Dear editor,

Comment by Anonymous Referee:
"To the authors
I am happy that the authors revised the manuscript thoroughly following the suggestions they got. The result is that It has improved a lot.
The only thing I would recommend is to again carefully look into the work of Kroon et al (2009,2010,2011) and check/compare if their analyses comply with the extensive tests and Best Practices for CH4/N2O EC measurements and analyses that are described in these manuscripts. As I said, I was totally surprised that they did not take note of this research, it could have avoided a lot of the hurdles they have to take now. "

Response:
Following the advice by the Referee, we carefully checked all literature by Kroon et al. between 2009 – 2011 and one relevant publication from 2007.

According to *Web of Science*, the literature in question are the following publications:

> Kroon, P.S., Hensen, A., Jonker, H.J.J., Zahniser, M.S., Van 't Veen, W.H., Vermeulen, A.T.: Suitability of quantum cascade spectroscopy for CH_4 and N_2O eddy covariance flux measurements. Biogeosciences 4, 715–728, 2007.

> Kroon, P. S., Hensen, A., Jonker, H. J. J., Ouwersloot, H. G., Vermeulen, A. T., & Bosveld, F. C.: Uncertainties in eddy covariance flux measurements assessed from CH_4 and N_2O observations. Agricultural and Forest Meteorology, 150(6), 806–816. doi:10.1016/j.agrformet.2009.08.008, 2010a.

> Kroon, P. S., Schrier-Uijl, A. P., Hensen, A., Veenendaal, E. M., & Jonker, H. J. J.: Annual balances of CH_4 and N_2O from a managed fen meadow using eddy covariance flux measurements. European Journal of Soil Science, 61(5), 773–784. doi:10.1111/j.1365-2389.2010.01273.x, 2010b.

> Kroon, P.S., Schuitmaker, A., Jonker, H.J.J., Tummers, M.J., Hensen, A., Bosveld, F.C.: An evaluation by laser Doppler anemometry of the correction based on Kaimal co-spectra for high frequency losses of EC flux measurements of CH_4 and N_2O. Agric. Forest Meteorol, 150, 794-805, doi:10.1016/j.agrformet.2009.08.009, 2010c.

The best practices described in these publications are already part of our flux calculation and quality check workflow:

Our analyses are based on daily average flux values to reduce random uncertainty in comparison to half-hourly flux values, in-line with findings by Kroon et al. (2010a). We added the respective publication to our references and added one sentence to Section 2.8:
"Using daily average values of CH_4 and N_2O fluxes in the statistical analyses as opposed to 30 min flux averages reduces random uncertainty (Kroon et al., 2010a)."

Already implemented during the last revision: We followed the suggestions by the Anonymous Referee to log transform the CH_4 and N_2O flux values for our analyses to account for non-linear dependencies, and added results from a stepwise regression analysis - in line with Kroon et al. (2010b). The respective publication has been added to our references.

Extensive co-spectral analyses were an important part of this study and necessary flux corrections were applied diligently. In contrast to Kroon et al. (2010c) we found that the Kaimal co-spectra were not representing the true co-spectra during stable conditions and thus we are using site-specific model co-spectra at our study site (described in Wohlfahrt et al.,

2005a which is cited in the ms) and we stress the important of correcting for high frequency losses in accordance with Kroon et al. (2010c).

In Section 2.5, we added the following information, the respective publication was added to our references:

"The importance of correcting CH_4 and N_2O fluxes for high frequency losses was shown previously (Kroon et al., 2010c)."

Kroon et al. (2010c) also give the recommendation to investigate the effect of inadequate footprint, non-stationarity and advection. For Neustift, these parameters were not only considered during the setup of the station (homogeneous terrain...), but are also routinely checked as part of our quality control workflow to ensure fluxes of highest quality. The quality control process is outlined in Section 2.6.

In their earlier work, Kroon et al. (2007) identified proper thermal control of laser optics and electronics as important to minimize drift that could affect system precision and stationarity negatively. The laser system in Neustift was placed in a climate-controlled hut to guarantee the best possible temperature conditions. Temperature in the hut but also temperature of the optical bench and housing of the instrument were monitored at all times. During our measurement campaign, no temperature related problems arose.

