We would like to thank all five reviewers for their detailed, constructive and positive feedback on our original manuscript "Winter GHG emissions in a sub-alpine grassland". We believe the comments improved the manuscript considerably. Here, we respond to all general and specific comments of each reviewer separately (regular font is the reviewer's comments, italic font represents our answer).

Reviewer 2:

The manuscript presents winter data of CO_2, CH_4, and N_2O fluxes for a subalpine grassland in Switzerland. CH_4 and N_2O fluxes were computed using concentration gradients measured weekly within the snow pack. For CO_2 flux, the results of two successful flux measurement methods (weekly gradient sampling and continuous eddy covariance) are compared while the reasons for failure of a third method (automatic gradient sampling of CO_2 and ^{222}Rn diffusion tracer) are discussed. Temporal correlations between fluxes and environmental parameters are presented and seasonal GHG budgets computed. In addition, separate sampling of spatial variation in fluxes within the grassland and other nearby ecosystem types is presented. The study found no temporal correlations between N_2O fluxes and environmental variables, but soil temperature and snow water equivalent (SWE) were found to be the major drivers of gradient CO_2 and CH_4 fluxes. No environmental drivers of CO_2 flux from the EC method were found. The gradient method underestimated CO_2 fluxes by about 40% compared to eddy covariance measurements, but seasonal estimates were similar when a model was used to extrapolate the gradient measurements throughout the season.

Given the paucity of high-latitude winter flux studies, including a further lack of non-CO_2 GHG budgets, the data and methods comparison presented in the paper are of great interest to the scientific community. The study has a lot of interesting elements to it that form a nice dataset and relatively comprehensive picture of the winter GHG fluxes of the grassland, including context of CO_2 fluxes with the surrounding ecosystems. Before publication, however, I think the paper needs moderate revision in several areas. The largest revision concerns the model used to extrapolate gradient CO_2 fluxes throughout the winter, as I think it yields unrealistic values at the beginning and end of the season which subsequently skew the season-wide estimate. Additionally, SWE was found to be a major driver of CO_2 and CH_4 fluxes (and used in the seasonal models), but a mechanism of control is either dismissed as a covariate with other factors or not discussed. Therefore, better justification for including SWE in the seasonal model is needed. Discussion of some other important results is incomplete, and there are inconsistencies and redundancies in the Discussion to be addressed. Finally, there are instances throughout the manuscript that need clarification, and the manuscript would benefit from careful editing. Specific comments and suggestions for improvement on these topics and other minor areas or typos are given in the sections below.

Specific Comments

1. Page 402: Abstract: The abstract needs mention of the motivation for the study. It also lacks the significance of the results, other than to say that it is unclear how winter GHG emissions will be altered by climate change, which in my opinion doesn't do justice to the findings for winter CO_2 and CH_4 flux since soil temperature was a significant driver of both.

We included the following sentence in the abstract of the revised manuscript."Our objectives were (1) to observe the temporal and spatial variation of GHG during the winter 2010/2011 and further (2) to estimate the GHG budget of the site during this specific season. We further replaced the sentence containing "...unclear how winter emissions will be altered by climate change" by "While this study revealed the major drivers of CO_2 *and* CH_4 *fluxes in this grassland ecosystem during winter, the drivers of* N_2O *could not be identified and need further investigation (1) to deepen our currently limited knowledge, (2) to thoroughly constrain annual balances and (3) to project possible changes in flux magnitude with expected shorter and warmer winter periods. .*
As a general comment, each paragraph was revised according to the comments made by all reviewers.

2. Page 405, line 20-21: A goal of the paper is stated "to identify the variables driving GHG emissions from different land-use type[s] in a subalpine valley". However, only the variables controlling the grassland CHG fluxes are presented and discussed, although the average CO2 fluxes for the other ecosystems are compared with the managed grassland. This goal could be better stated as two goals: (I) identifying the variables driving GHG emissions from the grassland and (II) placing the grassland CO2 fluxes in context with the surrounding ecosystems.

We would like to thank reviewer 2 for this detailed and helpful advice, which we therefore included in the revised manuscript. The objectives are now formulated as follows." In this study we quantified winter trace gas fluxes from a sub-alpine grassland in Switzerland. Our specific objectives were (i) to compare different approaches for measuring GHG emissions, the instantaneous gradient method, the permanent automatic monitored gradients, and eddy covariance, (ii) to identify the variables driving GHG emissions from the grassland and (iii) placing the grassland CO_2 *fluxes in context with the surrounding ecosystem. An additional objective (iv) was to estimate the cumulative emissions of* CO_2*,* CH_4 *and* N_2O *from the ecosystem during the snow-covered season."*

3. Page 406, line 1: Shouldn't the average days of snow cover be one number? Maybe this could read "snow cover typically lasts 139-175 days".

We adjusted the sentence to: "Davos is characterized by a mean annual temperature of 2.8°C, and snow cover typically lasts 155 days (Beniston, 1997)."

4. Page 406, line 8: It would be helpful to include the average temperature, total precipitation, and days of snow cover for winter 2010/2011 in comparison to the long-term averages and variation so the reader can place the year of study in context.

Indeed, we also see the importance of these values and therefore adjusted the manuscript accordingly.

5. Page 406, line 21-22: The statement "and are explained in the following paragraph" is unnecessary.

We agree with reviewer and deleted this part of the sentence in the revised manuscript.

6. Page 406-410: Sections 2.3 through 2.5 are really subsections of Section 2.2 (Greenhouse gas flux measurements). I recommend changing the section numbering accordingly.

We followed the suggestion of reviewer 2.

7. Page 407, lines 24-25: A u* threshold of 0.01 seems very low. Typical thresholds are around 0.1-0.2. Even the Goulden et al. 1996 reference indicated a threshold of 0.17. In addition, rejecting data below a u* threshold would avoid underestimation of night time respiration, not overestimation.

