<u>Reviewer 2</u>

Author response to BGD manuscript:

"Temporal and spatial variations of CO2, CH4 and N2O fluxes at three differently managed grasslands" (bg-2013-46)

We thank the reviewer 2 of this manuscript for their thoughtful and critical ideas towards improving this manuscript. Original comments are in regular font, and our responses in bold italics. Overall, we streamlined the original manuscript towards a better readability, leading towards the conclusions

To understand how GHG emissions respond to environmental and management forcings, the authors of the present paper have quantified temporal and spatial variations of manual chamber based CO2, CH4 and N2O soil fluxes of three Swiss grasslands differing in altitude and management. The manuscript is clear with regard to objectives and results presented. However, there is some methodological problems which might affect results, discussions and conclusions.

General comments:
The manuscript presents soil emissions and it is not clear from the introduction if the overall GWP of the references (Soussanna et al., 2007 and Schulze et al., 2009) include other emissions, e.g. CH4 from livestock. Please specifiy.

<u>Response:</u>
In this present manuscript, we present soil fluxes of CO2., CH4, and N2O only. The results presented in Soussanna et al. (2007) and Schulze et al. (2009) were not focusing on soil fluxes, but they included lateral and vertical fluxes (i.e. fertilizer, harvest related export, fluxes from animals, etc.). Further information was added in the introduction of the revised version of the manuscript:"While Vleeshouwers and Verhagen (2002) and Janssens et al. (2003) reported that the GWP of European grasslands is still highly uncertain, Soussana et al. (2007) and Schulze et al. (2009) estimated that European grasslands had negative GWPs (including vertical and lateral fluxes of the three GHGs; e.g. fertilzer input, harvested biomass, animal emissions)". (p3/l13).

Material and Methods section need to be extended by number of cattle/ sheep, duration of grazing, fertilization (organic or inorganic, how much nitrogen applied) and how chamber measurements represent potential hot-spots of urine and/ or excrement patches. Did you measure in higher time intervals after fertilization/ during grazing etc? Furthermore for N2O fluxes nitrogen fixing species such as clover is important which is common in grasslands. Please give any information on this rather than referring only to Zeeman et al., 2010.

<u>Response:</u>
We extended the information on species composition and number in the materials and methods section. Furthermore, we provide a table (Tab. 1) including management date, type and the amount of N addition.
With our experimental setup, we covered the major management activities on

each of the grasslands. This included regular harvests followed by the application of organic fertilizer, e.g. slurry and manure. Grazing as a fourth management occurred for few weeks in spring and fall at the intermediate site and roughly 2-4 weeks at the high altitude grassland. Our setup of static chamber was not designed to account for grazing which resulted in fencing of collars to avoid a destruction of the collars. Our setup was chosen as a tradeoff between either excluding grazing by fencing the chambers or continuous disturbance of the soils inside the chambers due to removal of the chambers before grazing and their re-insertion into the soil after grazing. Therefore we chose to keep the chambers at a precise location which excluded grazing. This resulted in an underestimation of the GHG fluxes from urine and dung patches. However this underestimation, particularly of CH4 and N2O emission was assumed less than a possible overestimation, mainly CO2, by regular disturbance caused be the insertion of chambers into the soil at each sampling event.

We referred to Zeeman at al. 2010 to avoid redundancies between the studies. In the revised version of the manuscript, species composition is now explained in detail. For CHA (p5/l6), for FRU (p5/l16ff), and for AWS (p5/l28ff).

Even though frost-thaw emissions can substantially contribute to annual N2O emissions in grasslands in higher altitude, nothing is said about their importance for your sites. Furthermore, I suggest including soil physical and chemical properties.