We added the following in Section 2.3, the respective publication was added to our references:

"The importance of a temperature controlled environment was previously pointed out by Kroon et al. (2007)."

We feel confident that this study complies with the best practice recommendations by Kroon et al. In comparison to their studies, we implemented additional quality criteria and thus are able to present the best possible CH_4 and N_2O fluxes over the grassland in Neustift.

Best regards
Lukas Hörtnagl
Georg Wohlfahrt

Methane and nitrous oxide exchange
over a managed hay meadow

Hörtnagl L., Wohlfahrt G.

We thank both reviewers for finding the time to look at our manuscript entitled "Methane and nitrous oxide exchange over a managed hay meadow". Both reviewers brought up many valid points of discussion.

We are happy to state that we were able to address all reviewer comments. In this carefully revised version of the discussion paper we focused on addressing every point of discussion raised by the reviewers, and rephrased paragraphs whenever our statements lacked clarity. As a consequence, we feel that the quality of this manuscript has greatly improved.

Below we reply to each reviewer comment in detail and outline all edits that have been made to the text.

Thank you for your consideration.
With best regards,
Lukas Hörtnagl and Georg Wohlfahrt

Reply to Anonymous Reviewer #1

General Comments by Reviewer #1: "This is an interesting paper based on a scientifically important and valuable dataset of CH_4 and N_2O EC measurements. The gap in knowledge and data surrounding 'CH_4 and N_2O 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: We followed the suggestion of Reviewer #1 by shortening some of the paragraphs in the introduction. However, 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. In its current,

revised form the introduction is mainly focused on the plant/soil-atmosphere GHG exchange in a global context. In addition, by clearly including our objectives in the introduction we outline a comprehensible framework for our motivation (see also Reply #2).

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 CO_2 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: Following the suggestion of Reviewer #1 we added points 1-3 to the text, point 4 was already mentioned in the manuscript.

The paragraph now reads:

> "The objective of this study is to (1) quantify eddy covariance CH_4 and N_2O fluxes, (2) couple exchange patterns to independent driving variables, (3) determine the annual total GHG balance and (4) compare our findings to previous results from chamber and eddy covariance measurements at ecosystem scale and from laboratory measurements. In line with these objectives and based on earlier studies we hypothesized for both compounds that […]"

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.

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: In this revised version of the manuscript we made sure that hypotheses, results, discussion and conclusion are all in line with our motivation and objectives by making numerous modifications to the manuscript text and providing additional information where necessary. Please also see the following replies for changes made to the manuscript.

ad 1) Section *2 Methods* already gives much information about the flux calculation process, going into even more detail would probably shift the focus of this paper and overemphasize the methodological aspect of the current manuscript. Many aspects, for example the virtual disjunct eddy covariance method, have been used and described extensively in other publications. Following the suggestion by Reviewer #1, we added more information about other aspects to the manuscript.

ad 2) The calculation of daily average fluxes is already described in section *2.8 Statistical analyses*.

ad 3) Seasonal fluxes were simply determined by aggregating daily (already gap-filled) fluxes in the procedure of deriving annual budgets (see (4) below).

ad 4) We included the following paragraph detailing gap-filling and annual GWP calculation in section *2.5 Flux calculations*:

> "In order to calculate the annual balance of CH_4 and N_2O in 2011, the respective quality-controlled half-hourly flux dataset was gap-filled. Gaps less than or equal to two hours were filled by linear interpolation. For the filling of larger gaps a lookup table was generated, using flux data in a time window of 14 days around the missing flux value and T_{soil} bin widths of 1°C. If no lookup table could be generated, e.g. no flux data were available within the time window, the mean diurnal variation (±14 days) was used to fill the gap. For the calculation of the annual GWP of the meadow in Neustift, CH_4 and N_2O fluxes were converted to CO_2-equivalents using the respective compound warming potential as given by Forster et al. (2007). "

We added the following text detailing annual GWP calculation to the discussion section *4.3 Global warming potential*:

> "In this study, year-round CH_4, N_2O and CO_2 flux data were available for 2011. When expressing the net exchange of the three compounds in terms of CO_2-equivalents and adding up these different contributions, the resulting GWP of the meadow in Neustift was -32 g CO_2-equ. m^{-2} yr^{-1} in 2011, whereby a yearly NEE of -71 g CO_2 m^{-2} yr^{-1} was offset by CH_4 and N_2O emissions of 7 and 32 g CO_2-equ. m^{-2} yr^{-1}, an offset of approx.. 55%."