The u threshold of 0.01 was a type error in the submitted manuscript and so was the overestimation. We adjusted this in the new version of the manuscript.*

8. Page 407, paragraph lines17-25: What was the typical footprint of the EC measurements? Were the cospectra or energy balance evaluated? If so, please discuss. Also, please give stats on accepted/rejected measurements. Were measurements rejected for highly stable conditions? (Although no extended periods of stable conditions were found, short periods of highly stable conditions should still be removed.) Fig. 4 shows what looks to be daily average measurements of CO2 flux. How were EC measurements gap-filled (or not) in order to derive daily averages? Given that an open-path LI-7500 was used for the EC measurements along with the ample snowfall and winter conditions indicated in Fig. 2d, I am very surprised at the complete data coverage indicated in Fig. 4. Answers to the above questions would help the reader understand this high level of data coverage.

The footprint of the EC tower was calculated according to Kljun et al. 2004 and was added in the revised manuscript (Fig 1). We were however unable to evaluate the energy balance due to missing ground heat flux measurements. Measurements were further filtered for stable conditions. Figure 4 visualizes gap-filled data (Reichstein et al. 2005). Processed flux data cover was 68% between December 2nd and 5th of April – 32% of the missing flux values occurred due to sensor malfunction (e.g. the sonic anemometer had to be replaced in December and in January). After post-processing (including all quality criteria), 20% of high quality flux data remained, which were equally distributed through the whole observation period (for details see the table below).

Table 1: Availability of 30min flux data and % from 2nd of December 2010 until 5th of April 2011. All possible indicated the maximum possible data coverage without sensor malfunction etc, Raw Flux Data indicates all Processed Flux Data calculated from available data and HQ Flux Data indicated the actual High Quality Data after post-processing used in the analysis.

Month	All	%	Raw Flux	%	HQ Flux	%

	possible		Data		Data	
December	1392	100	676	48.56	198	14.22
January	1488	100	950	63.84	246	16.53
February	1344	100	1226	91.22	285	21.21
March	1488	100	1068	71.77	325	21.84
April	240	100	215	89.58	83	34.58
Total	5952	100	4135	69.47	1137	19.10

9. Page 408, lines 2-5: The 5 manual and 2 automatic profiles are north of the tower(s), not surrounding them.

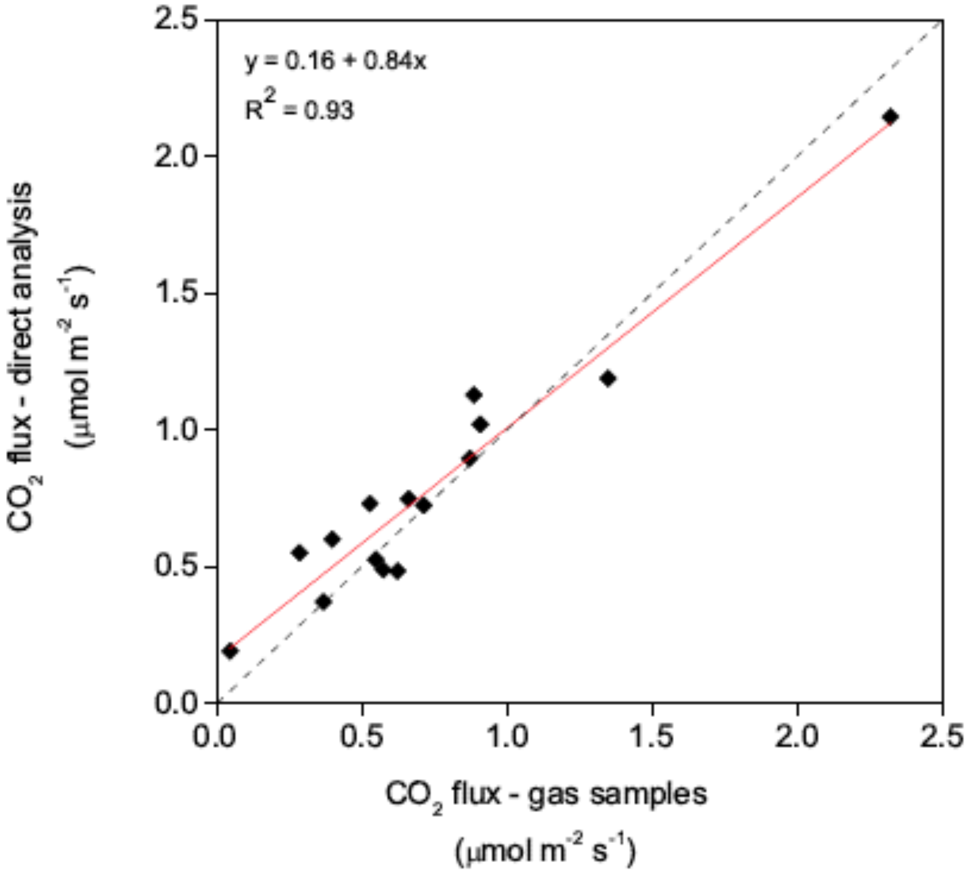
This was adjusted in the revised manuscript.

10. Page 408, line 14: The units of dc/dz are incorrect. They should be umol m-4.

This was a typo in the original manuscript and adjusted in the revised version.

11. Page 409, paragraph lines 1-14: Additional clarification of the ski pole method is needed. What depth intervals were the concentrations sampled at? Same as the 10 cm intervals on the ski pole? Was the ski pole inserted into a different hole each time sampled? For the weekly CO2 flux measurements, it is unclear which CO2 sampling method was used (LI-820, gas chromatograph, both?). Were the two methods compared against each other?

Reviewer 1 stated similar concerns and we therefore improved this section in the revised manuscript, which included the depth of measurements, clarifying that a different hole was used per sampling data.
"The ski pole had 10 cm interval depth markings along the pole to determine the insertion depth into the snow. The pole contained tubing inside and had a perforated tip allowing gas collection at any snow depth (Wetter 2009). Gas measurements were made at 10 cm increments either in the field via a portable gas analyzer directly connected to the ski pole or later in the laboratory following gas sampling. A different hole was used for each sampling date. Gas was collected from the ski pole using a micro diaphragm gas pump (NMP 015M, KNF Neuberger, Balterswil, Switzerland) to pull the air at a rate of approximately 0.4 l min^{-1} through the infrared gas analyzer (LI-820, LI-COR Inc., Lincoln, Nebraska, USA) until CO_2 concentrations remained constant (usually after 30 – 60 s). In addition, gas samples were taken from the ski pole using a 60ml syringe which were immediately transferred into pre-evacuated 12 ml vials (Labco Limited, Buckinghamshire, UK) with a needle."
Since both methods (LI-820 and GC) were used to measure the CO2 concentration we performed a comparison resulting in similar results (see the Figure below, which was not added to the manuscript).



