Biogeosciences Discuss., 10, C208–C210, 2013
www.biogeosciences-discuss.net/10/C208/2013/

Biogeosciences Discussions

Interactive comment on "Detailed regional predictions of N_2O and NO emissions from a tropical highland rainforest" *by* N. Gharahi Ghehi et al.

J. van Haren (Referee)

jvanhare@email.arizona.edu

Received and published: 4 March 2013

In this manuscript Ghehi et al. combine a process model (ForestDNDC-tropica) with a spatially intensive soil property database (part from the literature, part self generated) to estimate the magnitude and spatial variability of soil NO and N2O fluxes in a forested region of Rwanda. They then test whether several soil property data collections over time have much influence on the predicted fluxes.

This paper is interesting and has value based on the regional soil property database and its value to improve model predictions by it's more detailed scale than global soil datasets. However, several aspects of the paper greatly distract and diminish the value of the paper: 1) The model output is not well validated against field data: f.i., the biomass estimates are only roughly compared to one site within the study region, 2) the model gas flux output is only validated against soil gas flux incubations, not actual fluxes in the field, 3) Although the authors have detailed soil data, but no detailed climate data is available or incorporated, which I presume can be taken from the global TRMM database. 4) model parameter sensitivity analysis was already done by Kiese et al. 2005 and Werner et al. 2007, I do not see the need to keep repeating this for each model study, unless the model has substantially changed 5) the supplement supporting the hypothesis that high soil NO and N2O fluxes could be due to chemodenitrification is very poorly written and the analysis associated with the incubations poorly conceived. 6) The reported gas concentrations in the supplement appear erroneous (5-30 ppm reported for CO2, whereas atmospheric values are ~390ppm; 100-200 ppb reported for N2O atm ~320ppb), unless the incubation air was treated without mentioning in the text 7) The evidence presented in the supplement is suggestive for NO, but not convincing of the production pathway, that the experiment set out to accomplish, by the use of 15N NO2-, but 15N NO was not measured (this could have been accomplish by better planning)

This paper would be greatly enhanced if the authors could include chemodenitrification in their model to test whether that indeed can resolve the poor flux predictions by the current model version. However, the poor quality of the presented data in the supplement leaves me skeptical whether this is useful and the quality of the dataset the model is being tested against.

Sincerely,

Joost van Haren

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C208/2013/bgd-10-C208-2013-supplement.pdf

BGD

10, C208–C210, 2013

Interactive comment on Biogeosciences Discuss., 10, 1483, 2013.