We thank the reviewer for her/his thorough review and address the concerns raised point by point. Our replies are given below each point and numbered as 'R1' to 'R16'.

- The discussion could be a bit more balanced to include the possible importance of nitrification as a source of N2O in these soils. These sites have very high atmospheric deposition of ammonia and high nitrate levels, so there must be high rates of nitrification, and there is likely a significant amount of N2O production associated with that nitrification.

R1. The discussion has been changed and more emphasis is given on the nitrification as a potential source for N2O emission (see also R 4).

- Another area that needs work is the discussion about IPCC emission factors which should be clarified and expanded.

R2. The discussion on emission factors of subtropical forests on deposited nitrogen has been expanded (see also R.16).

1. The English grammar and usage need a final editing. For the most part, they are fine, but there are numerous small errors that need to corrected, e.g., the comma at the end of line 6 on page 14948 needs to be removed. Another example is inconsistent reference to the sampling sites; "on HS" or "in GDZ."

R3: We worked through the text and revised the grammar.

2. Page 14948, line 8. It might be worthwhile to mention that while low pH increases the N2O yield of denitrification, it can also decrease rates of nitrification, an additional potential source of N2O.

R4: We follow the reviewer and now elaborate on nitrification as a possible source for N_2O both in the introduction and the discussion. However, source apportionment of N_2O was not the primary objective of the present study. We address source apportionment in a separate study using ^{15}N *in situ* labeling (Zhu et al. submitted) in which we present support for the importance of denitrification as a source of N_2O at TSP. The main points of the present study are temporal and special dynamics of N_2O emissions and their up-scaling within the watershed. The effect of acidity on nitrification is still unclear. On the one hand, nitrification rates in acid soil may be very low, which may result in a low N_2O production potential by nitrification; on the other hand, low soil pH has been reported to increase N_2O/NO_3^- product ratio of nitrification, as noted by the reviewer, which may lead to increased N_2O production via nitrification. We now address the uncertainty arising from the adverse effects of acidity on potential N_2O formation by nitrification both in the Introduction section (line 81-87) and in the Discussion section (line 544-552).

3. Page 14949, lines 5 - 12. The statement of hypotheses could be improved. It might be useful to state that you hypothesize that denitrification is the dominant source of N2O in these

soils and that therefore you expect rates of emission to be higher in the wetter soils. This is a somewhat surprising hypothesis however as these soils receive high rates of atmospheric deposition of ammonia and appear to have high rates of nitrification. So the reader will want to know why you don't think that nitrification is an important/dominant source of N2O. Also it is not clear just what you are saying about the regression model. If developing this model was an objective of the work, with a specific hypothesis, this needs to be stated more clearly.

R5: The data presented in the current study do not allow us to conclude whether nitrification or denitrification is the major process of N_2O formation and therefore we do not have an explicit hypothesis on this. We rephrased the hypothesis, though, to make it clear that the emphasis of the present study lies on the temporal and spatial variability of the N_2O emission strength (line 118-124).

Regression models (PCA, ordinary linear correlations) were used primarily descriptively, in an attempt to find the major controlling factors for N2O emissions in the studied watershed. We agree that the interpretation of our results is somewhat biased towards denitrification, reflecting the fact that we found spatial patterns of denitrification potentials and inherent denitrification stoichiometries in a laboratory study (Zhu et al., 2013) which were congruent to the spatial pattern of N_2O emissions reported here. Further, we gained strong evidence for denitrification as a major N_2O source in an in situ labeling experiment conducted at two positions on the hillslope during summer 2010 (Zhu et al., submitted). Both experiments are now invoked in more detail in the discussion of the flux data.

4. Page 14951, line 12. It might be useful to include the total number of flux measurements that were made over the course of the entire study.

R6: done. Line 184-185.

5. Page 14951, line 14. Were the chamber bases inserted into the soil each time or were they permanently installed? If they were inserted each time, how much time elapsed between insertion and the measurements?