Comparison of direct analysis and gas sampling. The" direct analysi"s represents the data derived from the portable infrared gas analyzer and "gas samples" are based on analysis using the gas chromatograph. The dotted line show the 1:1 relationship (Steinlin 2011).

12. Page 409, line 9: "with 60 mL" what? Syringes? Was there a pump attached to the ski pole to draw the gas into the ski pole?

We adjusted the original sentence to "Gas was collected from the ski pole using a membrane pump to pull the air through the infrared gas analyzer and 60 ml syringes were used to collect samples for laboratory analysis".

13. Page 410, Section 2.5: It would be nice to see the spatial transects (grassland transversal & longitudinal as well as the one across the different vegetation types) in Figure 1. If for no other reason, it would help clarify the different study components.

We added such information in Figure, which includes both transects on the grassland and also the transect across vegetation types. We further included the footprint of the EC tower.

14. Page 410, line 26: What depth was the soil water content measurement made?

Soil water content was measured in 10cm depth and added in the revised manuscript.

15. Page 411, lines 20-24: It is unclear whether the 3 cm depth soil temperature described in Section 2.6 is the temperature at the snow-soil interface. It is also unclear whether topsoil (line 23) is the same as the snow-soil interface. Please clarify. Finally, on line 24, Fig. 2a-c are indicated to show the topsoil temperature, but these figure panels show three different things. Only Fig. 2b appears to show soil temperature.

In the revised manuscript, we consistently use the term "soil temperature at 3cm depth" to avoid any confusion..

16. Page 412, line 1: To exemplify that the maximum snow depth was "well-below the long-term average", it would be helpful to give the standard deviation of the long term average.

We write:" Maximum snow height (63 cm) was reached 27th of February (Fig. 2f), which was below the long-term average of 89 ± 30 cm (T. Jonas; unpublished data)."

17. Page 412, lines 15-17: The gradient CO2 fluxes increased starting with snowmelt but then decreased substantially for the last two measurements, contrary to the EC pattern… I recommend moving the sentence on Page 13, lines 14-16 to this location to explain the last measurement. In addition, I wonder whether the measurement just before 1 Apr may also have be erroneously influenced by active snowmelt and/or altered snow structure, as the discussion on Page 417, line 21-29 might indicate. Does removal of this additional point affect the results (for all three fluxes)? Please comment.

We are slightly unsure on which sentence reviewer 2 recommends to move where. In general we restructured and rewrote many party of the results section, e.g. "Fluxes of CO_2 calculated from the gradient measurements showed largest efflux rates at the beginning and end of the snow-covered period (Fig. 3a, 2.3 ± 2.2 µmol CO_2 m^{-2} s^{-1} in December 2010). Decreasing CO_2 fluxes were measured during the peak winter period (Jan/Feb 2011) reaching a minimum of 0.02 µmol CO_2 m^{-2} s^{-1} on the 23nd of February. Thereafter and with the beginning of snowmelt CO_2 fluxes started to increase along with high concentrations of CO_2 at the soil-snow interface (0.51 ± 0.23 µmol CO_2 m^{-2} s^{-1} in March and April) (Fig. 3a). In comparison to the gradient approach, CO_2 fluxes measured by the eddy covariance method showed much larger efflux rates associated with larger variation (Fig. 4, grey polygon), while providing better temporal resolution."

Concerning possible errors in our gradient derived flux measurements during snowmelt we highlight the section in the discussion paragraph, which states possible errors. In contrast the EC methods was only partly applicable to validate the gradient measurements since both approaches cover different scales (ecosystem scale vs plot scale) and the EC results show generally larger fluxes as estimated by the profile method but at the same time showing much larger variation (Fig 4, grey polygon). In order to gain confidence in the profile measurements we sampled 4-5 profiles during each sampling data. To avoid confusion and guessing of results we adjusted the revised manuscript accordingly. This included a recalculation of the seasonal budget for the time of actual available measurements only (140 days in the original version of the manuscript and 122 days in the revised version). See also the answers given to *reviewer 1 concerning this critical point.*

18. Page 412, lines 19-20: How can the short-term temporal variation in CO2 fluxes between the gradient and EC methods be compared? The sampling frequencies are drastically different. I think the latter part of the sentence beginning with "associated with…" could be removed and replaced with a description of how well the seasonal trends compared between the EC and gradient methods. In addition, since comparison of the different methods is a major goal of the paper, better presentation (including visualization) of the comparison between the gradient and EC measurements is needed. Some suggestions: Overlay the daily average EC data on Fig. 3a or show as a separate panel of the same figure with the same x- and y-scales. And/or if interesting, present a scatter plot of the relationship between gradient and EC fluxes.

We did not compare short term temporal variation in CO2 flux can not be compared between the both methods, EC and gradient. First of all, profiles were only measured once per week, and therefore these measurements only provide a snapshot of the flux, while the EC methods measures continuously. Secondly a direct comparison between both methods for matching sampling times could not be performed due to data scarcity. With data scarcity we refer to the described in detail filtering criteria of EC data. EC data was either missing due to sensor malfunction or data were rejected due to low quality flag. Howeve,r in order to better visualize the differences in the results derived from both methods we updated Figure 3, which now shows additionally the EC-based CO2 flux (daily averages of the gapfilled data).

19. Page 412, line 24: Reference to the grey polygon in Fig. 4 doesn't seem correct here. The CO2 flux drop toward 0 is better indicated by the black line in Fig. 4, with the grey polygon showing larger daily variability which in itself does not indicate photosynthetic activity. If the diurnal pattern of CO2 flux supported photosynthetic activity (by showing a negative correlation with light), this could be mentioned instead.

Our original aim for this graph was to show both, the calculated uncertainty in the fluxes as produced by the online gap-filling tool but also the daily variation of the flux. To avoid confusing we adjusted this figure and include the uncertainty only in the revised manuscript. We further included the EC fluxes in Figure 3 for better comparison with the results derive from the profile measurements.