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: Following the suggestion of Reviewer #1, all regression analyses were redone using LN transformed flux data. These reanalyses lead to only slightly different correlations, respective numbers in the manuscript text and in Table 1 were adjusted accordingly. These new results are not in conflict with statements made in the discussion paper, i.e. do not change our conclusions.

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: Following Reviewer #1, we included the results of a forward step-wise regression in the manuscript.

+We added in section *2.8 Statistical Analyses*:

> "The natural logarithm (ln) of the observed CH4 and N2O fluxes was calculated and used in the simple (SLR) and multiple linear regression (MLR) analyses as the dependent variable."

+We added in section *3 Results*:

"We expanded on these findings by performing a forward step-wise MLR analysis using the same data, effectively reducing the number of variables in the regression equation but yielding similar results. In this analysis NEE, SHF, Tair and VPD were identified as the most significant regressors (all $p < 0.05$), explaining 25 % of the observed $\ln(F_{CH4})$ variability over all years excluding snow periods (data now shown)."

and

"Seven parameters were highly significant ($p < 0.001$) in a forward step-wise MLR analysis covering all time periods except snow cover, with T_{air}, N_2O VMR, RH, NEE and LE being positively correlated with $\ln(F_{N2O})$, SWC and H negatively (data not shown)."

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: Following the suggestion of Reviewer #1, we did an additional regression analysis and looked at the temperature and soil moisture dependence of both compounds in different categories of T_{soil} and SWC. In the case of CH_4, no clear relationship could be found between the two parameters and the flux in any of the categories. In this case it is difficult to compare the bulk EC signal at Neustift to laboratory measurements, we discussed the 'problem' of analyzing the bulk EC signal in a regression analysis already in the manuscript and further pointed it out in the conclusions.

In the case of N_2O we already show in Figure 5 suprisingly clear relationships between N_2O exchange and SWC and T_{soil}. N_2O exhibited clearer exchange patterns than CH_4, so it was easier to find relatively clear correlations. In the scope of this paper we are not able to discuss the topic of activation energy or active ranges of microorganisms in detail, because of having available only the averaged EC signal and lacking concurrently performed soil and/or laboratory measurements for meaningful discussion. However, we performed the same regression analysis like for CH_4 described in the paragraph above also for N_2O, and found a clear relationship between T_{soil} and $\ln(F_{N2O})$ in the T_{soil} category 12-16 °C at low SWC. To some extend, this correlation is also shown in Figure 5 and discussed in section 4. We added in the discussion section 4.2 Nitrous oxide:

"The latter finding is further highlighted by a clear positive correlation between daily average $\ln(F_{N2O})$ and T_{soil} in the soil temperature range 12-16 °C as long as SWC was low (data not shown)."

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. Following the suggestion of Reviewer #1, we excluded snow-covered time periods from the regression analyses when examining "all periods" in Table 1, i.e. the

columns labeled "all periods" was renamed to "vegetation period" and now includes only time periods where the meadow was snow-free. We also added this information to the description text of Table 1:

> "Results shown for the "vegetation period" do not include time periods with snow cover on the meadow."

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 added an additional paragraph in section *2.5 Flux calculations* where we also outline the gap-filling procedure:

"In order to calculate the annual balance of CH_4 and N_2O in 2011, the respective quality-controlled half-hourly flux dataset was gap-filled. Gaps less than or equal to two hours were filled by linear interpolation. For the filling of larger gaps a lookup table was generated, using flux data in a time window of 14 days around the missing flux value and Tsoil bin widths of 1°C. If no lookup table could be generated, e.g. no flux data were available within the time window, the mean diurnal variation (±14 days) was used to fill the gap."

