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 4:
This manuscript details measurements of CO2, CH4, and N2O 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 CO2, CH4, and N2O 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 before final publication.

Specific Comments:
1. Page 404: lines 26-27. Which other GHG gases outside of CO2, CH4 and NO2 are you referring to? More specificity is needed to validate the claim that these omissions would alter the ecosystem carbon balance

We adjusted this in the revised version of the manuscript according to the suggestion made by reviewer 4. With other GHG's we only refer to CH4 and N2O in this manuscript.

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?

We agree with reviewer 4 on the different spatial scales covered by different techniques. Even though we only applied the gradient and the EC technique we stressed the important scale issue in the revised version of the manuscript. Furthermore we included the footprint, calculated after Kljun et al. 2004 in Figure 1.

3. Page 405: lines 9-14. It is stated that the gradient method seems to underestimate C02 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.

We adjusted the revised version of the manuscript to: "The gradient method has been documented to underestimate fluxes compared to the EC method (Suzuki et al., 2006) as well as compared to the chamber method (Mariko et al., 2000). In contrast, *Schindlbacher et al. (2007) observed that the chamber method underestimated CO_2 fluxes in comparison to the gradient method and McDowell et al. (2000) found no difference between the chamber and the gradient method at three sites in the Rocky Mountains."*

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.'

Done.
"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 ecosystems. 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."

5. Page 406; lines 18-22. Consider merging this entire paragraph into section 2.1.

Done.

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.

Done.

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.

This applies to the modified Burba correction and was applied to our data.

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.

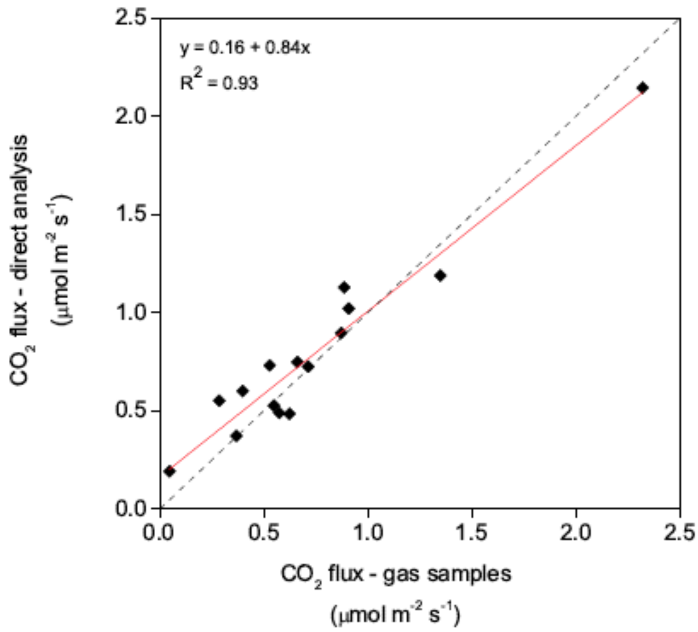
In order to avoid another table we included, measured and gap-filled data in Figure 4. Post-processing/data filtering commonly results in the loss of large amounts of data. In our case and under the specific conditions of two sonic anemometers which stopped working under the challenging winter conditions we remained on average with 21% high quality data per month.

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.

We changed on a weekly basis to "per week". For further clarification, all profiles were measured every single week.

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 other data or publications to suggest that this novel method does not significantly alter the diffusive gradient?

We are not sure if we understand the comment by reviewer 4 correctly. The ski pole was inserted 10cm in the snow and the sample was collected, thereafter the pole was pushed deeper for the collection of the next sample. The puncture points changed each sampling date, because the ski pole was inserted at another location.
If such advective effects were occurring we would further have seen this in possible nonlinearities in the concentration gradient, which were only rarely the case and not associated to ice-layers as stated previously. We further would have detected large differences between the online CO2 measurements using the IRGA and the analysis via gas chromatography, which was not the case. Further, the ski pole method has been used previously in a Master thesis (Wetter 2009).



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.

We tried to further disentangle both approaches in the revised version of the manuscript without creating a new section due to the comments of previous reviewers.

12. Page 409: lines 23-25. How often did these 'preferable' periods of snow compaction actually occur for tube layer set-up?

Such snow compaction I most common during a short warming after snowfall, see also Figure 2a and f for details.

13. Page 411: line 9. The predefined volume used to measure snow density should be

stated.

Done.

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.

We removed this in the revised version of the manuscript.