20. Page 412-413: Section 3.2: Were there any temporal correlations between CO2, CH4, and N2O?

We did not detect significant temporal correlations between the three different GHGs.

21. Page 413, line 13: Specify that the results pertain to the CO2 fluxes gathered with the gradient method.

Corrected to: The weekly measured CO_2 effluxes using the gradient approach correlated most closely with snow water equivalent (h_{SWE}; r^2=0.8) and with soil temperature at 3 cm depth (r^2=0.78; Figure 5).

$$F_{CO_2} = 7.88e^{(-0.17 h_{SWE})} \qquad \textit{(Eq. 6)}$$

22. Page 413, line 13: If SWE was found to be a major correlate with CO2 flux (and later CH4 flux), it would be nice to see it in Fig. 2, perhaps as an overlay on the snow depth plot.

We agree with reviewer 2 and added snow-water equivalent to Figure 2, though not in the snow height panel but in an additional panel below Fig. 2 f.

23. Page 413, line 16-18: What was the relationship between SWE and soil temperature? Was SWE simply a proxy for soil temperature in the data range? Both response functions in Fig. 5a,b show similar, albeit flipped exponential relationships. The discussion on Page 420, line 25 – Page 421, line 7, seems to dismiss SWE as a control on CO2 flux for this reason. Given the answers to these questions, is a general linear model with these two terms appropriate and ecologically relevant? Would it not be better to mention the result but then conclude that temperature alone best described the seasonal pattern of CO2 flux? If SWE is ecologically relevant, better justification of its relevance and discussion of the supposed mechanisms of control are needed in the Discussion section.

We would like to thank reviewer 2 for the constructive comments. Now, we removed the general linear model and present both correlation. In the discussion, we discuss the potential mechanism for the extraordinary high temperature dependency (limitation of substrate diffusion; substrate depletion due to progressing winter (especially after manure application before snowfall); and for SWE (progression time; gas diffusion)

24. Page 413, line 21-22: The lack of any significant correlates between environmental measurements and EC CO2 flux is surprising, since the seasonal trend appears similar to the gradient measurements. In addition, what frequency of EC data were analyzed? The half-hourly measurements or daily averages? Assuming daily averages were used, I wonder if the large daily variability in the EC measurements obscured seasonal correlations with soil temperature and SWE or other variables. To this end, I would be interested to know whether a longer averaging interval (e.g. weekly averages) of the EC data yields similar relationships as seen in the gradient measurements.

We re-analyzed our data which showed a similar relation to snow water equivalent (h_{SWE}) and soil temperature as our gradient data. Continuous values of h_{SWE} were derived by linear interpolation of the snow densities measured in the field on weekly basis and continuous available snow height measurements. We further included h_{SWE} in Figure 2 and extended/adjusted the results and discussion accordingly.

25. Page 413, Section 3.3: Were there any air pressure measurements taken at the meteorological station? If so, it would be useful to test a correlation between air pressure and the three GHG fluxes, as some previous studies (e.g. Sachs et al. 2008 JGR) have found a relationship.

Unfortunately, there was no pressure sensor installed at the site during 2010/2011.

26. Page 413, line 19 – Page 414, line 2: The seemingly paradoxical result - that gradient measurements of CO_2 flux were commonly lower than the EC method but higher when using the modeled gradient data - clearly points to a problem with the gradient model. The averages in Table 2 further exemplify this. I would expect a good model to overestimate sometimes and underestimate other times, but the gradient flux model almost always overestimates the measurements in Table 2. Furthermore, the model was used to extend the gradient measurements into the beginning and ends of the snow covered period during which soil temperatures far exceeded the very small range of temperatures during the period of measurements used to create the model. The range of temperatures during the non-rejected gradient measurements ranged from +- ~0.3 °C whereas during the extrapolated period the temperatures climbed as high as ~5 °C. Even a good model fit for the data in the small measured temperature range would yield dubious estimates at these much higher values. This is why the Jan-Mar modeled gradient values at least more reasonably show lower fluxes than the EC measurements whereas the Nov and Apr modeled gradients are drastically (and unreasonably) higher than the EC measurements. Therefore, the model should be re-evaluated for ecological applicability or extrapolation of the gradient measurements over the snow covered period should be done differently. This comment applies to all other places in the results and discussion where the modeled results are referenced. Perhaps the model could be used only during the period of analyzed gradient measurements, then simply extend the ending measurements to the periods before and after the data. I think this would be reasonable since the EC measurements show only moderate changes at the beginning and ends of the season. Alternatively, a correlation could be done between the EC and gradient measurements and, if strong, could be used to fill the gradient gaps since they appear to follow the same general trend. This would still allow a comparison of the methods using the actual measurements, which I think would be more useful than comparing the seasonal totals since it seems pretty clear the gradient data were consistently underestimated compared to the EC data. Finally, if a model is used to extrapolate the gradient measurements (even if only during the period of measurement), I recommend showing the modeled data in Fig. 3 so the reader can see how well it performs and whether it is a reasonable extrapolation.

We would like to thank reviewer 2 for these detailed comments on the modeling part of our study. Given the fact that the gradient extrapolation widely overestimated the seasonal budget and the general linear model originally used was partly misleading we thoroughly revised the new version of the manuscript. The following changes were made in the revised manuscript:

1. *The extrapolation between measurement points was only performed for the time period with available data for both methods (1st of Dec – 31st of March) totaling 121 days.*
2. *We added the modeled values for the gradient fluxes in Figure 3 for direct comparison with gap-filled EC data (daily averages).*

27. Page 414 lines 3-14: As recommended for CO_2 flux, it would be helpful to show the modeled values (or running mean) used to extrapolate CH_4 and N_2O fluxes as overlays in Fig. 3.

Figure 3 was adjusted accordingly.

28. Page 414, line 18: The sentence beginning "Measurements were extrapolated…" is unnecessary since the methods of extrapolation are given in the preceding section. In addition, the statement isn't quite complete since Eqns. 6 & 7 apply only to gradient CO2 and CH4 fluxes.

This was corrected in the revised version of the manuscript.

