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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.

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The below response are a copy of the original author response following the comments by reviewer #1.

Thank you for the review and useful recommendations. The reviewer recommends to add a paragraph on what the most important constraints are and how they can be solved. In the discussion we already mention areas and LUs that are underrepresented in the first paragraph of the section ‘dataset representativeness and average annual LU emissions’ from line LINES 391 to 413. The recommendations on what kind of study layout is needed and what data needs to be measured/reported is now added more specifically on LINES 480-485 and 546-547. In the conclusion, these points were already summarized (LINE 560-574). The comments on time since conversion and rice are answered below (see comment on LINE 204, and comment on LINE 215).

Detailed comments: Ln 81-82: The sentence is split up in two, improving readability.

Ln 88-89: Indeed, the reduction of uncertainties was not for all land uses. The sentence is adjusted to better reflect this (LINE90).

Ln 156ff: The total number of papers was suppressed to improve the readability.

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. The two sentences were merged and simplified.

Ln 201: N fixation is considered in the meta-analysis (see Table 3). In the majority of cases the amount of N fixation was not provided by the studies, but planted crops or tree species indirectly indicated presence or absence of N fixation. The column related to presence or absence of N fixing trees/crops was removed from Table 1.

Ln 204: We have included an analysis of time since conversion. Given the small sample size only the emissions of N2O in cropland and pasture cases were included. For croplands, we differentiated between fertilized and non-fertilized cases. Croplands showed a distinct pattern of high fluxes the first 5 to 10 years and then reduced to the same level as average F and LFC values. When fertilized, however, fluxes remain well above F and LFC values after 10 years. We have added this to the manuscript in LINES 355-359 & 417-475. For pastures the trend was less obvious. Some cases have high fluxes, others show very low fluxes compared to average F and LFC fluxes the first years after conversion, this is already discussed in LINES 458-467.

Ln 215: Throughout the manuscript rice indeed is already separated from cropland, for same reasons as given by the reviewer. The same holds for wetland forests. We added a coma on LINE 209 to clarify that rice fields formed a category distinct from the

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12, C7495–C7499, 2015

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cropland category; and also numbered the nine categories. The regression analysis without wetland forest and rice paddies changed the coefficients and r2 considerably even though these were only a few cases, likely due to the bias in NH4 concentrations like the reviewer mentions. Therefore WF and R were excluded from the regression. The low WFPS in wetland forests are due to their low bulk densities.

Ln 261-262: This sentence was deleted.

Ln 288-292: The lines 299-303 were suppressed from the result section and the total number of N2O and NO case studies was added in the method section(LINES 166 and 168-170).

Ln 295-296: Thank you very much for these studies. Kiese et al. (2003) was added to the dataset. Rowlings et al. (2011) is in our dataset as Rowlings et al. (2012). This is the same, as it is published in printed version in 2012, but appeared online in 2011. Castaldi et al., 2013 is already in the dataset as well. Wang et al., (2011) is not completely included as this study is a long-term field experiment dealing with different management practices. Values throughout the manuscript were updated, but did not lead to major differences in the results.

Ln 319: The sample size for soil temperature is indeed low, therefore we suppressed the sentence.

Ln 354: The d-values are all calculated using equations 1 to 3, explained in the method section in LINES 256-261.

Ln 368ff: Numbers are now added in table 3.

Ln 372: Also added in table 3. Also s.e. is adjusted, an error occurred in its calculation.

Ln 413: Unit is added.

Ln 419: The study by Duxbury et al. was conducted on drained organic soils of Florida cultivated with sugarcane, under grass or kept as fallow. The authors mention high

N mineralization rates (600-1200 kg N ha-1 y-1). The N2O emissions peaked during wet months and the authors hypothesized that most of the mineralized N was denitrified. The study does not mention fertilizer application. It also does not provide much information on land use history (time since drainage, drainage depth, previous use of the land). High mineralization and N2O emission rates may have been the result of a recent land-use change associated with deep drainage. There is no reason to exclude these results; the experimental design was sound and fluxes from organic soils are critically lacking. With respect to studies with high N fertilization rates, these were discarded because they were not representative of common practices. The cases with high N application rates that were excluded were not consistently reporting high N2O emissions. The Duxbury et al. results were not included in the meta-analysis, because no control site was studied. We showed the impact of these cases on the average fluxes on LINES 438-444 and added a short description of the study (LINE 440).

Ln 457: This is true, both numbers are based on a different sample size. A sentences is added to address the reduction in sample size when the meta-analysis is used (LINE 490-491)

Ln 462: A robust assessment of LUC effects should be evaluated through a meta-analysis which includes pair-wise comparisons. We added some recommendation on studies' design and data collection for strengthening the evaluation of LUC effects (LINE 491-499).

LN 491: A few cases in the dataset used a single bulk density value which was applied to monthly volumetric/gravimetric water contents for WFPS calculation. This is the reason why some WFPS were > 100%. Nevertheless, the values up to 130% are artefacts of the regression and the figure as well as the references in the text are adjusted to reflect the dataset maximum values only.

Figure 1: Figure 1 is deleted, in LINES 296-299 a few lines were added describing the observed trend.

Figure 2: The objective of the figure is to display the spatial variability of the LU cases in the dataset. We think that spatial variability can be best presented using a map instead of a table, in this way it is easily noticeable which regions are unequally represented and which LUs are not studied everywhere. Fig 2(c) also shows forest cover loss together with LUC cases; all these information grouped into one Table might be more complicated to read than when arranged inside a figure.

Figure 3: See our reply on LINE 491 above. All cases in the LU database (S1) that reported WFPS values are used for the figure. We clustered in 10% WFPS intervals and present the actual WFPS averaged values, which is why the points are not exactly on 10, 20, 30, etc. A clarification is made in the method section (LINE 242).

Figure 5: The figure was deleted, in LINES 364-370 these results were already described.

Figure 6: 3d plots would be ideal in situations with 2 predicting variables. We have tried, but due to the high variation along the y and z axis this would be hard to interpret. We therefore prefer a 2d presentation as currently used.

Table 1: Thank you for pointing out the high litterfall values, there was an error in the dataset which was corrected. These values were litter on the ground instead of annual litterfall rates. The inconsistencies, such as with soil and air temperature, indeed arise from combining and comparing different data sources. See also the reply to a comment on LINE 319. We added a line in the result section that cautions for this (LINE 327-329). The columns without statistics indicate non-significant differences between LUs. This is explained in the footnote of the table, see LINE 882.

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