<u>Response:</u>
Concerning frost-thaw emissions we were only able to access the lowland and intermediate in altitude grassland during winter since the alpine site can not be reached due to considerable avalanche danger in winter. We did not observe flushes of either GHG at the end of the winter season at Fruebuel. Furthermore we would like to point to a recent study by Merbold et al. (2013) who studied winter emissions of all GHGs at an sub-alpine grassland site, 1600m a.s.l.. The authors did not observed such peaks during freeze thaw cycles. Information on soil type and physical properties were added in the MatMet section in the revised manuscript. For CHA (p5/l4f), for FRU (p5/l15f), and for AWS (p5/l26f).

The geostatistic is only based on 16 chambers mostly placed in equidistance at linear transects rather than grids in varying mesh sizes. What was the rational for choosing linear transects for spatial variation? Beside the relatively low number of chamber position I question if this design allows you to make sound geostatistics. The minimum distance of 5-7m could be to low for spatial variation in soil GHG emissions which can vary on dm scale, in particular in grazed systems with urine and excrement patches.

<u>Response:</u>
Based on your comments and the ones from the other reviewer, as well as re-consideration of the focus of this manuscript, we removed the application of complex geostatistical approaches. The spacing of 5-7m may be too large to represent hot spots occurring from grazing, however since our primary focus was on estimating the GHG exchange from the major management activities – harvest and fertilization – we argue that a spacing of several meters between

each chamber is favorable in order to avoid autocorrelation between chamber fluxes but also representing possible topographic differences within the respective study site.

The PCA and derived variables driving GHG emissions do not consider grazing/ fertilization i.e. nitrogen input. However, for N2O emissions N availability might be the main driver for magnitude of emissions. This might also the reason why your model was less predictive than for CH4 and CO2. Even though you did not include fertilization in the statistical evaluation larger sections of the discussion deal with it. Could you include e.g. days after fertilization into your regressions?

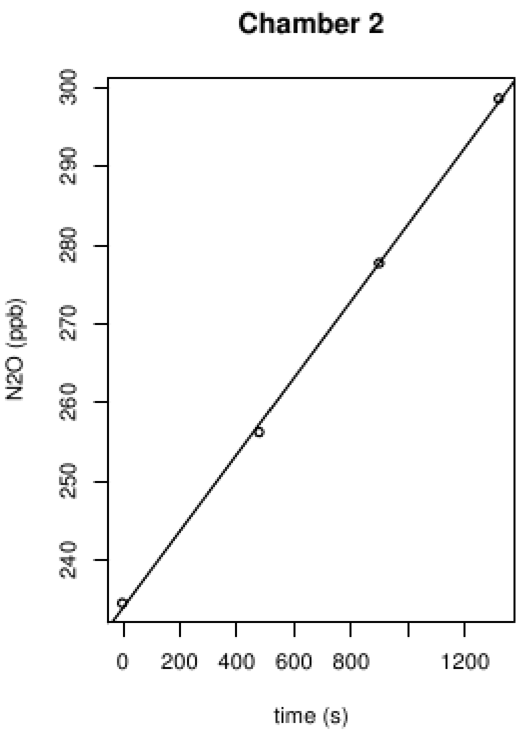
Response:
Nitrogen inputs were excluded from the PCA to avoid substantial losses of flux data. This is due to the fact, that flux data could have only been regressed with N inputs on dates after fertilization, thus only at six dates at CHA and three at FRU. In order to avoid the bias occurring via excluding N inputs we used LAI as a proxy. LAI changes commonly occurred after harvest followed by the application of fertilizer (not shown). Therefore we included LAI in the PCA and account to a large degree for changes in N input while avoiding the loss of flux data within the analysis. Furthermore, we now provide emissions factors (N in slurry/manure and subsequent N2O-N emissions) for the intensively and moderately managed grasslands CHA and FRU in the revised version of the manuscript, (p9/l25f) and (p14/l22ff).