29. Page 415, Section 3.5: The discussion and accompanying photograph of why the automatic gradient measurements failed is very useful to the scientific community and rarely included in manuscripts. I think this section adds significantly to the paper.

We would like to thank reviewer 2 for the encouraging comment.

30. Page 415, lines 13-15: The spatial variation in CO2 flux for the transversal and longitudinal cuts seem very similar to me, and their coefficients of variation differ by only 4%. I would say the variation along the transversal cut was "similar" or only "slightly stronger" than the longitudinal cut.

The sentence was adjusted accordingly. "We observed similar variation in CO_2 fluxes along the transversal cut of the grassland than along the longitudinal cut of the valley (Fig. 7a). Coefficients of variation were 75% for the whole grassland, 76% for the transversal and 72% for the longitudinal transect, respectively."

31. Page 415, line 17: Change "were" to "averaged".

We decided against changing this in the revised version, since this sentence states the maximum uptake rate only.

32. Page 415, line 26: The variability of N2O fluxes across the grassland is described at the end of the paragraph. Therefore, I recommend reporting only the mean value in this sentence.

We adjusted the section to: "N_2O fluxes across the grassland averaged 0.03 nmol N_2O m^{-2} s^{-1} (Fig. 6c)."

33. Page 415-416, Section 3.6: Were there any significant relationships (or lack there-of) between CO2, CH4, and N2O fluxes in the spatial sampling?

We could not determine any significant relationship between CO2, CH4, and N2O fluxes along the transects.

34. Page 416, Section 3.7: From Figs. 7 and 8 it appears that the LI-820 gave higher CO2 flux readings along the transversal cut of the grassland compared to the gas chromatograph method. Please comment. Depending on the answer, it may be

important to include this result/discussion in the paper since method comparison is a major goal.

As shown above both methods were compared and did not show significant differences. Fluxes stated in the subparagraph on the spatial variation of all GHG across the grassland (between 0.04 µmol m^{-2} s^{-1} and 1.14 µmol m^{-2} s^{-1}) were in the same order of magnitude than the fluxes measured using the LI-820 across ecosystem types (0.83 +/- 0.38 µmol m^{-2} s^{-1}) with two exceptions at the edge of the grassland (Figure 8).

35. Page 416-423 Discussion: Many of the results are restated in the Discussion before putting them in context of other studies or their significance. The Discussion section can therefore be better streamlined and more focused by removing redundancy and focusing on specific aspects of the results. In the comments below, I pick out some examples of this and offer some suggestions for improvement.

We would like to thank reviewer 2 for these comments. We believe the current discussion has been streamlined and avoids a repetition of previously stated results.

36. Page 416, line 25 – Page 417, line 4: A few comments here:

(I) This comparison of the spatial flux measurements to both the EC and weekly gradient measurements should be first presented in the Results section.

Done

(II) The average peak-winter EC measurements also look to be of the same order of magnitude as the spatial and weekly gradient measurements... therefore, the use of "Contrastingly" on Page 417, line 3 doesn't seem to be appropriate.

We disagree with reviewer 2 since the EC derived fluxes were about 50% larger than the fluxes measured by the gradients (Jan-Mar, Table 2).

(III) Include the applicable peak-winter EC value on Page 417, line 4 since values are presented for peak-winter spatial and weekly gradient measurements (Page 417, line 3).

Average flux values indicating the large differences were included in the revised manuscript.

(IV) Finally, since the spatial measurements were more similar to the weekly gradient measurements than the EC measurements during peak-winter, wouldn't it be appropriate to dismiss the notion that the different scales of measurement were responsible for the discrepancy between EC and weekly gradient measurements (Page 416, lines 20-24) and say that the methodology differences discussed in the following paragraphs are the more likely culprits? A statement such as this could serve as a transition between the paragraphs on Page 417, lines 4-5.

We moved this statement further down as recommended by reviewer 2.

37. Page 418: I think much of this page can be removed. I'm not sure that the suggestions on measuring snow structure significantly adds to the paper. The discussion of the benefits of tracers is often redundant with the methods section and can probably be almost completely deleted. The reasons for failure of the automatic gradient sampling were already presented in the results section and therefore the

discussion can be limited to suggestions for future improvement of the system.

We deleted redundant information in the revised manuscript (e.g. most of the information on the automatic gradients), without deleting the complete page 418.

38. Page 418, lines 12-17: The reason for the large discrepancy between EC and gradient CO_2 flux at the end of winter is suggested to be the dissolution of CO_2 in liquid snowmelt and removal from the system via leaching. However, the end of the preceding paragraph cites a study which estimated that "the magnitude of CO_2 flux in the liquid phase was two orders of magnitude less than the upward flux through diffusion of CO_2 in the gaseous phase". These two conflicting statements need to be reconciled. Additionally, wouldn't dissolution of CO_2 in leaching snowmelt also be at least partly reflected in EC measurements (assuming a net export of snowmelt from the footprint)? As indicated in a previous comment, couldn't the poor match between EC and gradient measurements during snowmelt at the end of the season (especially the last two gradient measurements) be more likely due to changes in snow porosity and tortuosity (discussed on Page 417, 21-29 as the largest uncertainty in gradient measurements)?

We deleted this misleading argument to further streamline the discussion paragraph and to avoid confusion. We thank reviewer 2 for pointing to this discrepancy.

39. Page 418, lines 15-17: Wouldn't a downward flux of CO_2 from dissolution in snowmelt and subsequent leaching result in a smaller concentration gradient rather than a smaller diffusion coefficient?

We can only guess how large a possible downward flux could be and we agree with the previous statement that if such leaching occurs we should see a similar decrease in fluxes measured by EC.

40. Page 419, lines 14-25: This paragraph belongs in the methods section.

We moved the crucial information to the M&M paragraph.

41. Page 420, lines 8-10: Specify whether the increase in CO_2 flux was due to increasing or decreasing soil water content above/below the threshold given.

Such increases in CO_2 fluxes were related to increases in soil moisture.

42. Page 420, lines 17-19: Increasing soil moisture is postulated to be among the factors responsible for increasing CO_2 flux during snowmelt, but soil water content was uncorrelated with CO_2 flux. Please reconcile.

