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 'R39'.

- P14949/L16: Please define 'TSP'.
R1: done (line 132).

- P14950/L8-11: Since there is no data underlying this statement please rephrase (e.g. the turnover rate … is 'likely' high), or turn the sentence around (i.e. starting with the result that little OM is accumulated, and continue from there (e.g. 'which might indicate that…').
R2: done (line 150-153).

- P14951/L13-15: This sounds as if the chamber bases were always inserted into the ground just before measurement, is that the case? This may introduce bias in the measured gas fluxes due to soil disturbance. Please expand on this possibility in the methods.
R3: No chamber bases were used in this study. Given the clayey soil surface with little roots and mostly high soil moisture, we found it more appropriate (i.e. less disturbing) to insert chambers directly into the soil (2-3 cm deep). Permanently installed chamber bases may impair surface runoff and interfow on the steep hillslope, thus we considered it advantageous to avoid permanently installed structures. Chambers were deployed by carefully pressing them 2 cm into the slid of the previous deployment. Disturbance was minor as judged from the observation that the initial concentrations of N_2O in the chamber (at 1 min) were close to ambient air and the chambers were tight as judged from an initially linear increase in CO_2 concentration within the chamber (data not shown). The text has been changed accordingly to explain this in line 187-194.

- P14951/L21-23: The gas samples were shipped to Norway for gas chromatographic analysis. How long was the time delay between sampling and analysis? Were the butyl septa tight for this time of storage, such that the possibility of biased results can be excluded? Did you test for this? Please expand on this.
R4: The vials were analyzed within half a year time after sampling. Test on blanks carried out during previous projects involving air freight and prolonged storage of samples had shown that deviations were within the accuracy of the GC (<1% for N_2O) when shipping vials pressurized. Note that we are using Chromacol butyl septa on 10 cc vials which have been found to perform better than Labco-exetainer used by others. During sampling, 20 ml gas was injected into a 12-ml vial, resulting in overpressure. The overpressure endured transport and storage, suggesting that the septa kept tight.

- P14951/L28-31: The choice of the mathematical model used to calculate the gas fluxes is crucial. How did you decide for each flux whether to use a linear or a second order polynomial fit? Has an objective criterion been applied to make this choice? Please be more specific in this important aspect!
R5: The phenomenon of second order polynomial increase of N_2O concentration was probably because of the diffusion inhibition due to N_2O accumulation during chamber deployment (yielding a negative quadratic term in the polynomial) or due to chamber heating (yielding a positive quadratic term). In any case, we were interested in the "initial" flux shortly after

chamber deployment and therefore inspected each single flux measurement manually by plotting concentration against deployment time. Steady increase in CO_2 mixing ratio was used as a quality criterion to prevent overfitting of "bad" N_2O data. Further, both linear and second order polynomial regression was executed and the R-square values were compared. Some explanation has been added in line 210-213.

- P14952/L19-22: I strongly suggest to also include the instantaneous denitrification rates of the deeper soil layers, and not to exclude them just because they were low. This is an important piece of information.

R6: After revisiting the data on instantaneous denitrification rates in deeper soil layers (Zhu et al., 2013) as suggested by the reviewer, we argue that including these data would not change the outcome of the PCA, but rather confound it. The study on instantaneous denitrification rates and *ex situ* potentials for N_2O loss (Zhu et al., 2013) was conducted to explore which landscape elements and soil layers would likely contribute to measured N_2O flux, and the top soils (O and A horizons) on the hillslope were found to clearly have the highest potential both with respect to denitrification and inherent N_2O/N_2 ratio. In contrast, the deeper soil layers showed low activity and most notably low variation among the plots (decreasing standard error with depth in fig. 6 of Zhu et al., 2013). Thus, deeper soil layers are unlikely to contribute appreciably to the N_2O flux observed in situ and are therefore excluded from the PCA.

- P14953/L4-5: How long and under which conditions were the soil samples stored before the KCl extractions? Please expand on this.

R7: Storage conditions of samples used for KCl extraction are described now in more detail in lines 243-246. See also R14.

- P14953/L29: Please insert a reference for the assumed soil particle density, e.g. (Linn & Doran, 1984).

R8: done.

- P14954/L16-18 and Fig. 8: The minus reciprocal is a rather uncommon transformation, and a sinusoidal transformation very uncommon. Why did you choose such uncommon transformations? What was the reason to transform the data? Unless there is a really argument promoting this choice I would strongly advise to stick to the common transformations used to transform right- or left-skewed datasets.