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:

ad 1) We added information about the raw data coverage to section 2.5 *Flux calculations*:

> "In total, 28891 raw flux values were calculated for CH_4 / N_2O, which corresponds to a data coverage of 88 % over the whole measurement period between 13 March 2010 and 29 February 2012."

ad 2) Correction and FIR filtering of fluxes does not result in a loss of data, i.e. both methods do not eliminate data points. Numbers regarding the data loss due to quality control are already given in section *2.5 Flux calculations*, quality control is described in section *2.6 Quality control*. We rephrased section *2.5 Flux calculations* to make the application of the FIR filter more comprehensible:

> "The high pass, non-recursive, finite impulse response (FIR) filter was applied digitally to account for an overestimation of the flux contributions of low-frequency eddies. Best results were achieved by applying the FIR filter using a Hamming window, whereby time constants of respectively 50 and 100s for CH_4 and N_2O sufficiently filtered out unwanted flux contributions at frequencies < 0.05 Hz (Fig. 1b). Missing low-frequencies were then back-corrected based on the site-specific reference model co-spectrum (Wohlfahrt et al., 2005b). Exchange rates of CH_4 and N_2O calculated with these settings constitute our final, best guess fluxes that were used for all analyses in this manuscript."

The effects of FIR filtering are already described in section in section *2.5 Flux calculations* and shown in Figure 1b and Figure 6.

ad 3) Coverage of half-hours and days is given in section *2 Methods* (see also point 1)).

ad 4) As suggested by Reviewer #1, we expanded section *2 Methods* our methods added an additional paragraph dealing with the gap-filling procedure in section *2.5 Flux calculations* (please see Comment #8). We think that a more detailed discussion of the gap-filling procedure 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).

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: The main focus of this paper is the CH_4 and N_2O exchange over a managed hay meadow. We feel that discussing the technical aspects of FIR filtering in even more detail would not only make this long manuscript even longer, but also distract attention away from our objectives and hypotheses, the core elements of this publication. However, we decided to clearly show the filtering effect in the very first figure of the manuscript (Figure 1b) and in section *2 Methods*.

Following the suggestions of Reviewer #1, we expanded section *2.5 Flux calculations* to make the application of the FIR filtering more comprehensible (please see Reply #9). The impact of the different FIR filters on the total balance is already described in the text 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 have included this recommendation in section *5 Conclusion*:

> "In addition, we recommend to carefully check flux results and underlying cospectra for an overestimation in the low spectral range and correct for this effect if necessary."

Comment #11: "For emission numbers in the tekst (CH_4 and N_2O) 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 CH_4 and N_2O 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: Following the suggestions of Reviewer #1, we added STDEVs in section *3 Results* when discussing the daily average cycles shown in Figure 4. In addition, an indicator for uncertainty is already shown in Figure 4.

Final annual numbers for CH_4 and N_2O emissions are only feasible for 2011 and are already in the manuscript.

Regarding uncertainty bands in Figure 6: 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 and shown in Figure 6.

As suggested, we included STDEV for the group means in Table 2.

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 (CO_2, CH_4 and N_2O) over managed temperate mountain grasslands 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. A short description 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). In our discussion we focused on the key findings of other studies that we felt were the most relevant to our results. We feel that including an additional table would considerably increase the length of this manuscript while only providing redundant information.

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 CH_4 and N_2O 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 CO_2, CH_4 and N_2O, but as pointed out by Reviewer #1 we missed out on the publication by Kroon et al. (2010). We added the publication to our references and also used it in our discussion.

Comment #14: "Units should be consistant troughout the manuscript. Since the focus is on finding ecosystem-based parameters that explain the CH_4 and N_2O fluxes and determining the GWP of the Neustift site, the suggestion is to express everyting in (m)g CH_4/N_2O m^{-1} yr^{-1} and CO_2-eq m^{-2} yr^{-1}. Not in terms of carbon (CO_2-C or CH_4-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 agree with Reviewer #1 and changed all mole units in the manuscript text and in Figures 1, 2, 3, 4 and 5 to mass units. Half-hourly flux values for CH_4 and N_2O are now given in ng m^{-2} s^{-1}, annual budgets in g m^{-2} yr^{-1} for CH_4 and in mg m^{-2} yr^{-1} for N_2O.