The motivation of the intensive field campaign for identifying diurnal patterns of CH4 and N2O fluxes in September is unclear. Your findings are only valid for the situation of exactly these 48 hours in September 2010 and do not allow a generalization since the pattern will may vary across seasons and e.g. for N2O with nutrient availability after fertilization. Thus, the use of it is rather limited. Since the data is included in the overall flux measurements I would suggest shorten this part in particular the section 3.2.1. and also 4.2

Response:
We agree with reviewer 2, therefore we kept this section as short as possible, providing only the most important results. Still, this short-term observation was meant as a snap-shot of GHG flux patterns towards the end of the season. Caused by limited resources we were unable to carry out intensive campaigns more regularly. The information derived from the September campaign remains valuable, particularly when focusing on the diurnal variation of GHGs during this period of the year. Further we avoided to draw conclusions from the results obtained during this campaign for the whole year and shortened the results section on this topic considerably.

The method of flux calculations is not completely sound. I question assumed linearity, at least no detailed criteria for potential evaluation of non-saturation is presented, and filtering of out of range values and values not different from zero which may leads to bias of calculation of mean site fluxes. What can we learn from comparison of soil chamber measurements with Eddy data at your sites. At least you mention agreement with soil CO2 fluxes (see also specific comment)

Response:
We thoroughly revisited our flux calculations. Yes, we quantified the curvature using the R2 of the linear regression between concentration changes over time. In addition, we computed fluxes using the intercept method according to Kroon et al. (2008). This methodology is also aiming to avoid underestimation of fluxes due to non-linearity over time. We however found, that resulting flux estimates of N2O and CO2 (both, were expected to show increasing concentrations with time) were not much different from the flux estimates obtained by the simple linear regression approach we used. For N2O, computed fluxes (intercept method) were on average 7.9% smaller as compared to fluxes calculated with linear regression. For CO2, respiration would have been 11.3% larger.



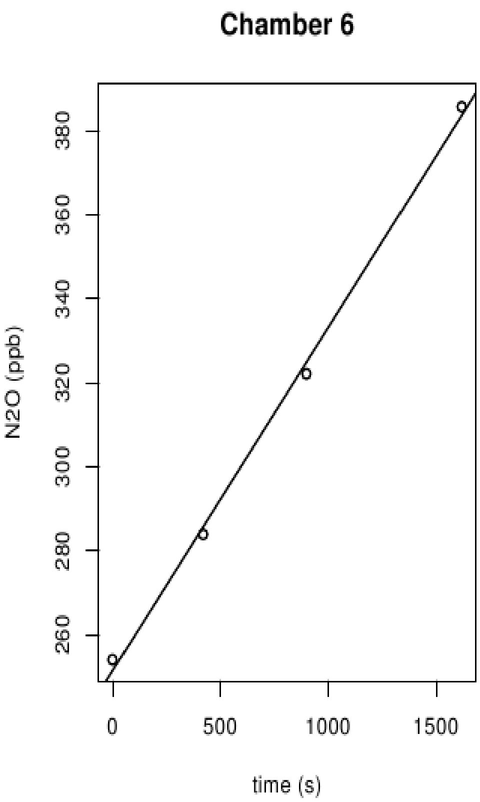
Furthermore we visually inspected the slope between the concentration measurements between time steps 3 and 4, especially for N2O after fertilization events at CHA (e.g. Fig. to the left). We found that for N2O, the slope was in ca. 50% of the cases the same or even steeper as the slope between the previous concentration measurements (time steps 2 and 3 and time steps 2 and 1). In addition, we computed fluxes using the intercept method according to Kroon et al. (2008). This methodology is also aiming to avoid underestimation of fluxes due to non-linearity over time. We however found, that resulting flux estimates were not much different from the flux estimates obtained by the simple linear regression approach we used. Therefore, we chose to rely on fluxes that were calculated by the linear regression approach only.

