wetland vegetation (Section 2.4). Instead of adding pft's for wetland vegetation (which the authors acknowledged to be a better solution), the authors assumed that methane production from the inundated portion of a gridcell scales with the heterotrophic respiration (R_H) in the entire grid cell (Equation 1). Such an assumption ignores the fact that wetland vegetation is structurally and functionally different from non-wetland vegetation, and that methane production is associated with anaerobic decomposition, while R_H is associated with aerobic processes. Granted, Walter et al. (JGR 2000), Spahni et al (BGD 2011) and others similarly assumed that the modeled NPP or R_H of a gridbox is indicative of substrate availability for methane production in the inundated portion of the gridbox. Walter et al.'s comparison with the observations shows however that the production is too large: they discussed this discrepancy at length and emphasized that the model must account for subgrid scale heterogeneity of R_H and perhaps of precipitation as well. In Spahni et al., the CH4:CO2 ratio (f_CH4 here) is not a global constant (as assumed here), but is used as a tuning parameter for each wetland ecosystem. For quantifying global methane emissions, the most important sensitivity calculations to perform are those related to the scaling of R_H and f_CH4. These are not included in this study.

The second most important set of sensitivity calculations to perform is the dependence of methane production on the modeled hydrology in CLM4CN. These calculations are also not included in this study. The comparison with the site measurements avoids the water table issue entirely by choosing the flux from the saturated or the unsaturated portion of the gridcell according to observed state of the water table. The authors should at minimum comment on the realism of CLM4CN simulation of hydrology, and how the simulation impacts the emission estimates. Does CLM4CN have a wet or dry bias?

In CLM4CN, NPP is overestimated in the tropics and underestimated at high latitudes (Section 2.5; Fig 6). Maybe this is why close to 80% of the global methane flux comes from the tropics and so little comes from the high latitudes. However, the modeled annual mean NPP has been adjusted (equation 11) at every gridcell to match that derived from MODIS. Yet Section 6 attributes the low high-latitude flux to the low high-latitude productivity in CLM4CN. It is not clear what's going on. The authors should explore what parameters give high tropical methane emissions in the model. Also, as far as I can tell, there are no peatlands or permafrost in the model. Is this a reason for the low emissions from high-latitudes?

Unlike Walter et al., the seasonality of methane fluxes are not well simulated at the few measurement sites (Fig 7, 8), despite claims in the text. While the modeled spring peak is discussed in Appendix B, there is no discussion of the poor simulation of the phasing of the methane flux. The observed methane emission in Alberta is later in the season than that modeled, while the Rhizospheric oxidation fraction appears to have the opposite seasonal trend from that observed. The model fails to capture the seasonal dynamics in Florida as well. This could be related to the poor simulation of the seasonal cycle of NPP, which may in turn depend on the modeled seasonal cycle of hydrology. The authors should figure out why the seasonal cycles are so poorly simulated here when they are well simulated in other studies (e.g Walter and Heimann, 2000), and how the uncertainties in seasonal cycles propagate to annual emission estimates.

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