

## ***Interactive comment on “Spatial and temporal variability of N<sub>2</sub>O emissions in a subtropical forest catchment in China” by J. Zhu et al.***

**Anonymous Referee #2**

Received and published: 28 December 2012

The manuscript presents a very nice dataset on nitrous oxide fluxes from an understudied ecosystem type that appears to have very high rates of flux. The experimental design is strong, with two distinct landscape positions sampled and multiple sample dates across very different seasons for nearly two years. The results are novel and important, increasing our understanding of ecosystem and landscape scale controls on nitrous oxide flux and adding an important missing piece to the database on emissions of this important gas.

The paper would benefit from some revision however. The discussion could be a bit more balanced to include the possible importance of nitrification as a source of N<sub>2</sub>O 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

C6914

significant amount of N<sub>2</sub>O production associated with that nitrification. Another area that needs work is the discussion about IPCC emission factors which should be clarified and expanded.

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 be 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.”
2. Page 14948, line 8. It might be worthwhile to mention that while low pH increases the N<sub>2</sub>O yield of denitrification, it can also decrease rates of nitrification, an additional potential source of N<sub>2</sub>O.
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 N<sub>2</sub>O 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 N<sub>2</sub>O. 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.
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.
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?
6. Page 14952, lines 17 – 22. This text is Results and/or Discussion and should not be included here in the Methods section.

C6915

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.
8. Page 14959, lines 17 – 25. This presentation of annual N<sub>2</sub>O 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 N<sub>2</sub>O 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?
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<sub>2</sub>O flux,” forests soils,” “neither of the two landscape elements.”
10. Page 14961, lines 7 – 9. I think this discussion of denitrification should be deleted here and augmented later (see comments below).
11. Page 14961, line 20. “relief” should be “relieve.”
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 N<sub>2</sub>O at these sites.
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 N<sub>2</sub>O 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 N<sub>2</sub>O 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

C6916

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 N<sub>2</sub>O in these soils, but the discussion should be more balanced.

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?

---

Interactive comment on Biogeosciences Discuss., 9, 14945, 2012.

C6917