R9: The minus reciprocal and the sinusoidal transformation were applied to soil bulk density and NO_3^- concentration in soil water ($NO_{3\ sw}^-$), respectively. These two parameters were neither right- or left-skewed but bimodal. We first tried common transformations such as natural logarithmic and exponential transformations. Neither could normalize the datasets. Therefore we turned to the uncommon ones, which gave much better results.

- Sect. 2.3: Please check throughout that you give the units for all used variables and parameters, e.g. missing for VM and VPD.

R10: The units for VM and VPD are cm^3/cm^3 and kPa, respectively. The units have been added to the text.

- P14958/L14ff: Are these the results of the multiple linear regression analyses, or are these linear regression results? Please specify. If these are linear regression results please add this method in your statistical methods description.

R11: The results are from stepwise multiple linear regressions as indicated in the text. And the method has been described in the Materials and Method section on line 293-299.

- P14958/L24-25: Here and for all other regression equations, please include a measure of variance of the parameter estimates, e.g. confidence intervals. It is not explained what 'R-S(adj)' means, please add.

R12: Done (line 417, 426 and 428).

- P14960/L1-7: It seems that you compare your maximally observed flux rate with mean values over longer time periods from the literature? If so I don't think that this is adequate, please reconsider. Mean flux rates of less than 11 μg N_2O-N m^{-2} h^{-1} were also reported in the literature, see e.g. the compilation of soil N2O fluxes for montane forests in (Koehler et al., 2009).

R13: No, here we compare the reported highest observed flux rate from different studies. Some of them have been reported as in the text; the rest can be estimated from the figures. For instance, in Koehler et al. (2009), we read out the maximum flux rate from Fig. 4, the control treatment, which had the highest flux rate of about 4-5 ng N_2O-N cm^{-2} hr^{-1}, equivalent to 40-50 μg N_2O-N m^{-2} hr^{-1}.

- P14960/L22-23: Storage of tropical soil samples before extraction of the extractable nitrogen can severely bias the results, and may e.g. artificially inflate the nitrate concentrations compared to in-situ extracted soil samples (Arnold et al., 2008; Turner & Tania, 2009). From the current methods description it is not clear how long the soil samples were stored before extraction, and if this may have biased the results. Please expand on this in the methods section!

R14: We acknowledge the concern of the reviewer. The storage condition of our soil samples has been described in the Material and Method section (see R7). According to the study of Turner & Tania (2009), who analyzed inorganic N fractions in acidic soils (0-10 cm depth) from tropical rainforest, freezing may increase the amount of 2M KCl-extractable NH_4^+ (after 4 weeks storage) and decrease extractable NO_3^- to 1/15 (after 3 months storage). This bias may have occurred in our samples, which should have led to an underestimation of NO_3^- concentrations. However, comparing observations of the soil solution at our site (Larssen et al. 2011) with those reported in other studies (Foster et al. 2005, McSwiney et al. 2001) leaves little doubt that the soils at TSP are rich in NO_3^- and we therefore do not believe that high values of extractable NO_3^- are a result of storage. Furthermore, we found a clear decrease in extractable mineral N along the hydrological flow path in the GDZ which matched the concentration pattern of NO_3^- in soil solution, suggesting that bias due to storage would not change the relative pattern in NO_3^- concentrations between HS and GDZ. Extractable NO_3^- data were used for PCA analysis only, and supported the distinction between HS and GDZ. Mineral N dynamics in time are based on concentration data in soil solution, which are less prone to bias by storage.

- P14960/L25-28: Please check this with an appropriate statistical analysis, i.e. an analysis for repeated measurements. Do the results remain the same? The same question arises e.g. on P14961/L10-12.

R15: As suggested, the data were reanalyzed using a general linear model (GLM) with mixed effects in Minitab 16.1.1 (Minitab Inc.) The GLM considered the effect of sampling dates, locations and the mixed-effect of both. The analysis confirmed that plots in different landscape elements (hillslope or GDZ) differed significantly from each other with respect of N_2O fluxes and NO_3^- concentrations in soil water. As for the two instances mentioned by the reviewer: 1) P14960/L25-28, the result remains the same that WFPS at 10 cm depth of T3 was significantly lower than that of B1; 2) P14961/L10-12, it actually refers to a simple correlation analysis for cumulative N_2O emissions and average NO_3^- concentration in stream water which we believe is proper.

- P14961/L20: Please include a reference for the statement that decreasing diffusion promotes N_2O-N_2 reduction.

R16: This sentence is not about that 'decreasing diffusion promotes N_2O-N_2 reduction', but that an increasing residence time of N_2O in soil with increasing ground water level likely increases the chance of N_2O being reduced to N_2. The statement has been rephrased to avoid misunderstanding (line 501-503).

