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Interactive comment on "Methane and nitrous oxide exchange over a managed hay meadow" *by* L. Hörtnagl and G. Wohlfahrt

L. Hörtnagl and G. Wohlfahrt

lukas.hoertnagl@usys.ethz.ch

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We thank Reviewer #1 for taking the time to review the manuscript and his / her detailed and valuable input. We will use this input to prepare a revised version of the manuscript where we address the points of discussion raised by Reviewer #1. Please find below our point-by-point reply to the detailed comments by reviewer #1:

General Comments by Reviewer #1:

"This is an interesting paper based on a scientifically important and valuable dataset of CH4 and N2O EC measurements. The gap in knowledge and data surrounding 'CH4 and N2O emissions' is far from complete, and therefore this paper could add significantly to the answers on some scientific questions. The paper could have answered

relevant scientific questions, however, the focus, the structure of the paper and descriptions of research questions and accompling conclusions are somewhat scattered. See below for more detialed suggestions. The methods that the researchers/authors used for reaching their goal (determination of annual GHG balance) are generally accepted and are overall described in a transparent way. However, the data analyses for answering the reasearch questions could be improved by taking some 'extra steps' and it seems that some very relevant existing literature has been missed in their considerations for data analyses. See below for more detailed comments."

Detailed Comments:

Comment #1: "The introduction is broad and quite long, it has much tekst/information on the atmospheric composition and the impacts of climate change, while this is not really a focus of the publication. The focus is on the plant/soil-atmosphere exchange of GHG's and the driving variables. The intro could be improved by making it shorter and more focussed."

Reply #1: The introduction is meant to provide the background for our study and thus necessarily needs to review the role that the investigated GHGs play in atmospheric chemistry and the resulting implications for the global climate. We though agree that the background may have been too comprehensive and for the revised version of the manuscript we will thus shorten the introduction where possible and conversely put more emphasis on plant and soil GHG exchange.

Comment #2: "What exaclty is the objective of this paper? Currently it is stated that the objective is to compared the resuts with existing data. I dont think that that is really the objective. I If I understand it correctlyy, the objective is to 1) measure fluxes in this specific ecosystem to 2) couple emissions to driving variables 3) to determine the annual total GHG balance, including existing data of CO2 4) to compare outcomes to previous studies and to 5) find mitigation strategies to reduce emissions. The discussion and conclusion should comply with these objectives."

Reply #2: We will add points 1-3 of the reviewer to the objectives (point #4 is mentioned already). As for point #5, we feel that this would be out of scope as we are in no position to find or recommend mitigation strategies to reduce emissions within the scope of this publication.

Comment #3: "The paper needs some restructuring and the objective and research questions of the paper needs to be in line with the results, discusison and conclusions. The methods, results and discussion sections are not build up in a consistent way. For the methods and result section, the authors could consider to use a consistent order of writing up of the calculation of an annual balance from 20 Hz data. E.g.: 1) How are half hour fluxes determined from 20 Hz raw data 2) How are day fluxes determined from half hour data (how dealing with diurnal variability, gaps, processing) 3) How are seasonal fluxes determined, dealing with seasonal variability 4) How are annual fluxes derived from day-data. "

Reply #3: As suggested by Reviewer #1, we will check the entire paper for consistency issues and modify it accordingly for the revised version of the manuscript.

Comment #4: "The manuscript could improve a lot if the following points should be considered: In many papers multiple regression is done for LN transformed CH_4 and N_2O fluxes since the dependency of underlying processes if often exponential: e.g. microbial activity is exponentially related to Tsoil. It seems that the authors did all regressions with non-tranformed data. The suggestions is to re-do the analyses with LN transformed data to get more robust and more scientifically based results, closer to reality."

Reply #4: We agree and will redo the multiple linear regression (MLR) analysis with LN transformed CH_4 and N_2O fluxes as suggested.

Comment #5: "Step wise multiple regressions could be done (see other studies) to stepwise eliminate variables that do not significantly contribute to the predictive power of a regression (or that overlap with other variables). The authors could consider to

re-do the regresssion by using this approach and end up with 2 or 3 variables that together explain a larger part of the variability. The suggestion is to at least test for Tsoil and SWC (and if there is water table data, also water table depth could be a good candidate for extra explanatory power)."