In our study soil water content did not show a significant correlation to CO_2 flux (probably due to a small temporal variability), however other studies showed exactly such relation.

43. Page 421, line 1: Change "respiration" to "CO_2 flux", since it was NEE rather than respiration that was measured.

This paragraph was deleted in the revised manuscript, due to the comments given by reviewer 1.

44. Page 421, line 3-7: This is the first mention of this result and therefore should be first presented in the results section.

This result was adjusted in the new version of the manuscript, which included a re-analysis of the data.

45. Page 419-421, discussion of CO2 flux results: There is no discussion of why none of the correlates with CO2 flux from the gradient method were found to correlate with the EC method of CO2 flux. If the ecological explanations given for the gradient correlations are valid, should not they also have been reflected in the EC measurements? I think this may reflect a need to average out short-term temporal variability present in the EC measurement commented about previously. In any case, discussion on this topic is warranted.

After detailed reanalysis of the data and using daily averages (based on the gapfilled data) we found similar correlations as found for the CO_2 fluxes derived from the gradient measurement.

Daily averages were used in order to average the short-term temporal variability in EC fluxes out – we are therefore very thankful to the constructive comment provided by reviewer 2.

46. Page 421, line 8: Methane uptake is stated to be "as large as" -0.14 nmol CH4 m-2 s-1, However, it appears that this was the average methane uptake for the winter and the number should be referenced as such.

Done.

47. Page 421, line 8-12: The comparison of methane flux rates with previous study is contradictory. A measured rate of -0.14 nmol CH4 m-2 s-1 is stated to be similar to two previous studies (Sommerfield et al. 1993 and Alm et al. 1999). But then the rates in the previous studies are said to be much lower than the measured rates in this study. Also, be consistent with the language used to compare negative fluxes. The statement "Methane uptake was as large as…" refers to a large uptake, or very negative flux. But this is followed by "uptake rates were considerably lower" when seemingly referring to more negative values of -0.8 and -0.1. Therefore, very negative rates are referred to as both high and low. Finally, the latter statement is in itself confusing because the indicated rate of -0.14 is within the range of -0.8 and -0.1. Please clarify.

We removed the misleading sentence - "Methane uptake was as large as −0.14 nmolCH4 m−2 s−1 on the Dischma grassland." – to avoid restating previously shown results and avoid confusion concerning the magnitude of methane uptake. We further adjusted this subparagraph as follows: "The methane uptake rates observed in our study were similar than in dry sub-alpine soils (-0.14 nmol CH_4 m^{-2} s^{-1}; Mast et al. 1998) and in a completely drained fen in Eastern Finland (-0.09 nmol CH_4 m^{-2} s^{-1}; Alm et al. (1999). By comparison, Sommerfeld et al. (1993) measured uptake rates up to -0.8 nmol CH_4 m^{-2} s^{-1} for a sub-alpine meadow in the Rocky Mountains, USA. While Mast et al. (1998) found net methane production under moist and water saturated condition, our results indicate that even at a volumetric water content of 0.4 m^3 m^{-3} the grassland in the Dischma Valley is still characterized by net methane consumption."

48. Page 421, lines 8-14: Were the studies referenced to compare methane flux rates also done in winter? Please clarify.

These studies were referenced for such comparison, since these studies were carried out in similar ecosystems and using similar approaches.

49. Page 421, line 15: Is 0.4 m3 m-3 considered to be high or low soil moisture for this site? It would be helpful to place this in the context of saturation or soil porosity. Also, what is the significance of this result and what might be the cause of it?

Values of 0.4 m^3 m^{-3} were reported for other sub-alpine grasslands in Switzerland representing the upper end of possible values for soil water content. Furthermore soil water saturation has been shown at other grassland sites during winter, e.g. Merbold et al. 2012, Biogeochemistry.

50. Page 421, lines 16-19: Why might the two studies differ? The discussion on lines 20-29 is a nice example of the type of discussion that could be done here.

The results of the study by Mast et al. (1998) only differ for the water-saturated site, showing the opposite pattern than our results. However the seasonal course as reported for the dry sub-alpine soils coincides with our measurements from the Dischma valley. The manuscript was adjusted accordingly.

51. Page 421, line 21-23: I think an R2 of 0.43 for the relationship between CH4 flux and soil temperature is moderate, not weak.

We agree on the moderate relationship with soil temperature and changed it in the text.

52. Page 421, discussion of CH4 flux: SWE showed the most significant relationship with weekly CH4 flux and was used to extrapolate the CH4 flux measurements throughout the season, but potential mechanisms and the ecological significance behind this relationship are not discussed.

We included some discussion on this topic in the revised manuscript, while few points have been stated before. Snow water equivalent combines several variables, which strongly influence diffusion through the snowpack, therefore each gas should be affected. This is the case for CO2 (gradient and EC) and CH4 but not for N2O. However we also found largest spatial variability in N2O fluxes, with several hot spots and plots of only minor importance. This variability was mostly caused due to the fertilizer application before the first snow fell in November, resulting in spots of large nutrient availability. Even though we were unable to measure soil nutrient content in this study such patterns have been reported by other study undertaken in managed grassland ecosystems, e.g. AEE, Hoeft et al. 2012).

53. Page 422, line 1-3: How would distance to river/stream or slope drive spatial variability in CH4 fluxes along the transversal cut of the valley? Do these things correlate with vegetation or soil characteristics (i.e. soil temperature, moisture, organic matter)? Should they also then reflect spatial variability in CO2 flux?

Unfortunately, we do not have these additional environmental variables for the spatial transect on the grassland. However, fluxes are expected to increase towards the stream (emission) and to decrease towards the grassland slope, however we only measured CO2 at these locations (Figure 8) since the transversal cut through the grassland did not extend toward the edges of the grassland.

54. Page 422, lines 4-9: Were the comparable studies done during winter?

As indicated in the original manuscript, the studies stated we looking at the same GHGs and in comparable ecosystems during winter. One could further state e.g. Filippa et al. 2009, Biogeochemsitry, Wolf et al. 2012, Nature.

55. Page 422, lines 20-22: What are the significance of the results of the Mohn et al. 2013 study in the context of this study and especially the foregoing discussion on hypothetical control of N2O flux by soil moisture? If still under investigation, say so.