CO_2-equivalents are only used when the GWP is calculated and compared to the GWP of sites in other studies. The carbon and nitrogen-only units CH_4-C and N_2O-N were abandoned and converted to CO_2-equivalents.

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 N_2O emission between 2010 and 2011. What is the reason?"

Reply #16: Unfortunately Reviewer #1 does not explain what number he / she is refering 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 N_2O fluxes were characterized by larger variability (Fig. 3, Fig. 4). Cumulative numbers are only given for 2011 (Fig. 6).

Comment #17: "The CO_2 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 CH_4 and N_2O exchange with CO_2 fluxes in terms of CO_2 equivalents, the CO_2 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: Please see also our reply to comment #15. We outlined further research to better address this issue in the discussion. The following paragraph was added in the discussion section *4.1 Methane*:

"Unfortunately we lack detailed high-resolution spatial data (e.g. vegetation, soil) about small areas and patches within the sampled flux footprint at the study site that is required for a meaningful footprint analysis. Therefore, we are currently not able to discuss potential emission hotspots, their impact on calculated CH_4 balances and the problem of possibly preferential sampling within the scope of this manuscript. Hot spot footprint analysis merits its own research and would provide important insights in how to interpret eddy covariance flux data."

We now also emphasize this point in section *5 Conclusion*:

"Knowledge about emission and deposition hotspots within the footprint area would allow for a more comprehensive interpretation of the bulk EC flux. Additional information about GHG producing and consuming patches within the flux footprint could be achieved for example via chamber measurements, another possibility would be to perform a detailed statistical analysis of EC fluxes and underlying footprint information in combination with detailed spatial data of the sampled area."

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: Unfortunately we have to say that we lack detailed enough information about the amount of applied nitrogen during the fertilization of the meadow. We have a very rough estimate of 341 kg N ha^{-1} yr^{-1} that is applied during fertilization, but this number is highly uncertain. Using the amount of nitrogen emitted in 2011 in the form of N_2O we come up with an emission factor of 0.1 %. Because of the uncertainties involved we decided to not include this number in the manuscript.

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: We reworded the sentence to make our statement unambiguous, it now reads:

"Although the GWP calculated from CO_2 and CH_4 fluxes was much lower in Neustift (-64 g CO_2-equ. m^{-2}), the situation […]"

Comment #22: "Also line 6-7 on pshr 8205: is similar to -32 g CO_2-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. We reworded the paragraph in section *2.5 Flux calculations* to make this clear:

> "The high pass, non-recursive, finite impulse response (FIR) filter was applied digitally to account for an overestimation of the flux contributions of low-frequency eddies. Best results were achieved by applying the FIR filter using a Hamming window, whereby time constants of respectively 50 and 100s for CH_4 and N_2O sufficiently filtered out unwanted flux contributions at frequencies < 0.05 Hz (Fig. 1b). Missing low-frequencies were then back-corrected based on the site-specific reference model co-spectrum (Wohlfahrt et al., 2005b). Exchange rates of CH_4 and N_2O calculated with these settings constitute our final, best guess fluxes that were used for all analyses in this manuscript."

Comment #23: "The -19.2 g CO_2-C mentioned in line 20 page 8205, is that calculated from the -32 g CO_2-eq (line 6, page 8205)? Then the calculation is not right. Please be consistent in units, and explaine where numbers come from."

Reply #23: Following the suggestion of Reviewer #1 in Comment #27, we converted all carbon, nitrogen and mole based units to mass units (please see Reply #27). This makes number comparisons more comprehensible and replicable.

In the discussion paper of this manuscript the questioned -19.2 g CO_2-C m^{-2} yr^{-1} were correctly calculated from -70.5 g CO_2 m^{-2} yr^{-1} for 2011.

