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Biogeosciences
Discussions

Interactive comment on "Soil N_2O and NO emissions from land use and land-use change in the tropics and subtropics: a meta-analysis" *by* J. van Lent et al.

Anonymous Referee #1

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The manuscript presents a review on soil N2O and NO emissions from forest, pasture and arable systems of the tropics and subtropics including impacts of land use change. Generally it seems - even though the database of Stehfest and Bouwman (2006) on N2O and NO emissions was substantially extended - that there is still limitations with areal coverage (Oceania and Africa are underrepresented) and that many published studies do not provide a complete set of environmental or co-variables (Nfix, litterfall etc.) which might be essential to further improve the prediction capacity of the regression models. Due to experience with this study it would be most helpful including a paragraph into the discussion what are currently the most important constraints and how could they be solved in the future, e.g. where do we need more data, what kind of

study layout is needed, and what minimum data need to be measured/ reported. A further constrain is that the study does not provide info and discussion on elevated N2O (NO??) emissions several years after land use change e.g. from forest to pastures and how this short period may impact alteration of N trace gas emissions budgets. It would be most helpful if this issue would be included in more detail into the discussion. Integrating rice paddies into arable systems can likely bias the meta-analysis since soil environmental conditions of flooded fields are not comparable to upland crops. The same my hold for wet forests. Therefore I suggest redoing the meta-analysis.

Detailed comments: Ln 81-82: Wording, no complete sentence. Ln 88-89: Not clear what you mean with this sentence, since your study mainly deals with upland forests. Ln 156ff: the numbers of 277 studies and 379 papers do not match. I found this section about number of studies a bit confusing. Ln 189ff: I suggest writing this section in a more general way. Thus, studies with very high fertilization rates were not considered and do not provide the few citations. Ln 201: Is the amount of N fixation e.g. from Acacia considered in the meta-analysis? The column of Table 1 on N-fixation is just providing counts of yes and no. If you can′t provide N fixation rates I would suggest skipping the column. Ln 204: Age after conversion is very crucial for magnitude of fluxes, since they can be very high several years after the conversion. If you want to evaluate impacts of land-use change this period of time cannot be neglected. Since you have the most comprehensive database of N2O and NO emissions I highly recommend discussing this issue, even given the low number of studies. Ln 215: There might be some difficulties with rice being part of the croplands since rice is mostly cultivated under flooded conditions which is completely different to soil environmental conditions of upland crop cultivation which can have also an impact e.g. on elevated NH4 concentrations due to limited nitrification. This could bias the regressions analysis. The same holds also for wet forest, even though they have surprisingly the lowest wfps of all investigated ecosystems. Ln 261-262: delete the sentence with the Gelfand citation. Ln 288-292. This is redundant to Material and Methods. Ln 295-296: There is some studies of Oceania and Africa which I could not find in the references. E.g. Rowlings et al., 2011, Global Change Biology; Kiese et al. 2003 Global Biogeochemical Cycles; Wang eet al., 2011 Global Change Biology; Castaldi et al., 2013 Biogeosciences. Ln 319: I don't think this comparison makes sense since sample size of soil temp for all ecosystems are very low. There might be also some bias coming from heterogeneity of geographical distributions, e.g. rice paddies are mainly located at Asia. I suggest if providing differences of ecosystem characteristics than rather for mean annual temperature. Ln 354: d = 0.11. How did you calculate this value? Ln 368ff: I cannot find these numbers in Table 3. Ln 372: what about N2O, again is the number provided for NO linked to Table 3? Ln 413: 1.4 unit is missing Ln 419: What is the reason for such high emissions? Since you discarded studies with high fertilization rates I am wondering why you keep this "outliers". In this section you report impacts on mean NO/ N2O emissions, but how would this effect your Hedges'd values. Is there really a reason to keep this studies? Ln 457: You may need to mention, that the different approaches used also different amounts of data. Ln 462: This needs to be elaborated in more detail. Coming back to my general comment about constraints of incomplete data, here some suggestions of how to solve this problem in the future would be helpful. What do you think we can trust more, averages or Hedges'd calculation? LN 491: WFPS of >100% is not possible. This is just an artefact of the fit. Figure 1 is not needed. Figure 2 is not really transferring the information you aim in an easy way. It might be better presenting all info in a Table. Here you could include the percentage of studies of any ecosystem in any region and globally. Figure 3: WFPS > 100% does not make sense. What data were used for the Figure, any were WFPS was available? You mention that WFPS was clustered in classes differing by 10%, which is sometimes more or less. Figure 5 not needed Figure 6 would be better as 3d plot including d(WFPS) in detail Table 1 I was wondering about very high litterfall N of 831 in LFC and 479 in Pl. You may need to mention that due to different data sources, there might be some inconsistencies between e.g. annual air temperature and soil temperature (LFC soil temp about 5°C higher). There is columns without statistics.

Interactive comment on Biogeosciences Discuss., 12, 12783, 2015.