p2642, l1-2: "...fluxes were only computed if the linear regression yielded a r2 >0.8." Do you mean by this, that GHG fluxes (for the three gases CO2, N2O and CH4) were only computed when the Rˆ2 for CO2 was above 0.8, with CO2 being taken as a quality criterion for the whole chamber operation (based on the assumption that there must always be CO2 mineralisation and thus evolution from the soil, and that any noisy CO2 temporal profile indicates a dysfunction of the sampling system, for example a large leak)? Or do you actually mean that for each gas taken separately you applied a selection based on the Rˆ2 of the gas in question? In the case of the latter, consider the hypothesis that the N2O concentration is almost exactly 320 ppb (+/- the uncertainty in the GC concentration measurement) at sampling times t0, t10min, t20min, t30min. The Rˆ2 is very close to 0, and yet the flat concentration profile tells us that net emission or uptake takes place. Discarding all such flux events would inevitably bias the temporal or spatial average upwards (overestimation of the annual-scale and field-scale emissions).

Please comment.

<u>Response:</u>

We applied the R2 threshold per GHG, and not only for CO2. And in the case of CO2, our concentration increases were in 92% of the cases linear with R2>0.96. We would like to thank reviewer 1 for the additional comment concerning omitting flux values due to low r2 values of the linear fit. In general we agree with the fact of counteracting processes. However for N2O these counteracting processes are very difficult to disentangle and to our knowledge few studies have reported considerable N2O uptake rates. Furthermore N2O uptake and the involved processes are still unclear.



In our study N2O concentration changes (t0-t30min) were greater than 45 ppb in >86% of the cases. Especially after fertilization, we observed very large concentration changes over time (e.g.>120 ppb and still not saturated as the increase between t3 and t4 was larger than between t2 and t3; see Fig. to the left). We however added the following information to the revised manuscript: "If the slope between the first and second concentration obviously deviated from the one of the remaining three concentration measurements, we omitted it and calculated the flux from the remaining three."

In addition we included a comparison between chamber and EC derived soil fluxes of CO2 in the discussion paragraph of the revised manuscript (p16/l7).

Specific comments:
Introduction:
P2637; Ln20: is this only soil emission, or does it include other emissions like CH4 from livestock?

<u>Response:</u>

We improved the wording in the revised manuscript to "soil emissions" in order to avoid confusion.

P2637; Ln22ff: The altitudinal variation is rather a climatic variation. Therefore, I would rather write . . .do not consider grasslands in higher elevation "with cooler and wetter climatic conditions which may lead to higher soil CH4 and N2O emissions”.

Response:
Done.

P2638; Ln4: change into: which are characteristic for CH4 and N2O fluxes between climatic or management driven pulse events.

Response:
Done.

P2638; Ln22: add “soil” CO2, CH4 and N2O fluxes

Response:
Done.

Material and Methods:
P2639; Ln9-10: It says (CHA) winter location for sheep and cattle. Was urine patches and feces included in the chamber measurements? Can you provide number of animals and time of grazing.

Response:
In the period 2010 and 2011, CHA was not used for sheep and cattle grazing. To avoid potential misunderstanding, changed this to:” In 2010 and 2011, the pastures were used for forage production”. (p5/l7)

P2639; Ln18ff: Give more details on animal numbers also
for FRU. At this site also fertilization is relevant. Again what type mineral or organic, how much nitrogen is added?

Response:
We included a table giving the dates and type of management activity per site and further added N content of the respective fertilizer in the revised mss.
As stated above, our experimental setup included the major management types but excluded grazing to avoid damage and displacement of the chambers as well as possible disturbance of the soil continuum leading most likely to unrepresentative GHG flux estimates.

P2639; Ln24ff: AWS animal numbers, time of grazing? Manure, how much nitrogen?

Response:

P2640; Ln7: chamber height in average was 0.136m. How did you manage to include the plants if they were growing higher? Did you correct the headspace volume; otherwise you may substantially overestimate fluxes.

Response:
During periods of higher plant growth we used extensions of the chambers (45

cm in height) and adjusted the head space volume accordingly.
During most the sampling events (>90%) including vegetation height <20cm, after harvest events and during winter – we observed limited snow cover at the lowland site and avoided measurements during larger snow height at Früebüel (>15cm) – the head space volume was suitable for our aims.