15. Page 411: lines 5-6. How close is this meteorological tower to the research site? Be more specific if possible.

The distance is few meters east of the eddy covariance tower, please see also Figure 1c.

16. Page 413: line 8. Was air temperature (Ta) estimated from the sonic air temperature or from the adjacent meteorological station?

Air temperature was estimated from the adjacent meteorological station.

17. Page 413: lines 19-20. How many weekly gaps occurred during the measurement period?

Concentration measurements were undertaken once per week, resulting in gaps each week.

18. Page 414: lines 1-2. Does the prior noted fetch/flux footprint issue impact these noted deviations?

This is not the case. Therefore we included the flux footprint in Figure 1b of the revised manuscript.

19. Page 414: lines 12-14. Backwards extrapolation of N20 fluxes before the measurement period needs further justification. Is there good reason to suggest that this procedure will impart correct fluxes?

We rejected this backwards extrapolation in the revised manuscript to avoid stating incorrect fluxes. Therefore seasonal budget were only calculated from 1st of Dec – 31st of Mar. in the new version of the manuscript.

20. Page 416: lines 13-18. This section seems better placed in the introduction of the paper and 21. Page 418: lines 22-29. This section also seems better placed in the introduction of the paper.

We would like to thank reviewer 4 for these comments but kindly reject this suggestion since the gradient method has been a core method in this study and one of our specific objectives was to study the differences between methods. Therefore we believe this discussion is needed to explain the reported deviations.

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.

We would like to thank reviewer 4 for this helpful comment and moved this part of the discussion to the M&M paragraph.

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.

Done.

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.

Done. We rephrased large parts of the conclusion in the revised version of the manuscript.

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.

We apologize for this inaccurate statement "most important" and changed this to "basic". The table was given to have a direct overview of the single months. In addition we show environmental variables such as snow density, snow water equivalent etc in Figure 2, which was extended in the new version of the manuscript.

26. Page 438: Figure 2. Consider limiting the time range of meteorological variables to the active measurement period (such as in Fig. 2e).

We disagree with reviewer 4, since we believe that the whole part of the winter season is important. However we clearly highlight the period of continuous snow cover in the revised manuscript.

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.
We included both November and the snow covered period to better visualize the time period of available data in comparison to the winter season 2010/2011.

Technical Corrections:
28. Page 402: line 9. Change to "the progressing."

Done.

29. Page 402: line 22. Change "according" to "variable."

This sentence has been deleted in the revised manuscript.

30. Page 404: line 5. Change to “wetlands and ruminant husbandry are the major CH4 sources. . .”

Done.

31. Page 404: line13. Change “system” to “systems.”

Done.

32. Page 404: line 18. Change to “only a few studies on sub-alpine grasslands have investigated...”

Done.

33. Page 404: line 26. Change “Much less studies” to “Even fewer studies.”

This part was rephrased in the revised manuscript.

34. Page 405: lines 4-5. Change to “methodological challenge, as many. . .”

Done.

35. Page 408: line 12. The flux rate of CH4 should be rewritten. The assumed correction is nmol CH4 m-2s-1.

Done.

36. Page 408: line 14. Change “turtuosity” to “tortuosity.”

Done.

37. Page 409: line 11. Change to “was carried out a few hours later. . .”

Done.

38. Page 411: line 12. Change “than” to “then”

Done.

39. Page 411: line 16: Change to “snow conditions”

Done.

40. Page 411: line 19,20,25. Remove the definite article “the” before each calendric entry, such as “the 27 December.”

Done.

41. Page 412: line 7, 15. See Note 40.

Done.

42. Page 415: line 4-5. Change “we have chosen” to “we chose.”

This part was rephrased in the revised manuscript.

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

Done.

44. Page 417: line 8. Change “adjective” to “advective.”

Done.

45. Page 423: line 3. Change to “fluxes had only a minor influence. . .”

Done.

46. Page 424: lines 10-14. The term “Last but not least” is utilized twice. Consider a change of term.

Done.

47. Page 437: Fig. 1. Change “Dischmavalley” to “Dischma valley” and automatically gradient measurements” to “automatic gradient measurements.”

Done.

48. Page 438: Figure 2(e). There is a contradiction between the figure label (gcm-3) and the y-axis (gm-3).

Done.

49. Page 442: Fig. 6. Change to “222Rn measurements were incorrect.”

Done.

50. Page 444: Fig. 8. Change “Dischmavalley” to “Dischma valley.”

Done.