We clarified the findings by Mohn:" In mid Februrary, high-precision N_2O isotopomer analysis using laser spectroscopy indicated that the main N_2O source processes were heterotrophic denitrification and nitrifier denitrification (Mohn et al., under revision). However, both processes might partly be outbalanced by N_2O consumption, which prevent a correlation with environmental variables. "

56. Page 423, lines 3-5: Discussion of the GHG budget contributions can be restated to remove redundancy with the results and also include the significance (ecological or otherwise) of the result.

We strengthened the discussion by moving the contribution of CH4 and N2O to the total budget to the results section and further included a sentence on the relevance of such information in the discussion. These low contributions of N_2O and CH_4 to total GHG budgets strongly suggest that the hypothesized offset of a net carbon sink by other GHG's than CO_2 is negligible for the majority of grasslands.

57. Page 423, line 13: How might this manure application have affected the other fluxes (spatially, temporally, and budget contribution)?

We think that manure application affected the temporal pattern with the highest CO effluxes directly after manure application that was applied just before the permanent snow cover (added to the Discussion). Manure might also explain the higher spatial variability of N2O fluxes than of the other GHG, because manure applications have been shown to directly affect N2O fluxes while not directly affecting CO2 and CH4 fluxes on differently managed grassland ecosystems in Switzerland (Imer et al. 2013, BGD, Imer et al. in prep.) which included larger spatial and temporal variability of N2O. Furthermore as analyzed for a restored grassland N2O fluxes may contribute up to 20% to the annual budget (Merbold et al. in prep).

58. Page 423, lines 15-17: It is not clear what this sentence means. Was the same conclusion about manure application found in the Yao et al. 2009 study?

Similar findings of larger N2O emissions and large spatial variability under fertilization were shown by Yao et al. 2010.

59. Page 423, Section 4.2: There is no discussion of the results from different land uses. This is needed.

In the revision, we discussed the land-use aspect: "The significantly higher winter CO_2 fluxes in the grassland than in the forest (Figure 8) are consistent with the observation of decreasing summer soil respiration rates during afforestation of a sub-alpine pasture (Hiltbrunner et al., 2013). In their study, the lower rates in the forest were explained with a smaller root turnover, a lower litter quality and a less favorable microclimate in the forest than in the adjacent grassland. In our study, the thinner snow cover in the forests (<30 cm) leading to colder soils (Groffman et al., 2006) might have contributed to the lower CO_2 effluxes in the forest."

60. Page 423, Conclusions section: In my opinion, if a Conclusions section is to be included, it should revisit the significance of the major goals of the paper. This is done for the determined controls on CO2, CH4, and N2O fluxes, but is not really done for the other major goals of the paper.

We revised the Conclusion: "Total greenhouse gas emissions during the winter 2010/2011 in the Dischma Valley were primarily determined by CO_2 fluxes. Even when calculating the Global warming potential (GWP) for all three GHGs, the contributions of N_2O and CH_4 to the seasonal budget were minor, 5% and < -0.1%, respectively. GHG fluxes of CO_2 and CH_4 varied largely with changes in snow water equivalent. Snow water equivalent was identified as a physically relevant variable affecting gas diffusion of CO_2 and CH_4 through the snow pack. Our method comparison indicated that during high winter the CO_2 fluxes based on the gradient approach were 50% smaller than those estimated by eddy-covariance, probably due to an inappropriate estimation of gas diffusion in the snow. This implies that additional efforts are needed to accurately measure CO_2 fluxes at the plot scale.

GHG fluxes varied strongly within the grassland with N_2O showing the largest coefficients of variation which was most probably occurring due to a application of organic fertilizer shortly before the first snow event. CO_2 emission across different land-use types in the Dischma Valley were shown to be largest on the grassland with about 50% higher rates than in an adjacent forest."

61. Page 423, line 20-22: Re-stating the relative contributions of the three GHG gases is redundant with the Discussion. The sentence preceding this already does a good job of concluding on this result.

Since we removed the preceding sentence from the Discuiion, we kept the contribution of CH4 and N2O in the Conclusion of the revised version of the manuscript and added statements on the major outcomes of this study. Please see the copied text above.

62. Page 423, lines 22-23: The statement that CH4 fluxes varied largely with changes in temperatures at the snow-soil interface directly conflicts with the discussion on Page 421, lines 21-22. Please reconcile.

We agree with reviewer 2 and this problem does not occur any longer since we re-analyzed the data and determined SWE besides soil temperature as the major variables driving CH4 and CO2 flux.

63. Page 423, line 23-Page 424, line 4: I don't think a shorter winter season reducing the winter contribution to the GHG budget is relevant here, since season definition is more a technicality, and a mechanism for control of fluxes by snow cover itself was not well supported. I think the more ecologically relevant discussion that could be expanded upon is that winter temperatures are expected to change, and since the CO2 and CH4 fluxes showed the greatest response to temperature (other than SWE), this has bearing on expected future emissions.

We agree with reviewer 2 on the length of the winter season and removed it from the manuscript to avoid any misunderstanding. However we still argue that no common consensus on the definition of winter has been agreed on and therefore complicating possible comparison approaches. Furthermore as stated in the original version of the manuscript and in the revised version SWE explained a larger portion of the variability in GHG fluxes (CO2 and CH4). While SWE includes several parameters as stated before it directly affects diffusion through the snowpack

64. Page 434, Table 1: Include the depth of soil temperature measurements.

The information was added in table 1, 3cm depth.

65. Page 435, Table 2: There is a column of modeled EC measurements but no significant correlations with environmental variables were found nor a model described. Please explain.

This should be rephrased to gap-filled. Gap-filling was based on a MDS (marginal distribution sampling) procedure as described by Reichstein et al. 2005.

66. Page 436, Table 3: Since the CH_4 and N_2O fluxes were only measured via the gradient technique, there should be a "—" in the EC-derived Cumulative Flux and Total GHG Budget rows instead of repeating the gradient-derived value. The caption can say that the numbers used to calculate the contribution to the overall budget for the EC technique include the gradient values for CH_4 and N_2O flux.