Comment #24: "Suggestions for Paragraph 4.3: give clear numbers for the total balance, including standard deviations (CH_4, N_2O and CO_2), perhaps in a table or figure"

Reply #24: We followed the suggestion of Reviewer #1 and rephrased the first paragraph in section *4.3 Global warming potential*:

> "The availability of year-round data allows for the calculation of a yearly GWP balance over a specific ecosystem. In this study, year-round CH_4, N_2O and CO_2 flux data were available for 2011. When expressing the net exchange of the three compounds in terms of CO_2-equivalents and adding up these different contributions, the resulting GWP of the meadow in Neustift was -32 g CO_2-equ. m^{-2} yr^{-1} in 2011, whereby a yearly NEE of -71 g CO_2 m^{-2} yr^{-1} was offset by CH_4 and N_2O emissions of 7 and 32 g CO_2-equ. m^{-2} yr^{-1}, an offset of approx. 55%."

Regarding the standard deviations: please see Reply #11.

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

Reply #25: We added to section *2.5 Flux calculations*:

"For the calculation of the annual GWP of the meadow in Neustift, CH_4 and N_2O fluxes were converted to CO_2-equivalents using the respective compound warming potential as given by Forster et al. (2007)."

Comment #26: "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)."

Reply #26: Following the earlier suggestion by Reviewer #1, we unified units throughout the manuscript to make number comparisons more comprehensible.

Comment #27: "Express the numbers in CO_2-equivalents, not in carbon."

Reply #27: We followed the suggestion by Reviewer #1 and now give the respective numbers in terms of CO_2-equivalents. The carbon and nitrogen-only units CH_4-C and N_2O-N were abandoned and converted to CO_2-equivalents.

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

Reply #28: We followed the suggestion of Reviewer #1 and revised our conclusions. In addition, we added a new paragraph in the middle part to make the conclusions in line with the objectives.

"[…] Our analyses showed that daily average N_2O exchange during most of the vegetation period can be well explained with simultaneously recorded ancillary data, especially in the time period after the 1st cut in June up until snow cover towards the end of the year. In contrast, modeling daily average exchange with the same ancillary data worked considerably worse for CH_4, a finding that suggests the possibility of a more heterogeneous footprint in regard to methane sources and sinks. For both compounds it was not possible to single out one driving variable as the most important, which is to be expected due to the nature of the eddy covariance flux signal. […]"

Reply to Anonymous Reviewer #2

Comment: "This is an interesting study of the actual greenhouse potential from CO2, CH4, and N2O from an European Hay meadow. The authors used the eddy covariance method to measure fluxes of these three gases and calculated their individual warming contributions. An interesting result is that N2O seems to be as important as CH4 in the overall scheme. This may be in opposition to American studies which have found that the highly episodic character of N2O emissions tended to minimize their overall impact.

While I would normally recommend publication of this work, the fact that the authors used multiple linear regression analysis to explain their flux numbers makes this impossible. It is well known that CO2 and CH4 both show distinctly non-linear temperature dependencies. In addition, several of the authors conclusions are supported by mean values with uncertainties that are sometimes five or six times the mean value. This is very risky! Because of these issues, I recommend that the authors re-analyze their data and re-write this manuscript."

Reply: As suggested by Reviewer #2, we re-analyzed our data and re-wrote the manuscript. For this we followed the suggestion of Reviewer #1 and used LN transformed concentration values for CH_4 and N_2O in the multiple linear regression to account for non-linear dependencies.

Unfortunately Reviewer #2 does not detail which number he / she refers to when addressing the topic of uncertainty. We would like to point out that eddy covariance measurements over a site that is possibly characterized by CH_4 and / or N_2O hot spot emissions will result in fluxes with a high degree of uncertainty. This uncertainty is further enhanced by generally low fluxes and by CH_4 / N_2O exchange alternating between emission and deposition. One of the objectives of this paper is to describe the grassland ecosystem in respect to GHG fluxes, and according to our findings this high degree of uncertainty is one of the characteristics at the investigated measurement site (and probably other similar sites). At an annual time scale, we address systematic uncertainty by giving numbers for different FIR filtering approaches.

In addition, we added numbers for STDEVs where possible throughout the manuscript, e.g. for average values in Table 2. We also emphasize the point that EC fluxes are bulk signals and that analysis has to be performed carefully.