P2640; Ln10ff: I see a problem in the different distances of chamber positions and I doubt that the minimum distance of 5-7m is applicable for spatial variation in soil GHG emissions which can vary on dm scale, in particular in grazed systems with urine and excrement patches.

Response:
The spacing of 5-7m may be too large to represent hot spots occurring from grazing, however since out primary focus was on estimating the GHG exchange from the major management activities – harvest and fertilization – we argue that a spacing of several meters between each chamber is favorable in order to avoid autocorrelation between chamber fluxes but also representing possible topographic differences within the respective study site.
Furthermore we decided to install the 16 chambers along transects within the (1) the footprint of the EC towers (which was less important for this study, but of large need for another research question, (2) to represent all aspects of the terrain/slope, and (3) to allow for the common farming practices. Thus, we think representing both farming practices as well as site conditions with 16 chambers at each site gives reasonable estimates of the GHG fluxes at these sites.

P2640; Ln16ff: This sentence is not needed.

Response:
We deleted this sentence.

P2640; Ln18ff: Did you increase measurement frequency after fertilization or grazing. The growing season per se does not say anything about event driven emissions due to management. What about frost-thaw emissions. What about contribution of frost-thaw emissions which can be substantial in your systems?

Response:
We agree with reviewer 2 on the importance of more frequent sampling after management events. However when applying a total of 48 static chambers at three research sites which are located in large distances from each other, such intensive sampling had to be omitted caused by resource limitation.

We added a recommendation in the outlook of the revised manuscript to overcome the limitation of low sampling frequencies in the future:"For soil fluxes of N2O, we suggest the use of portable chambers in conjunction with recently developed laser spectrometers allowing for much shorter sampling times and therefore sampling of additional hot spots as occurring during grazing and hot moments after fertilization." (p18/l21).

Frost-thaw events and associated GHG pulses have been reported to play a

major role in a variety of ecosystems. We did not observe such pulses either at FRU nor CHA, while the third research site – located at 2000m a.s.l. was inaccessible during winter. Furthermore we would like to highlight a recent study be Merbold et al. (2013, this Special Issue) who investigated winter GHG exchange at a sub-alpine grassland located at 1600m a.s.l. in Switzerland whom did not observe such flushes for either GHG.

P2640; Ln22ff: What was the motivation to measure diel patterns of CH_4 and N_2O emissions in September 2010? Was this date chosen because of management issues e.g. grazing or manuring? Please indicate also at what sites you did this intensive field campaign. Merge this section with P2642; Ln 13ff.

<u>Response:</u>
The intensive field campaign was carried out at all three grasslands simultaneously. The motivation was to assess short-term flux variations in comparison to those at the annual scale. September was chosen for various reasons: (1) to derive the diurnal variation of the background fluxes of CO_2, CH_4 and N_2O which were not affected by management – even though this could not be achieved for the alpine site, (2) to include possible pattern of these background fluxes into modeling approaches at a later stage and (3) to optimize with the available resource, e.g. labor and lab capacities. We merged the two sections according to the suggestion.

P2641; Ln6ff: This statement is very general. The linearity does not depend only on the closure time but also on the magnitude of flux. After fertilization you have substantial N_2O fluxes (up to 15 nmol N_2O m-2 sec-1,) and I doubt that your statement is still valid. Did you test also non-linear calculations for periods of high fluxes? There might be a problem also with the linearity of your ECD detector at high concentrations at high flux conditions. Did you calibrate your ECD for high concentrations because potentially without calibration you may underestimate concentrations?

<u>Response:</u>
We agree, that our statement sounded like a general rule. Moreover, we fully agree that the deployment time should be adjusted to the magni-tude of the expected flux and the head space volume. In this study we never observed any saturation effects, neither after fertilizer applications at the intensively managed site Chamau nor for wet conditions at the mid-altitudinal site Früebüel. To avoid confusion wewe changed the sentence to: "This closing time was sufficiently short to avoid saturation effects inside the chamber head spaces." (p7/l8)

P2641; Ln15: does this mean you converted CO_2 into CH_4 with a methanizer? Provide details.

