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Biogeosciences Discussions

Interactive comment on "Nitrous oxide emissions at the landscape scale: spatial and temporal variability" *by* K. Schelde et al.

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We thank the two reviewers for their thorough reading and their very constructive comments and suggestions, which have helped us to improve the manuscript. We have carefully tried to address the issues raised and to revise the paper accordingly. In the revision we have been able to incorporate nearly all the suggestions of the referees as explained in our responses to each reviewer. Below the essence of the questions and suggestions of the reviewers (RE) are given along with our author (AU) replies:

REVIEWER # 1.

RE1: Introduction, Pg 11944, the objectives of this study at the end of the introduction is not very clear.

AU: We have rephrased the description of the objectives.

RE1: Section 2.1, Pg 11945, Line 25-26: please clarify..:

AU: We mean '10mL gas samples were taken at four intervals between 0 up to 70min (CH1, CH3) or 0 up to 90 min (CH2)'.

RE1: Section 3.1 Meteorology, pg 11947: You need to define what you mean by "reference evapotranspiration".

AU: Reference (or potential) evapotranspiration was calculated using the Makkink equation, as was explained in the next few lines. We will expand on these calculations and their applicability in the revised manuscript.

RE1: Section 4.1, pg 11951, line 10: how can you make a comparison between the different chamber types for N2O fluxes when no statistical analyses were made?

AU: In principle we cannot, due to the different locations and timing of sampling. However, a new table of means and variation per chamber type and location will show that mean emissions were equal between chamber types 1 and 2 when located within few meters within the same field.

RE1: Also line 4: suggest delete "low technology methods" and replace by static chamber method.

AU: Agreed.

RE1: Was the slurry applied after the chamber frames were inserted or before so that chambers can be positioned in a way that can resolve the temporal variations?

AU: All chamber frames were installed prior to, and remained in place during, and after, slurry application. This information is clarified in the revised manuscript.

RE1: Table 1. Add another column for the measurement period for each site.

AU: We will clarify the short versus long measurement periods in Table 1.

RE1: Fig 7 b: Don't think this is necessary as they don't show extra information in the background flux level.

AU: The lower panel of Fig. 7 will be omitted.

RE1: Fig 8: It will be better to delete the units (g N kg-1 DM) from the x scale and added to the text.

AU: Agreed; this will simplify the figure.

RE1: Fig 8 c: the x axes should be "N_25-50 cm" and not "N_0-50 cm"?

AU: The lower panel was actually supposed to be values for the 0-50 cm interval but as you also point out, that did not match with the comments made in the text. In the revised manuscript we instead include the proper data for the 25-50 cm interval. The trend here was the same (low fluxes concurring with dry periods).

RE1: Introduction, Pg 11944, line 2: Revise to something like:. . . AU: Agreed.

RE1: Section 2.1, Pg 11945. Please define each treatment/site after first mentioned... AU: Done.

RE1: Pg 11945, line 20 & line 24, Pg 11946, line 25 and Pg 11947, lines 1-8:

AU: All suggestions for clarification adopted, thank you.

RE1: Pg 11946, line 8, Do you mean..?

AU: We mean "The HMR tool will analyse non-linear concentration time series based on the model by Hutchinson and Mosier (1981)."

RE1: Section 3.1 Meteorology, pg 11947: Consider revision to..

AU: Rephrased as suggested, except that measurements of soil water content at 5 cm depth of field Wheat 2 were only made during the intensive measurement period.

RE1: Section 3.2, pg 11948, line 8: . . .The same low range?

AU: We mean: N2O fluxes were equally low.

RE1: Section 3.3 Spatial variability, pg 11948, line 19-21.

AU: We mean: "During the 2009 intensive campaign, CVs, calculated per chamber type and site, ranged between 60% and 140%. Practically all measured fluxes were positive."

RE1: Line 25-28, Please change to.. AU: Rephrased as suggested.

RE1: pg 11949, line 24-26: you mention words like "trend" or "somewhat higher fluxes" etc.. can you do statistics to resolve these ?

AU: Fig. 5 and associated comments are removed from the manuscript.

RE1: Section 3.4, pg 11950, first paragraph, please revise. AU: Has been rephrased.

RE1: Also line 17, Consider revising to.. AU: Rephrased as suggested.

RE1: Section 4.2.2 Arable land, pg 11954.

AU: Has been revised with regard to English language.

RE1: Page 11955, line 12, Please specify what kind of deep litter?