Reply #5: We will include the results of a stepwise MLR in a revised version of the document.

Comment #6: "To improve the predictive power of regressions, the dataset could be split in sets that cover the 'active ranges' of microbes (both the formation of CH_4 and N_2O are driving by the microbial communities). E.g. take the Tsoil range of 10-25 0C and a more specific tsoil moister range and re-do the analyses. Look into the literature what the 'active ranges' are for both gases (and thus the ranges that a clear relation between temperature and emissions and/or soil moisture (WT) and emissions is expeted). It is e.g. known that in terms of water table depth, the CH_4 emission is close to 0 if water table's drop below -30 cm, while emissions usually increase exponentially in the range -20 cm below field level to 0 WT."

Reply #6: That is an excellent idea and will be done in a revised version of this paper.

Comment #7: "To improve the predictive power of the regressions not only the data in 'management event periods', but also the data in periods of snow cover could be eliminated from the dataset."

Reply #7: As stated in the description of Table 1, management data were excluded from the regression analyses. Although we already conducted the MLR for certain time windows to account for changing environmental conditions including snow cover and snow free conditions, we will exclude the snow covered time periods from the analysis that covered all years. This accounts for the suggestion of Reviewer #1.

Comment #8: "After performing the additional analyses mentioned above, the best models could be chosen to fill the data gaps that exist. Annual numbers could then be determined from a ‘ complete’ dataset.”

Reply #8: Annual numbers were determined from a complete, gap-filled dataset. We will rephrase the respective section in the manuscript to make this clear.

Comment #9: “The manuscript would improve from a detailed description about how is dealt with data gaps. E.g. describe in a more clear and strucutre way: 1) data coverage before processing 2) data coverage after correction and filters (including a detailed description and discussino on the FIR filtering, see below) 3) coverage of half hours and days and 4) how is dealt with data gaps.“

Reply #9: This would be outside the scope of this manuscript and merits its own research paper. We have done similar tests for VOCs recently (Bamberger et al. 2014). Some of the numbers demanded by Reviewer #1, e.g. data coverage, are already given in the manuscript. As suggested by Reviewer #1 we will include information about the filling of data gaps for annual numbers. Please note that correction and FIR filtering of fluxes does not result in a loss of data, i.e. both methods do not eliminate data points.

Comment #10: “One of the results of this research is that the FIR filtering influences the results of the annual balances dramatically. This means that the paper should have a focus on this filtering: why is this filtering done in the case of this site, should this filter be applied for the calculation of annual balances (for this site and more general) and which filter should be (is) used. What is the impact of the different FIR’s on the total balance etc. But specifically in the discussion: what filter is recommended and why and in the methods section: what filter is used for the calculation of the final balances + jusification.”

Reply #10: Most of the recommendations regarding FIR filtering raised by Reviewer #1 are already addressed in the manuscript: Reasons for the FIR filtering and which FIR filter was used for the analyses are given in Section 2.5 and Section 3. The impact of different FIRs on the total balance is given in Section 3 and shown in Figure 6. Because the present manuscript is a single site study we are not able to give recommendations

regarding other field sites other than to check for an overestimation of fluxes in the low spectral range. We will include this recommendation in the manuscript.

Comment #11: “For emission numbers in the tekst (CH4 and N2O) the uncertainty should be given. Also in figures, such as e.g. fig 4 and fig 6 (uncertainty bands). The manuscript should improve from a figure or table that clearly shows the final numbers for CH4 and N2O emissions for the site for 2010 and 2011, including the STDEV’s. And e.g. table 2 should include STDEV’s for the group means.”

Reply #11: As suggested by Reviewer #1, we will include the uncertainty for emission numbers where possible and in Figure 6. An indicator for uncertainty is already included in Figure 4. Following Reviewer #1, we will include STDEVs for the group means in Table 2. Final numbers are only feasible for 2011 and are already in the manuscript. Random errors, which would typically be represented by a STDEV or alike, are very small on an annual timescale because these decrease with the square root of the number of measurements. In contrast, the major source of uncertainty is the choice of the FIR filter time constant, which is already treated extensively in the paper.