R7: There were no chamber bases; chamber was inserted directly into the soil. The first sample was taken right after the installment of chamber. This may result in some bias, as also was pointed out by reviewer 1. We have discussed this aspect in the text. See R3 to reviewer 1# and manuscript line 188-194.

6. Page 14952, lines 17 – 22. This text is Results and/or Discussion and should not be included here in the Methods section.

R8: This part is the follow up of the explanation of IDR (data from another experiment published elsewhere) and an attempt to explain why we included only data from the upper soil layers into the PCA presented here. Please see R6 for Reviewer 1# for further explanation. We simplified the text (line 232-234).

7. Page 14956, lines 12 – 27. You either need to present some statistical analysis to support the statements about differences between seasons, years, and sites or give some reason why you are not presenting statistical analysis.

R9: The original text was giving purely descriptive information about the spatial and temporal patterns of N_2O flux. We have changed the text by adding statistics.

8. Page 14959, lines 17 – 25. This presentation of annual N2O flux values may be the most important section of the paper. Using one winter period to produce annual estimates for two different years is confusing and of dubious validity. Is there some way to make some assumptions about soil conditions and N2O flux during the winter of 2010/2011 so that you can produce two genuine estimates of annual flux for two genuinely different years? Also, were the differences in annual flux for the two years significantly different? And why were the differences so small given the much larger fluxes during summer 2009 than 2010?

R10: Following the referee's suggestion, we applied the regression model for N_2O flux rates driven by soil temperature and soil moisture and estimated the N_2O flux for the winter of 2010/2011. As mentioned in the text, N_2O fluxes from Nov. 2009 to Mar. 2010 accounted for only 11.5% of total annual N_2O flux. Similarly low N_2O fluxes were estimated for the winter of 2010/2011. Further explanations are presented in the text (line 450-452). In this way we now present annual fluxes for the two genuinely different years.

The differences between the fluxes in the two years are presented in detail in the Result (line 445-448) and Discussion section (line 574-577).

9. Page 14960, line 8 though page 14961, line 9. The English grammar and usage in this paragraph need work, there are several errors, e.g., "cumulated N_2O flux," forests soils," "neither of the two landscape elements."

R11: done.

10. Page 14961, lines 7 – 9. I think this discussion of denitrification should be deleted here and augmented later (see comments below).

R12: We follow the reviewer's suggestion and deleted the sentence; the possibility of N_2O production via denitrification and nitrification is now discussed later in the paragraph starting at line 536.

11. Page 14961, line 20. "relief" should be "relieve."

R13: done.

12. Page 14961, lines 25 – 27. This sentence contradicts the first sentence of this paragraph and

seems to reflect a bit of a bias on the part of the authors that denitrification is producing all the N2O at these sites.

R14: This sentence was moved to the beginning of the paragraph and rephrased (line 491-494). The possibility of N_2O production via denitrification and nitrification is discussed later in the paragraph starting at line 536.

13. Page 14962, line 25 to page 14963, line 8. I think this discussion should be a bit more balanced to include the possible importance of nitrification as a source of N2O in these soils. These sites have very high atmospheric deposition of ammonia and high nitrate levels, so there must be high rates of nitrification, and there is likely a significant amount of N2O production associated with that nitrification. It is a bit misleading to suggest that "nitrification rates in the acidic forest soils of SW China are low" when these sites clearly have high nitrification rates. The authors are probably right, and they have significant ancillary evidence that denitrification is the dominant source of N2O in these soils, but the discussion should be more balanced.

R15: We extended the discussion to make the statement that "even though the fate of the deposited NH_4^+ remains unclear in this catchment, nitrification is not the main source of the high N_2O emission observed in this study." (line 550-552).

14. Page 14964, lines 7 – 9. This is a very interesting observation about "emission factors" that could use some more development. You should explain briefly just what the IPCC Tier 1 factor is. And you should mention that there are factors for cultivated land, but also for atmospheric deposition to uncultivated land. Can you compare your results to other studies, e.g., Hefting's work?

R16: Following the reviewer's suggestion, the IPCC Tier 1 emission factor is now explained in the text and compared with emission factors reported in other forest studies (line 588-591).