This information was originally indicated by (a/b). However we adjusted this according to the suggestions given by reviewer 2 in the revised manuscript. And we try to give 2 budget, one as estimated by EC and gradients of CH_4 and N_2O and another budget which is based on gradient measurements and an extrapolation based on the functional relations as stated in the Results paragraph.

67. Page 437, Figure 1: It might be the way Biogeosciences Discussions fits the figure and caption on the same page, but the size of this figure needs adjustment to be readable and informative. Perhaps adjusting the orientation of the panels and increasing panel size as one zooms in would help.

That was unfortunately a Biogeosciences Discussion product. We changed Figure 1 in the revised version of the manuscript, including the transects of the spatial campaign and a footprint of the EC tower.

68. Page 438: Figure 2 caption: Specify the time frequency and any averaging done in panels a-d. Specify what the error bars indicate in panel e. The units of measurement are unnecessary in the caption since they are included in the y-axis labels.

We removed the units from the figure caption and adjusted Figure in the revised manuscript, now showing additional variables, such as SWE, global radiation and wind speed during the time of observation. Error bars in the original panel e denote the standard deviation of the 5 measurements of snow density at each sampling data. Time averaging for all other variables was 30min.

69. Page 439, Figure 3 caption: Specify what the error bars indicate.

We added this information in Figure caption 3 and further included the modeled data – based on the functional relations – for each greenhouse gas. N_2O estimated were based on a linear interpolation.

70. Page 440, Figure 4: In a previous comment I suggested that this figure be combined with Fig. 3 to enable better comparison of the EC and gradient methods. In the event my suggestion is rejected, I have a couple suggestions for this figure alone: (I) Be consistent among figures with the calendar date vs. Julian day on the x-axis. It's hard to compare the gradient versus the EC data because the date format is not the same. (II) I'm not sure the grey polygon adds anything here; I think it can be omitted.

In the revised version of the manuscript we stayed consistent concerning the labeling of the x-axis. We further removed the grey polygon and only added the uncertainty as produced by the gap-filling tool.

71. Page 443, Figure 7 caption: How were concentrations in the snow profile interpolated between measurement points? Same as that state in Fig. 8?

Concentrations were similarly interpolated as stated in Figure 8. We added this information to the figure caption.

72. Page 444, Figure 8: To save space, the two panels could probably be overlaid as in Fig. 7.

We overlaid both panels as done in Figure 7.

Technical Corrections:

73. Page 404, line 26: Change “Much less” to “Far fewer”.

Done.

74. Page 405, line 10: Change “seems to” to “has been documented to”

Done.

75. Page 405, lines 9-15: Use consistent tense to refer to findings of specific studies. For example, reference to the Schindlbacher et al. (2007) study uses past tense while reference to the Lohila et al. (2007) study uses present tense.

Done.

76. Page 405, line 18 – I recommend changing the comma in “emissions, the instantaneous” to a semi-colon.

Done.

77. Page 405 – “type” should be “types”.

Done. The sentence was adjusted to “the grassland and (iii) placing the grassland CO_2 *fluxes in context with the surrounding ecosystems.”*

78. Page 410, line 24: Change “such as” to “including”.

Done.

79. Page 410, line 25: Insert a comma between the closing parenthesis and “soil temperature”.

Done.

80. Page 411, line 12: “than” should be “then”.

Done.

81. Page 411, line 19: “distinctly” seems to be the wrong word here. “Considerably”?

This was changed to “strongly”.

82. Page 412, line 26: Insert “The” before “largest uptake rates…”

Done.

83. Page 414, line 7: Replace “annual” with “seasonal”

Done.

84. Page 414, line 19: Replace “cumulated” with “cumulative”

Done.

85. Page 415, line 4-5: Use consistent tense. Change “have chosen” to “chose”.

Done.

86. Page 415, line 26: Change “mean fluxes” to “a mean flux”.

This was changed to “averaged”.

87. Page 415, line 27: “Fig. 6c” should read “Fig. 7c”.

Done.

88. Page 416, line 6: Change “considerable” to “considerably”.

Done.

89. Page 416, line 19: Insert “for the gradient method” between “underestimation” and “during”

Done.

90. Page 416, line 22: Change “to quantify” to “quantification of”.

Done.

91. Page 416, line 22: Remove “so-called”.

Done.

92. Page 417, line 2: Change “than” to “as”

Done, this part of the Results paragraph was changed considerably.

93. Page 417, line 3: Add “, respectively” after the values in parentheses.

Done, this part of the Results paragraph was changed considerably.

94. Page 417, line 4: Change “results” to “magnitudes”.

Done, this part of the Results paragraph was changed considerably.

95. Page 417, line 5-7: The first sentence of this paragraph is awkward. Reword.

Done. “The underlying approach of the gradient method is the estimation of gas diffusivity across the snowpack by measurements of snow density assuming steady-state conditions (Eq. 2).”

96. Page 418, line 13: “year” should read “winter”, and “Table 3” should read “Table 2”.

Done, most of this subsection was rephrased/reformulated.

97. Page 419, line 20: Change “methods underestimates” to “method underestimated”.

Done.

98. Page 420, line 3: Change “clearly” to “clear”.

Done.

99. Page 420, line 5: Change “Mountain” to “Mountains”.

Done.

100. Page 420, line 12: “Fig. 2e” should be “Fig. 2c”.

Done.

101. Page 420, line 13: Remove “period”.
Done.

102. Page 420, line 22: Change “and the according” to “do to the according”.

Done. This part was largely rephrased in the revised manuscript.

103. Page 420, line 26: Insert “temporal” between “the” and “variability”.

Done.

104. Page 423, line 21: Change “the contribution of … was minor” to “the contributions of … were minor”.

Done.

105. Page 436, Table 3 caption: “winter 2012/2011” should read “winter 2010/2011”.

Done.

106. Figures 1, 2, 3, 5, & 7: These figures need larger fonts for axes values, labels, and legends.

Done.

107. Page 437, Figure 1 caption: There are two typos: (I) on the first line, “Dischmavalley” should be two words, and (II) on the second to last line, “automatically” should be changed to “automatic”.

Done.

108. Page 444, Figure 8 caption: “Dischmavalley” should be two words.

Done.