<u>Response:</u>
We added the information about the mathanizer. (p7/l15)

P2640; Ln20ff: What were the criteria for non-occurrence for saturation?

<u>Response:</u>

We quantified the curvature, and thus possible saturation, using the R2 of the linear regression between concentration changes with time.

P2642; Ln4ff: To my opinion out of range values cannot be filtered by using +/- 10 SD. For these measurements you need to check the 4 measurements representing the increase of concentration over time. If the increase follows a plausible pattern you cannot discard fluxes, which can be real due to hot spots (urine/feces patches) or hot moments e.g. frost-thaw. Another issue is that at times when you have fluxes not differing from zero (at the detection limit)
you might have r2 <0.8. But if you delete these values your mean would be biased towards overestimation of fluxes. Same for CO2 fluxes. I think it is too easy to delete values below 0. Again, you need to go back to the single concentration measurements.

<u>Response:</u>
We agree with the reveiwer and adjusted the calculations: We omitted the SD filter, as we might have had to discard measurements which were yet realistic particularly for N2O emissions being known to vary largely in space but also in time, e.g. after management.Therefore all valid individual chamber fluxes were used in the analysis (valid = the linear regression for the concentration changes over time >0.8). However, there is no difference in presented flux data notable as very few data points were excluded via the SD filter originally. Thus, mean fluxes of the respective GHGs only changed at the second decimal digit (after the comma).

In our study N2O concentration changes (t0-t30min) were greater than ppb in 90% of the cases. Especially after fertilization, we observed very large concentration changes over time (e.g.>120 ppb and still not saturated as the increase between t3 and t4 was larger than between t2 and t3; see Fig. to the left). We however added the following information to the revised manuscript: "If the slope between the first and second concentration obviously deviated from the one of the remaining three concentration measurements, we omitted it and calculated the flux from the remaining three."

Results:
P2644; Ln7ff: There is a trend of CH4 emissions at wintertime, might be correlated with high water contents, which may also explain sporadic CH4 emission events in other seasons.

<u>Response:</u>
We changed it to "CH4 emissions were mostly observed during winters, whereas uptake rates were prevailing in summers." (p9/l18)

P2644 Ln15ff: How representative is the mean flux for your system under grazing.

<u>Response:</u>
The mean fluxes at AWS were only representative for the system excluding the influence of N inputs in the course of grazing, which was primarily caused by the experimental setup and the associated fencing of the static chambers. Yet, we removed the standing biomass at times of grazing, to account for the biomass removal at AWS. To avoid confusion we adjusted the statement on

grazing to:"At AWS, peak emissions were in correspondence with manure application and biomass removal, which was done manually during the period of grazing." (p10/l1)

P2645 Ln11ff: I am not sure but it seems you have used CO2, CH4 and N2O fluxes altogether in the PCA. Why didn t you do the PCA for any of the fluxes.
E.g. you did also not include N fertilization/ grazing which is probably the main driver for N2O emissions.

Response:
The PCA we performed included the individual biotic and abiotic drivers only, but not the GHGs. The purpose of the PCA was to identify collinear relationships between potential drivers, without the need of the flux data Also, the PCA does not provide any information on the strength of correlations between drivers and fluxes, which was performed within the multiple-linear regression models.
As stated before, nitrogen inputs were excluded from the PCA to avoid substantial losses of flux data. This is due to the fact, that flux data could have only been regressed with N inputs on dates after fertilization, thus only at six dates at CHA and three at FRU (as mentioned before). In order to avoid the bias occurring via excluding N inputs we used LAI as a proxy. LAI changes commonly occurred after harvest followed by the application of fertilizer (not shown). Therefore we included LAI in the PCA and account to a large degree for changes in N input while avoiding the loss of flux data within the analysis. We included the following to the revised manuscript:"Nitrogen inputs in the form of slurry/manure applications were not considered for the PCA, as only six and three data points would have been available at CHA and FRU, respectively, and LAI already can be seen as a proxy for management activity." (p10/l24ff)

P2645 Ln25ff: Are you sure that capillary rise and hydraulic redistribution in the vascular plant root system have the capacity to be SWC replenished?