Comment #12: “The authors should have compared their numbers with the numbers of comparable sites, otherwise it is confusing and conclusions could be biased. Most comparisons are with peat sites (Hendriks et al 2007, Baldocchi et al., Schrier-Uijl et al etc etc), peat sites are very different in their processes and carbon content and given the hydrology also very different in moisture regime and vegetation. Besides, management has high impact on the height of fluxes, so also the management of the different sites that are used for comparision should be described in more detail (e.g. the site of Hendriks in an abandoned sites under restoration with no management). A comparision table could improve the overview. This table should take into account different climate zones. Given (one of) the objective of the study (to compare different studies), this should be given more attention. The 10 sites of Sousanna et al that are mentioned in the tekst should be split up and described (perpaps also in this table).”

Reply #12: Publications describing year-round eddy covariance GHG measurements of all three compounds (CO2, CH4 and N2O) over managed grassland are still scarce, available data from peer-reviewed journals have been included and discussed in this publication. Still, setting our findings in relation to other ecosystems, e.g. peatland, is an important step in understanding ecosystem fluxes on a global and regional scale. Information about the different sites that were used in the discussion as demanded by Reviewer #1 are already given in the text, e.g. regarding Hendriks et al. (2007). As suggested by Reviewer #1, we will explore the possibility of including a comparison table in a revised version of the manuscript.

Comment #13: "The authors overlooked some scientific publications that did similar analyses, which is a pity because they could have taken the advantage of reading these. An example of a study that could have helped the authors is that of Kroon et al., 2010 in the European Journal of Soil Science. They calculated CH4 and N2O annual balances for a meadow in the Netherlands based on three years of Ec data and proposed gap filing procedures etc."

Reply #13: We are confident that we included the majority of EC publications that are dealing with year-round fluxes of CO2, CH4 and N2O, but as pointed out by Reviewer #1 we missed out on the publication by Kroon et al. (2010). We will complement our references by adding the paper by Kroon et al. (2010) and other publications we might encounter during the revision of this manuscript.

Comment #14: "Units should be consistant troughout the manuscript. Since the focus is on finding ecosystem-based parameters that explain the CH4 and N2O fluxes and determining the GWP of the Neustift site, the suggestion is to express everyting in (m)g CH4/N2O m-1 yr-1 and CO2-eq m-2 yr-1. Not in terms of carbon (CO2-C or CH4-C) or (n)mols. Unless the authors change the scope of the manuscript and also focus on the carbon-balance or atmospheric compositions etc. The authors should consider making the units consistent (also in the figures, e.g. figure 2)."

Reply #14: We will follow the suggestion of Reviewer #1: in a revised version of this manuscript half-hourly flux data will be converted from molar to mass fluxes of the respective molecule, the same will be done when presenting annual, cumulative fluxes; carbon or nitrogen-only mass based units will be abandoned. CO2-equivalents will only be used when the GWP is calculated and compared to the GWP of sites in other studies.

Comment #15: "The authors attribute most of the differences between previous studies to the heterogeneity in the field and the unability to separate emission hotspots. One point (that has been mentioned earlier) is that by drawing such conclusions the authors must make clear that the sites were they refer to are comparable. In addition, could additional footprint analyses shine some light on this issue? I believe that currently there is software available that on a quite detailed scale the origin of fluxes could be tracked back. Please consider this."

Reply #15: This is an interesting suggestion. Calculating the footprint (2D) would be an easy task, finding hot spots within the footprint however would have to be done via statistical analyses in combination with detailed, spatial data (vegetation, soil, water content…) about small areas and patches within our flux footprint – unfortunately we currently lack information this detailed and as a consequence the significance of our findings would be very limited. In addition, a footprint analysis that detailed as part of the current publication would make this publication even longer. We therefore think that the suggested footprint analysis merits its own research and publication and that we are currently not in the position to follow up on this suggestion.

Comment #16: "There is a remarkable large difference in N2O emission between 2010 and 2011. What is the reason?"

Reply #16: Unfortunately Reviewer #1 does not explain what number he / she is referring to. Daily average fluxes were about the same in both years (Fig. 2), while diurnal cycles generally showed more emission in 2011, except during a period between snow cover and the 1st cut 2010 when N2O fluxes were characterized by larger variability (Fig. 3, Fig. 4). Cumulative numbers are only given for 2011 (Fig. 6).

