

## ***Interactive comment on “Winter greenhouse gas emissions (CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O) from a sub-alpine grassland” by L. Merbold et al.***

### **Anonymous Referee #4**

Received and published: 6 February 2013

This manuscript details measurements of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O fluxes during the winter 2010/2011 at a sub-alpine managed grassland in the Dischma valley of Switzerland. As was noted in the manuscript, there is currently a dearth of studies that have been conducted on winter respiration in high latitude and altitude systems. As such, this manuscript can be of much interest to the scientific community. As well, the concurrent analysis of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O is relatively novel for this biogeographic region, and adds much scientific merit to this work. However, before publication considerable revision is required, especially in regards to proper English grammar and syntax. Additionally, the authors would do well to further clarify certain questions about their methodology and experimental set-up. In deference to previous reviews, I have only listed the most substantial concerns and revisions as I have seen fit. Nevertheless, the authors should take care to consider the comments and amendments listed below

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before final publication.

Specific Comments: 1. Page 404: lines 26-27. Which other GHG gases outside of CO<sub>2</sub>, CH<sub>4</sub> and NO<sub>2</sub> are you referring to? More specificity is needed to validate the claim that these omissions would alter the ecosystem carbon balance 2. Page 405: lines 9-15. Although it is noted once later in the manuscript, it is important to stress the difference in the flux footprint and spatial coverage of EC versus chamber and diffusive soil respiration measurements. These differences could confound any estimation and analysis of respiration rates between the different methodologies. Was any EC footprint analysis completed? 3. Page 405: lines 9-14. It is stated that the gradient method seems to underestimate CO<sub>2</sub> fluxes, but only one supporting study is mentioned, and yet a further mentioned study (Schindlbacher et al. 2007) contradicts this claim. If there is no true consensus then the original phrasing should be amended accordingly. 4. Page 405: lines 19-21. Consider breaking up subsection (ii) into two separate sections, with a separate subsection (iii) under the phrase 'to identify the variables driving GHG emissions from different land-use type(s) in a subalpine valley.' 5. Page 406; lines 18-22. Consider merging this entire paragraph into section 2.1. 6. Page 406: lines 20-22. Although the automatic gradient method did not gather much useable data it should still be mentioned in this section of the manuscript. 7. Page 407: lines 20-22. Were these coordinate and axis rotation corrections then also applied before the final flux calculation, as the current language does not actually state if they were applied. 8. Page 407: lines 23-25. How much data was filtered out due to low friction velocities? Consider noting here or in the results the amount of data that was filtered out in post-processing or the total extent of gap-filling that was applied to this data set. 9. Page 408: lines 2-3. The time scale is uncertain. Was there only one manual measurement taken once a week or was there intensive data collection once a week. The latter is presumed but more detail is needed. 10. Page 409: lines 1-14. For the instantaneous measurement did the ski pole method occur at the same depth and puncture point each time in the snow layer? If so, would this introduce advective effects and disturb the diffusive transport of GHGs in the snow layer? Do you have

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other data or publications to suggest that this novel method does not significantly alter the diffusive gradient? 11. Page 409: line 15. Consider creating a new section title here (2.4) to differentiate the preceding manual flux methodology from the automated flux measurements. 12. Page 409: lines 23-25. How often did these 'preferable' periods of snow compaction actually occur for tube layer set-up? 13. Page 411: line 9. The predefined volume used to measure snow density should be stated. 14. Page 411: line 11. Data on the depth and thickness of the ice layers should be supplied in the manuscript or this sentence should be considered for removal. 15. Page 411: lines 5-6. How close is this meteorological tower to the research site? Be more specific if possible. 16. Page 413: line 8. Was air temperature ( $T_a$ ) estimated from the sonic air temperature or from the adjacent meteorological station? 17. Page 413: lines 19-20. How many weekly gaps occurred during the measurement period? 18. Page 414: lines 1-2. Does the prior noted fetch/flux footprint issue impact these noted deviations? 19. Page 414: lines 12-14. Backwards extrapolation of N<sub>20</sub> fluxes before the measurement period needs further justification. Is there good reason to suggest that this procedure will impart correct fluxes? 20. Page 416: lines 13-18. This section seems better placed in the introduction of the paper. 21. Page 418: lines 22-29. This section also seems better placed in the introduction of the paper. 22. Page 419: lines 14-25. Perhaps consider integrating this paragraph into the methodology section. It seems out of place at the end of this section of the discussion, and its removal or transfer could improve the overall 'flow' of the manuscript. 23. Page 420: line 12. The referenced figure notes snow density not soil water content. This should be amended to a reference to Fig. 2c. 24. Page 424: lines 1-2. This statement is somewhat incongruous and should be removed. The narrow characterization of winter emissions by the period of snow cover versus a more appropriate calendric definition is problematic and should not be used in evaluations of seasonal contributions to annual budgets. 25. Page 434: Table 1. Are these variables truly the most important? One could argue that snow density (which is used in this paper to defined the final diffusivity coefficient) is just as important or more important than the mean snow height and monthly snowfall. 26. Page 438: Figure 2.

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Consider limiting the time range of meteorological variables to the active measurement period (such as in Fig. 2e). 27. Page 439: Figure 3. Including the month of November in this figure seems unnecessary, and the figure could be refitted to periods of active measurement only for a better presentation. As permanent snow cover overlaps for the entirety of this period grey shading may also be unnecessary.

Technical Corrections: 28. Page 402: line 9. Change to “the progressing.” 29. Page 402: line 22. Change “according” to “variable.” 30. Page 404: line 5. Change to “wetlands and ruminant husbandry are the major CH<sub>4</sub> sources...” 31. Page 404: line 13. Change “system” to “systems.” 32. Page 404: line 18. Change to “only a few studies on sub-alpine grasslands have investigated...” 33. Page 404: line 26. Change “Much less studies” to “Even fewer studies.” 34. Page 405: lines 4-5. Change to “methodological challenge, as many...” 35. Page 408: line 12. The flux rate of CH<sub>4</sub> should be rewritten. The assumed correction is nmol CH<sub>4</sub> m<sup>-2</sup>s<sup>-1</sup>. 36. Page 408: line 14. Change “turtuosity” to “tortuosity.” 37. Page 409: line 11. Change to “was carried out a few hours later...” 38. Page 411: line 12. Change “than” to “then” 39. Page 411: line 16: Change to “snow conditions” 40. Page 411: line 19,20,25. Remove the definite article “the” before each calendric entry, such as “the 27 December.” 41. Page 412: line 7, 15. See Note 40. 42. Page 415: line 4-5. Change “we have chosen” to “we chose.” 43. Page 416: line 6. Change “considerable” to “considerably.” 44. Page 417: line 8. Change “adjective” to “advective.” 45. Page 423: line 3. Change to “fluxes had only a minor influence...” 46. Page 424: lines 10-14. The term “Last but not least” is utilized twice. Consider a change of term. 47. Page 437: Fig. 1. Change “Dischmavalley” to “Dischma valley” and automatically gradient measurements” to “automatic gradient measurements.” 48. Page 438: Figure 2(e). There is a contradiction between the figure label (gcm-3) and the y-axis (gm-3). 49. Page 442: Fig. 6. Change to “222Rn measurements were incorrect.” 50. Page 444: Fig. 8. Change “Dischmavalley” to “Dischma valley.”

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