AU: We were mainly referring to dairy or pig house deep litter with a high content of straw. This has now been specified with a reference.

RE1: Pg 11956, in the paragraph Line 10-16: ..Need revising.

AU: The suggestions have been taken into account. And the correct lower panel of Fig. 5 has been included to match with the text.

REVIEWER #2

RE2: The authors should address concerns related to using four different static chamber designs during the intense campaign, and constructing annual budgets from infrequent discrete sampling with no control plot.

AU: The campaign was initiated to capture the temporal evolution in N2O emissions during a period with targeted fertilization at several sites. Different chambers were applied at different sites to improve spatial coverage. We agree with the reviewer that comparisons between these chamber types are difficult and therefore the figures attempting to compare sites and land-use types while applying different chambers, have been taken out. Also, the discussion has been extended with details, and uncertainties, on the calculation of the annual N2O emissions budget.

RE2: Suggest just presenting means/medians for the intensive April 2009 periods between chamber types, landscape positions and land-use types, plus/minus some indication of variation. Discuss the difference in water balance between 2008 and 2009 and the influence of soil texture sooner rather than later in the Results and Discussion. Suggest shortening and cutting out superfluous sections of the Discussion that read like a literature review.

AU: The figures taken out have been replaced by a table of means and standard error in measured emissions during the intensive period. Also, parts of the discussion (Rdiff) have been taken out.

RE2: Title should likely be changed.

AU: We adopt the suggested title to distinguish from the conference paper.

RE2: Li. 1-4 (11946): were reference gas standards and He blanks taken to the field and back with the other vials? How was QA/QC maintained between the three different labs and GCs?

AU: Gas standards and blanks were only handled in the laboratories. There was limited QC between labs except that a few samples taken concurrently by CH1 and CH2 technicians in five CH1 chambers were analysed and compared between the two labs. Results showed that measured concentrations agreed within 10-15 ppb and with an R2 in a scatter plot of 0.89. However, since this was a check rather than a QA/QC

measure, this is not described in the text.

RE2: Line 19-24 (11946): Describe the met station. Where were the Bowen Ratio instruments located? How was ET gap-filled?

AU: The met station delivering climate data for the 2007-2009 period was a standard met station continuously operated by the Danish Meteorological Institute and in our view needs no description of instruments etc. The Bowen ratio instruments were only installed during the intensive campaign, which has now been pointed out more clearly. Their location is now mentioned.

RE2: Results- Li. 17-21 (11947): why was actual ET Âń reference ET?

AU: Reference ET was potential ET representing a well-irrigated grass surface. Due to the dry soils and crop water stress limiting transpiration, actual ET was lower than reference ET. We now added more detail to this paragraph.

RE2: Li. 6-8 (11950): No irrigation effect, Fig. 3 shows CH3 with same pattern as CH1 (irrigated)

AU: We agree that no irrigation effect was evident and will omit the 'possible small peak..' remark.

RE2: Too many figures of flux time series of intense campaign. Suggest just presenting means/medians for the periods between chamber types, landscape positions and land-use types.

AU: Figures 3-5 have been omitted in the revised manuscript and the suggested table has been included.

RE2: Figure 2 – what type of linear regression does the slope refer to? Model I or Model II? Ordinary least squares? A model II regression (eg. Geometric mean) is most appropriate in this case, and would have a slope closer to 1.4 if the slope that is given is for an OLS regression.

AU: The regression was an OLS regression considering the HMR model as a function of the Linear model, since the linear model can be viewed as the (up to now) 'reference' analysis method.

RE2: Were not all chamber types measured on a given day? Why not? Or is this a case of issues with the chamber data or P-values on certain days?

AU: During the intensive period, not all chamber types were measured on a given day. The measurements were part of a larger campaign measuring N fluxes in the landscape, and the individual sampling teams had to coordinate sampling with other activities, trying to sample in limited time slots to avoid temperature effects. The data quality criteria rejected some datasets, but chambers showing no significant flux were assigned to the value of 0 g N/m2/s, and all (5 or 6) chambers in a group contributed to the calculated mean flux.

RE2: Figure 3, 4, 5 – suggest deleting this figure and presenting the data as means/medians plus/minus S.E./range for the entire period, and for before and after slurry application and comparing them in the text between the landscape positions.

AU: We take out figures 3-5 and present the results in a table, for periods before/after slurry application when applicable.

RE2: Figure 6 – this figure may be included.

AU: We keep figure 6 to show the temporal development in emissions at slurry-amended fields and a field not supplied with slurry during the period.