Comment #17: “The CO2 results are from a previous study. These results do not have to be described in the results section.”

Reply #17: We disagree on this point. First, data from that particular year (2011) have not been published previously (Wohlfahrt et al. 2008 published data from 2001-2006 from this site). Second, as we link CH4 and N2O exchange with CO2 fluxes in terms of CO2 equivalents, the CO2 data need to be described, otherwise it would be difficult to follow our reasoning.

Comment #18: “In the discussion there should be some more discussion on potential emission hotspots and the impact on the balances.”

Reply #18: See our reply to comment #15. We will outline further research to better address this issue in the discussion.

Comment #19: “There could be discussion on mitigation strategies since this is mentioned in the intro.”

Reply #19: Developing or recommending mitigation strategies are not objectives of this paper – a discussion on the topic would shift the focus of the manuscript. We still feel that mitigation strategies as one of the reasons for GHG-research should still be mentioned in the introduction, mainly to embed discussed topics in a broader, global setting.

Comment #20: “In the discusion there could be some more discussion on the comparison with IPCC default data.”

Reply #20: We will follow the suggestion of Reviewer #1 and will include respective information in the discussion.

Comment #21: “The numbers that are mentioned in the discussion are not consistent

are sometimes unable to track back. E.g. in line 24 page 8204. What does the respective balance in Neufit mean? Is that including the numbers that are presented in fig 6? Likely not, since the NEE is -64 g CO2-eq m-2.”

Reply #21: “Respective balance in Neustift” relates to the GWP balance based on CO2- and CH4 fluxes alone (not including N2O) that was presented for a boreal fen one sentence earlier – will reformulate to make this unambiguous.

Comment #22: “Also line 6-7 on pshr 8205: is similar to -32 g CO2-eq m-2. Please specify, is this with FIR filter? I can not track back the calculation of this number.”

Reply #22: Yes, this is with FIR filter. As stated in Section 2.5, FIR filters with time constants of 50 / 100 s were used for final, best guess CH4 / N2O fluxes. In a revised version of this manuscript we will make it clearer that these data were used for all subsequent analyses.

Comment #23: “The -19.2 g CO2-C mentioned in line 20 page 8205, is that calculated from the -32 g CO2-eq (line 6, page 8205)? Then the calculation is not right. Please be consistent in units, and explaine where numbers come from.”

Reply #23: The information is already given in the text, please note the used units. The number in question, -19.2 g CO2-C m-2 yr-1, refers only to the carbon associated with CO2 exchange, hence the unit. The conversion from -70.5 g CO2 m-2 yr-1 for 2011 is correct. In addition, it is clearly stated in the text that -32g CO2-equ. m-2 yr-1 is the net global warming potential (GWP) by adding up contributions of CO2, CH4 and N2O in 2011.

Comment #24: “Suggestions for Paragraph 4.3: give clear numbers for the total balance, including standard deviations (CH4, N2O and CO2), perhaps in a table or figure“

Reply #24: Clear numbers for the total balance are already given in Section 3. We will follow up on the suggestion by Reviewer #1 and also clearly state this number in Section 4.3. We will also explore our options of including this number in one of the figures, preferably Figure 6.

Comment #25: “describe in the methods section what GWP’s have been used for the different compounds“

Reply #25: As suggested, we will include the respective information from the introduction also in Section 2.

Comment #26: “Compare with other studies, but make clear if it is for the total balance (including CO2, CH4 and N2O) or for the partial balance (CO2 and CH4 only, or any other balance).”

Reply #26: This is already clearly stated throughout Section 4, please note the units.

Comment #27: “Express the numbers in CO2-equivalents, not in carbon.”

Reply #27: We will follow the suggestion by Reviewer #1 and express respective numbers in terms of CO2-equivalents.

Comment #28: “The conclusion needs rephrasing and needs to be in line with the objectives and hypothesis.”

Reply #28: As suggested by Reviewer #1, we will look into consistency issues in our conclusion and rephrase where necessary.

Interactive comment on Biogeosciences Discuss., 11, 8181, 2014.