- P14963/L8: Please give the exact P-value, not just 'P<0.05'.

R17: done (line 544).

- P14964/L4-6: The fluxes are even comparable to a tropical lowland forest in Panama which was experimentally N-enriched for 9-10 years (Koehler et al., 2009).

R18: Thanks! This information has been added into the text (line 582-583).

- Table 1: Please include a measure of variability, e.g. standard errors.

R19: For the measurements of TOC, TN, pH, as well as the incubation experiment for IDR and *ex situ* potential for N_2O loss, composite soil samples were taken at each plot and analyzed (Zhu et al. 2013). Therefore no standard error can be given for these parameters on a plot level. For bulk density, $NH_4^+{}_{ex}$ and $NO_3^-{}_{ex}$ as well as $NH_4^+{}_{sw}$ and $NO_3^-{}_{sw}$, the standard error has been added in the table.

- Fig. 3: How comes that WFPS does not clearly drop during the 'dry-cool season'? Why did WFPS in T3 drop so much during July-September 2010?

R20: We believe that even though precipitation is low during the dry-cool season, the evapo-transpiration is also very low. The air is actual quite moist and it is often foggy with the average of Vapor Pressure Deficit about -0.68. Therefore the WFPS is relatively at a 'base' level during the dry season. By contrast, in later summer, such as July-September 2010, both temperature and evapo-transpiration were very high (Fig.2 and Fig. 3b). The average of Vapor pressure deficit was -1.99 during this period. The dryer weather in late summer resulted in low WFPS values which increase only during brief periods after precipitation events.

- Fig. 5: This is not a nice representation of the data. I suggest to revise the figure, and present the data similar as for soil-extractable nitrate in Fig. 7.

R21: The figure has been revised according to the comment.

- Fig. 6: How is 'summer' defined? How comes summer was just ~1 month long in 2009, but more than 3 months long in 2010?

R22: The sampling frequency in 2009 was not high enough to obtain the cumulative N_2O emission rates of the whole summer based on measurements. Therefore, flux data obtained during an intensive sampling campaign of 27 days in summer 2009 were used predominately to explore the relationships between N_2O flux and soil parameters. For 2010, we changed the sampling strategy and sampled fluxes on a near-weekly basis throughout 106 days. Some explanation has been added in line 215-218.

- P14963/L26: I don't find this argument very logical. If you know how much of the area is covered by GDZ and you determined typical emissions, why not upscale accordingly?

R23: Estimating the annual N_2O flux rate from GDZ requires different methodology than estimating the annual emission from the hillslope. At GDZ, we did not measure soil temperature and WFPS; the N_2O flux was not very clearly related to ground water level for which we did not have continuous measurements either. Together, this makes annual estimates of N_2O emissions from the GDZ rather uncertain. Since the area of GDZ is only a very small fraction of the whole catchment with a clearly lower emission of N_2O as compared with the hillslope, the contribution to the annual N_2O flux from this catchment will be minor. Therefore, N_2O emissions from the GDZ were omitted from the up-scaling exercise.

TECHNICAL CORRECTIONS

- P14952/L12: There are two references for Zhu et al. 2012, please distinguish between them.

R24: Done. They are the same. One is redundant and has been deleted.

- P14953/L6: Please don't use acronyms that are not so common and may not be known to all readers, in this case please spell out what 'FIA' means.

R25: Done.

- P14954/L14: I suspect here is a typo 'for O and O', please correct.

R36: Done.

- P14955/L4: MLR is an unnecessary abbreviation, please spell out.

R37: Done.

- P14960/L16: Suggest to replace the 'as' with 'because'.

R38: Done.

- P14960/23: Please correct to 'non-limiting'

R39: Done.

ADITIONAL REFERENCES

Foster N, Spoelstra J, Hazlett P, Schiff S, Beall F, Creed I, David C. 2005. Heterogeneity in soil nitrogen within first-order forested catchments at the Turkey Lakes Watershed. Canadian Journal of Forest Research-Revue Canadienne De Recherche Forestiere 35: 797-805.

Larssen T, Duan L, Muder J. 2011. Deposition and leaching of sulfur, nitrogen and calcium in four forested catchments in China: implications for acidification. Environmental Science & Technology 45: 1192-1198.

McSwiney CP, McDowell WH, Keller M. 2001. Distribution of nitrous oxide and regulators of its production across a tropical rainforest catena in the Luquillo Experimental Forest, Puerto Rico. Biogeochemistry 56: 265-286.