RE2: Figure 7. Indicate the timing of manuring/fertilization events with arrows for Arable1 and Arable2. Not sure of the value of including the lower panel, suggest just using the upper panel as Figure 2.

AU: The main fertilization events have been indicated in Figure 7 and the lower panel has been omitted.

RE2: Discussion- Li. 10-17 (11951) – provide means/medians during concurrent sampling periods at least and compare them if/when possible.

AU: All chamber types are now included in the new table and compared when possible.

RE2: CH4 chambers, maybe the data from these chambers should not be mentioned or presented if most of these chambers did not capture the slurry application?

AU: We think there is a point in presenting the data and pointing out how the smallest chambers do not capture field-scale management effects.

RE2: Li. 17-27 (11951) – see also Venterea, 2010 (J. Environ. Qual. 39: 126-135).

AU: This paper is now added to the list of studies pointing out the need to apply more than a linear regression to calculate fluxes. However, the Venterea study is one where a linear regression is applied first, and then the 'error' is estimated and corrected for. Rather than holding on to simple spreadsheet solutions, we advocate the application of a non-linear regression method that is neither complicated nor ambiguous, using the freely available HMR tool.

RE2: Li. 5 (11952) – comparing median emissions is likely more appropriate given the data is not normally distributed.

AU: These samples were very small (5 or 6 chambers to average) and even if the individual samples might not all pass a normality test, we choose to stick to calculating the means.

RE2: Li. 8 (11954) – suggest deleting the words "crop establishment spring and autumn and". The N2O fluxes are due to manure and synthetic fertilizer application. How much precipitation fell during those periods following nutrient application? WFPS? Water balance? Influence of soil texture (sandy loam in present study) on magnitude of emissions observed compared to other studies?

AU: We agree on the suggestion and we now make reference to the more detailed

discussion on soil water balance in the following section.

RE2: Li. 4-7 (11955) – see magnitude of emissions reported by Denmead et al. (Agric. For. Meteorol. 150 (2010): 748-756).

AU: Thank you for pointing to another study with unusually high N2O emissions. We have included the reference.

RE2: Li. 21-26 (11956): was crop or vegetation type accounted for in the reference evapotranspiration calculations? Were the Bowen ratio measurements of actual ET used in any way?

AU: The reference evapotranspiration was representative of an irrigated grass surface and probably close to the potential ET of the range of land use types investigated here. As mentioned the Bowen ratio station was only installed during the intensive campaign and the cumulative data was used for indicating soil water stressed conditions for the crops during the period.

RE2: Li. 24 (11957) – Li. 16 (11958) – not sure how this relates to the data presented in the current discussion paper. Suggest deleting.

AU: For future studies we suggest Rdiff could be a better parameter than WFPS or our equivalent calculated soil water balance for indicating conditions prone to emissions. However, we admit that this is somewhat speculative, given that Rdiff measurements were not included in our study. This section has been deleted.

RE2: Li. 18-20 (11958): how were the annual emission budgets calculated? Were they linearly interpolated between measurements? See Mishurov and Kiely (Agric. For. Meteorol. 151 (2011): 1763-1767) on gap-filling N2O fluxes.

AU: According to the Mishurov and Kiely classification, the approach taken here was a combination of linear interpolation and look-up table. For periods with relatively frequent sampling, linear interpolation was applied. For periods with more infrequent sampling, the environ-mental and management conditions prevailing at the time of the

gap were considered: Emissions during early spring were assumed to remain at a low value (measured before the time gap) and not start increasing until the time of fertilization, after which linear interpolation was applied.

RE2: Li. 2-5 (11959) – should also mention influence of using non-linear regressions vs. linear regression on the inherent flux underestimation with static chamber technique.

AU: Agreed; this has been included.

RE2: Li. 8 – 11 (11959): Note that the IPCC emission factor accounts for background emissions before applying N. There were apparently no control plots (eg. 0 kg N applied) to the arable land in this study so unless you subtract a global background emission estimate first (1 kg N2O-N ha/yr following Bouwman, 1996) or estimate what it may be in another manner you need to be mindful of the wording. Stating the fraction per unit N input is okay, but you should be cautious about speculating and assuming how much of the N2O flux originated from manure or synthetic fertilizer vs. organic matter mineralization.

AU: Yes; agreed. We will calculate the emissions fraction of applied N but will stress that there are other (unaccounted) N sources than the fertilizer contributing to the emissions.

Interactive comment on Biogeosciences Discuss., 8, 11941, 2011.