

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

Anonymous Referee #1

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General comments:

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 accomplishing conclusions are somewhat scattered. See below for more detailed 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 trans-

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parent 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.

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.

What exactly 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 correctly, 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.

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.

The manuscript could improve a lot if the following points should be considered:

• In many papers multiple regression is done for LN transformed CH4 and N2O

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fluxes since the dependency of underlying processes is often exponential: e.g. microbial activity is exponentially related to T_{soil} . It seems that the authors did all regressions with non-transformed data. The suggestion is to re-do the analyses with LN transformed data to get more robust and more scientifically based results, closer to reality. – Step wise multiple regressions could be done (see other studies) to step-wise 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 regression 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 T_{soil} and SWC (and if there is water table data, also water table depth could be a good candidate for extra explanatory power). – 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 driven by the microbial communities). E.g. take the T_{soil} range of 10–25 °C and a more specific t_{soil} moisture 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 expected). 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. – 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. – 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.

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

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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 + justification.

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.

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 comparison should be described in more detail (e.g. the site of Hendriks in an abandoned sites under restoration with no management). A comparison 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).

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

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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.

Units should be consistent throughout 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 everything 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).

The authors attribute most of the differences between previous studies to the heterogeneity in the field and the inability 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 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.

There is a remarkable large difference in N₂O emission between 2010 and 2011. What is the reason?

The CO₂ results are from a previous study. These results do not have to be described in the results section.

In the discussion there should be some more discussion on potential emission hotspots and the impact on the balances.

There could be discussion on mitigation strategies since this is mentioned in the intro.

In the discussion there could be some more discussion on the comparison with IPCC default data

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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 $-64 \text{ g CO}_2\text{-eq m}^{-2}$.

Also line 6-7 on pshr 8205: '...is similar to $-32 \text{ g CO}_2\text{-eq m}^{-2}$...'. Please specify, is this with FIR filter? I can not track back the calculation of this number.

The $-19.2 \text{ g CO}_2\text{-C}$ mentioned in line 20 page 8205, is that calculated from the $-32 \text{ g CO}_2\text{-eq}$ (line 6, page 8205)? Then the calculation is not right. Please be consistent in units, and explain where numbers come from.

Suggestions for Paragraph 4.3: $\hat{\text{A}}$ give clear numbers for the total balance, including standard deviations (CH_4 , N_2O and CO_2), perhaps in a table or figure $\hat{\text{A}}$ describe in the methods section what GWP's have been used for the different compounds $\hat{\text{A}}$ Compare with other studies, but make clear if it is for the total balance (including CO_2 , CH_4 and N_2O) or for the partial balance (CO_2 and CH_4 only, or any other balance). $\hat{\text{A}}$ Express the numbers in CO_2 -equivalents, not in carbon.

The conclusion needs rephrasing and needs to be in line with the objectives and hypothesis.

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