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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.

Anonymous Referee #2

Received and published: 7 February 2012

General Comments: This manuscript is on a timely topic for biogeochemistry researchers, investigating the variation in N2O fluxes from a common soil (sandy loam texture) under natural vegetation and various agricultural management regimes in the same geographical region in Denmark. It is generally well written with only a few minor typos and errors noted that should be caught with a careful proof-reading by the authors. The use of reference evapotranspiration and precipitation analysis (running water budget) to identify dry and wet periods is fairly novel and interesting when applied to interpreting a N2O flux time series.

Although, the authors use a fairly robust approach to analyses (non-linear vs. linear regression model) and state important considerations regarding interpretation of the data set collected, the static chamber measurement methodology used is prone to error

and uncertainty. This error and uncertainty is related to both design and use of static chambers (particular the ones that are not vented), as well as, the infrequent collection of gas samples in the study. 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 plots.

I recommend refocusing the story being told here, major reorganization of the paper and selection of Figures presented. Focus on the bigger picture of the longer time series of Arable1, Arable2, and Meadowa, as related to agricultural management and the water balance analysis. Narrow in briefly and concisely on the intense measurement campaign and how it relates to the longer period of measurement in the region, and any implications for the low frequency of measurements used to construct the annual budgets for the three longer term sites. The focus on the intense campaign in this discussion paper is not worthwhile, the fluxes are very low (they are lower than the definition of low fluxes given by the authors Li. 8 (page 11952)!), and the measurements are apparently not concurrent or made with consistent chambers. 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 and are not directly related or supported by the data presented (such as the section about Rdiff).

Specific Comments:

Title-

Should likely be changed, as it is pretty much the same as the following conference proceedings: http://nitrogen.ceh.ac.uk/nitrogen2011/_oral_presentations/S12_2_Schelde.pdf

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Interactive Comment

Something like “Spatial and temporal variability of nitrous oxide emissions in a mixed farming landscape of Denmark” is better.

Materials and Methods-

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?

Line 19-24 (11946): Describe the met station. Where was it located, what instruments/sensors were used, make/model. Where were the Bowen Ratio instruments located? How was ET gap-filled?

Results-

Li. 17-21 (11947): why was actual ET « reference ET?

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

Figures-

Too many figures of flux time series of intense campaign. Focus on intense campaign is not worthwhile, fluxes are low, measurements are not concurrent or made with consistent chambers. Suggest just presenting means/medians for the periods between chamber types, landscape positions and land-use types.

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.

Figure 3 – 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? Suggest presenting the data as means/medians plus/minus S.E./range for the entire period, and for before

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and after slurry application and comparing them in a table or in the text between the chamber types.

Figure 4 – 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.

Figure 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 land-use types.

Figure 6 – this figure may be included. Suggest 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.

Figure 7 – this should be Figure 3 in my opinion. Also, 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. If the lower panel is kept, indicate how the small symbol Arable1 values were estimated.

Discussion-

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

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

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

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

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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?

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

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? Why not?

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

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.

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.

Li. 8 – 11 (11959): be careful how you word this. 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 ($\sim$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.

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