Review of Merbold et al.: Winter greenhouse gas emissions (CO_2, CH_4 and N_2O) from a sub-alpine grassland

General Comments

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.
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 CO_2 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 CO_2 fluxes in context with the surrounding ecosystems.

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".
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.
5. Page 406, line 21-22: The statement "and are explained in the following paragraph" is unnecessary.
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.
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.
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 CO_2 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.
9. Page 408, lines 2-5: The 5 manual and 2 automatic profiles are north of the tower(s), not surrounding them.
10. Page 408, line 14: The units of dc/dz are incorrect. They should be umol m^{-4}.
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 CO_2 flux measurements, it is unclear which CO_2 sampling method was used (LI-820, gas chromatograph, both?). Were the two methods compared against each other?
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?
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.
14. Page 410, line 26: What depth was the soil water content measurement made?
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.

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.
17. Page 412, lines 15-17: The gradient CO_2 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.
18. Page 412, lines 19-20: How can the short-term temporal variation in CO_2 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.
19. Page 412, line 24: Reference to the grey polygon in Fig. 4 doesn't seem correct here. The CO_2 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 CO_2 flux supported photosynthetic activity (by showing a negative correlation with light), this could be mentioned instead.
20. Page 412-413: Section 3.2: Were there any temporal correlations between CO_2, CH_4, and N_2O?
21. Page 413, line 13: Specify that the results pertain to the CO_2 fluxes gathered with the gradient method.
22. Page 413, line 13: If SWE was found to be a major correlate with CO_2 flux (and later CH_4 flux), it would be nice to see it in Fig. 2, perhaps as an overlay on the snow depth plot.
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 CO_2 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 CO_2 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.
24. Page 413, line 21-22: The lack of any significant correlates between environmental measurements and EC CO_2 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.

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.
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.
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.
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 CO_2 and CH_4 fluxes.

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.
30. Page 415, lines 13-15: The spatial variation in CO_2 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.
31. Page 415, line 17: Change “were” to “averaged”.
32. Page 415, line 26: The variability of N_2O fluxes across the grassland is described at the end of the paragraph. Therefore, I recommend reporting only the mean value in this sentence.
33. Page 415-416, Section 3.6: Were there any significant relationships (or lack there-of) between CO_2, CH_4, and N_2O fluxes in the spatial sampling?
34. Page 416, Section 3.7: From Figs. 7 and 8 it appears that the LI-820 gave higher CO_2 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.
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.
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. (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. (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). (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.
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.
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)?

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?
40. Page 419, lines 14-25: This paragraph belongs in the methods section.
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.
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.
43. Page 421, line 1: Change “respiration” to “CO_2 flux”, since it was NEE rather than respiration that was measured.
44. Page 421, line 3-7: This is the first mention of this result and therefore should be first presented in the results section.
45. Page 419-421, discussion of CO_2 flux results: There is no discussion of why none of the correlates with CO_2 flux from the gradient method were found to correlate with the EC method of CO_2 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.
46. Page 421, line 8: Methane uptake is stated to be “as large as” -0.14 nmol CH_4 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.
47. Page 421, line 8-12: The comparison of methane flux rates with previous study is contradictory. A measured rate of -0.14 nmol CH_4 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.
48. Page 421, lines 8-14: Were the studies referenced to compare methane flux rates also done in winter? Please clarify.

49. Page 421, line 15: Is 0.4 m^3 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?
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.
51. Page 421, line 21-23: I think an R^2 of 0.43 for the relationship between CH_4 flux and soil temperature is moderate, not weak.
52. Page 421, discussion of CH_4 flux: SWE showed the most significant relationship with weekly CH_4 flux and was used to extrapolate the CH_4 flux measurements throughout the season, but potential mechanisms and the ecological significance behind this relationship are not discussed.
53. Page 422, line 1-3: How would distance to river/stream or slope drive spatial variability in CH_4 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 CO_2 flux?
54. Page 422, lines 4-9: Were the comparable studies done during winter?
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 N_2O flux by soil moisture? If still under investigation, say so.
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.
57. Page 423, line 13: How might this manure application have affected the other fluxes (spatially, temporally, and budget contribution)?
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?
59. Page 423, Section 4.2: There is no discussion of the results from different land uses. This is needed.
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 CO_2, CH_4, and N_2O fluxes, but is not really done for the other major goals of the paper.
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.
62. Page 423, lines 22-23: The statement that CH_4 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.
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 CO_2 and CH_4 fluxes showed the greatest response to temperature (other than SWE), this has bearing on expected future emissions.

64. Page 434, Table 1: Include the depth of soil temperature measurements.
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.
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.
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.
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.
69. Page 439, Figure 3 caption: Specify what the error bars indicate.
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.
71. Page 443, Figure 7 caption: How were concentrations in the snow profile interpolated between measurement points? Same as that state in Fig. 8?
72. Page 444, Figure 8: To save space, the two panels could probably be overlaid as in Fig. 7.

Technical Corrections

73. Page 404, line 26: Change "Much less" to "Far fewer".
74. Page 405, line 10: Change "seems to" to "has been documented to"
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.
76. Page 405, line 18 – I recommend changing the comma in "emissions, the instantaneous" to a semi-colon.
77. Page 405 – "type" should be "types".
78. Page 410, line 24: Change "such as" to "including".
79. Page 410, line 25: Insert a comma between the closing parenthesis and "soil temperature".
80. Page 411, line 12: "than" should be "then".
81. Page 411, line 19: "distinctly" seems to be the wrong word here. "Considerably"?
82. Page 412, line 26: Insert "The" before "largest uptake rates…"
83. Page 414, line 7: Replace "annual" with "seasonal"
84. Page 414, line 19: Replace "cumulated" with "cumulative"

85. Page 415, line 4-5: Use consistent tense. Change “have chosen” to “chose”.
86. Page 415, line 26: Change “mean fluxes” to “a mean flux”.
87. Page 415, line 27: “Fig. 6c” should read “Fig. 7c”.
88. Page 416, line 6: Change “considerable” to “considerably”.
89. Page 416, line 19: Insert “for the gradient method” between “underestimation” and “during”
90. Page 416, line 22: Change “to quantify” to “quantification of”.
91. Page 416, line 22: Remove “so-called”.
92. Page 417, line 2: Change “than” to “as”
93. Page 417, line 3: Add “, respectively” after the values in parentheses.
94. Page 417, line 4: Change “results” to “magnitudes”.
95. Page 417, line 5-7: The first sentence of this paragraph is awkward. Reword.
96. Page 418, line 13: “year” should read “winter”, and “Table 3” should read “Table 2”.
97. Page 419, line 20: Change “methods underestimates” to “method underestimated”.
98. Page 420, line 3: Change “clearly” to “clear”.
99. Page 420, line 5: Change “Mountain” to “Mountains”.
100. Page 420, line 12: “Fig. 2e” should be “Fig. 2c”.
101. Page 420, line 13: Remove “period”.
102. Page 420, line 22: Change “and the according” to “do to the according”.
103. Page 420, line 26: Insert “temporal” between “the” and “variability”.
104. Page 423, line 21: Change “the contribution of … was minor” to “the contributions of … were minor”.
105. Page 436, Table 3 caption: “winter 2012/2011” should read “winter 2010/2011”.
106. Figures 1, 2, 3, 5, & 7: These figures need larger fonts for axes values, labels, and legends.
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”.
108. Page 444, Figure 8 caption: “Dischmavalley” should be two words.