Response:
We revisited our data and came to the conclusion to delete this statement, as diurnal variations in SWC were simply too small, as such an assumption would qualify. What we however saw, was that PAR and SWC were negatively correlated. Thus, we changed the manuscript to:"In contrast, under fair weather conditions, the typical diurnal cycle of PAR was likely linked to a similar cycle of SWC in the opposite direction." (p11/l5)

P2646 Ln 6ff: The PCA and derived variables driving GHG emissions do not consider grazing/ fertilization i.e. nitrogen input. However, for N2O emissions N availability might be the main driver for magnitude of emissions. This might also the reason why your model was less predictive than for CH4 and CO2.

Response:
We agree, that nitrogen input is definitely important for N2O emissions at managed grasslands. Therefore we now provide N2O-N emission factors in this manuscript. In order to avoid the bias occurring via excluding N inputs we used LAI as a proxy. LAI changes commonly occurred after harvest followed by the application of fertilizer (not shown). Therefore we included LAI in the PCA and

account to a large degree for changes in N input while avoiding the loss of flux data within the analysis.

P2647 Ln 9ff: The motivation of this experiment is not fully clear, since your findings are only valid for the situation of exactly these 48 hours. Thus, the use of it is rather limited, you can mainly say that you found or not found a diurnal pattern. We know that the occurrence and magnitude of diurnal patterns can vary depending on soil environmental conditions over seasons even days. Thus, repeating this experiment let’s say for typical seasonal conditions, spring, summer, winter, autumn, or after fertilization would have increased the usefulness of the data.

<u>Response:</u>
This short-term observation was however meant as a snap-shot of GHG flux patterns. Additional campaigns would have been very valuable but had to be rejected due to resource limitations. Therefore we still stress the importance of such campaigns and strongly encourage future research to use less labour intensive techniques in order to achieve higher sampling frequencies.

Discussion:
P2650 Ln4ff: I question if the sampling design (transect with 12-16 chambers; 5-7m minimum sampling distances allows for sound application of geostatistics (see general comment)

<u>Response:</u>
Both reviewers stated this comment and we decided to remove the complex geostatistical approach from the revised version of the manuscript.

P2652 Ln23ff: Can you provide some details, what is agree? Section 4.3 and Fig.9 rather focus on CH_4 and do not provide new information except that inclination was correlated with soil moisture. Not sure if this section is needed. If than provide more details of N_2O and CO_2 and why they differ among each other.

<u>Response:</u>
We included a plot, showing the agreement between EC and chamber based respiration of CO_2, which we briefly describe in the discussion section of the manuscript.

Conclusions:
P2655 Ln7ff: Inclination is just one part, but there could be also topographic depressions which are wetter than elevated parts. Further it depends where you are at the slope. You can have water and nutrient flows along topographic gradients and reflow and accumulation down slope with impacts on GHG fluxes. Thus, I found your conclusion is too general. More important at least for N_2O urine patches and spatial spreading of excrements will have a higher impact on magnitude of emissions than inclination.

<u>Response:</u>
We extend this part of the conclusions to:
"Thus, on sloping terrain, mean chamber fluxes of CH_4 should be estimated from an ensemble that is (a) sufficient in size, (b) represent the common

species composition including hot-spots occurring due to grazing, and (c) the terrain of the site. This is important since SWC is one of the major environmental drivers of CH4 exchange." (p18/l15)

Since our setup covered cut and fertilized grasslands only, we avoided to discuss urine patches and excrement and their effects on fluxes.