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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Nitrous oxide emissions from soil of an African rain forest in Ghana" *by* S. Castaldi et al.

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Interactive comment on "Nitrous oxide emissions from soil of an African rain forest in Ghana" by S. Castaldi et al. - Bg-2012-500

Answer to comments by Anonymous Referee #1

Specific comments p. 16566, l. 19/20: It's not a concept that tropical rainforests constitute the strongest natural N2O source, but an assumption or hypothesis that needs to be tested.

Author: That is correct, the last two statements of the abstract should be deleted being hypotheses.

p. 16566, l. 20/21: ": : :most probably the strongest source of N2O in the African continent." This statement should not be made in the Abstract as it is not supported by the data presented. If the studied had covered a range of different ecosystems across Africa, then this statement might have been justified.

Author: correct, see the answer above.

p. 16569, l. 4/5: While changing the chamber positions every time soil gas efflux measurements were made, have you taken care of not placing the chambers at spots that had been affected by trampling during previous measurements, e.g. by establishing walkways?

Authors: We defined areas of sampling and left on the ground pieces of plastic stick during the campaigns.

p. 16569, l. 7: Inserting the chamber collars into the soil only 3 h prior to the measurements is a pretty short time. The effect of root mortality and decomposition on soil CO2 fluxes will not have vanished after 3 h, but more after 3 weeks or even 3 months. A better reasoning could be that 3 h is a time period after which the major impact of pressure changes in the soil due to insertion of the collar have more or less disappeared, while fine root decomposition has not yet had a chance to develop and to have an effect on soil CO2 efflux.

Author: The sentence we wrote" taking care to insert the chamber base in the ground about 3 h before starting the measurements so as to avoid false fluxes caused by soil pressure changes and by root mortality and decomposition (Keller et al., 2000)," means exactly what the referee explains, i.e. that inserting the chamber 3 hours before analysis allows you to avoid effect of pressure variations which occur within the first 1-2 hours and effect of decomposition of killed roots and soil disturbance which can start in tropics quite quickly and last for some weeks.

p. 16569, l. 19/20: It is hard to believe that the maximum daily variation of soil tem-

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perature was only 0.5_C at 5 cm depth, and even only 0.1_C at 10 cm depth. I would expect a much higher variation between day and night.

Author: What we report is the maximum daily variation of soil monthly mean temperature, so it is the difference between the maximum and minimum value that soil temperature can have, averaging hourly values over a month. Thus being a difference of averages it smoothes extreme values. However, the rain forest has a very close canopy and exception made for few spots of light there is no direct light on the ground. The temperature under the canopy is quite stable. At 2 meters above ground variations are in the order of 2°C (meteo station of the eddy covariance tower).

p. 16569, l. 28f: Sampling 3x 30 ml from a chamber volume of approx. 3500 ml would induce a pressure drop of 2500 Pa if not a venting tube was used. Pressure changes of less than 1 Pa (!) have been shown to have a measurable effect on soil respiration measurements. Have you used a venting tube or a similar device to avoid pressure drops in your chambers?

Author: samples are 20 ml, so 1/3 less than calculated. This however can still induce some bias. We have made some tests using bags applied to the chamber, which can follow variation of volume and pressure which can occur in the chamber. However over a 1h interval we did not find statistical differences compared with no bag. We assume that the long incubation time, needed to have enough N2O accumulation, so to be above the background threshold, tends to mask variations of CO2 due to errors of the applied technique, with an overall underestimation of the flux anyway. As we have to measure N2O, which concentration is in ppt, we found quite complicated to calibrate the right venting tube diameter/length having great variability of concentrations over time and over space. The best option would be to have 2 different systems for gas analysis, one for CO2 and one for N2O.

p. 16570, l. 4: How did you handle the potential problem of outliers, if only three time points were used? Which was your quality criterion for flux calculations? Did you reject

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flux values below a certain r2 value?

Author: First of all we have in the same sample both CO2 and N2O. The first check for an outlier is the comparison of these fluxes. CO2 generally grows linearly in the chamber over 1h, so if the CO2 behaves strangely, for example one point is unexpectedly high or low we can also check what happens to N2O and make due considerations. As data are affected by experimental error, the linearity of the fit is not only influenced by the mechanism of N2O emission in the field but the scatter of points around the fitting line is also due to the experimental error. The data are hence tested by comparing the standard error of the liner fit with the standard error associated with the analytical determination.

p. 16570, l. 17-20: What you probably want to say here is that the method you used for measuring soil gas fluxes was not suitable for precisely quantifying soil CO2 efflux. Please explain why you are confident that it was good enough to "be used for comparative analysis between sites and to identify trends in soil respiration", especially in view of the fact that CO2 fluxes were considered as drivers of N2O fluxes in this study?

Author: we do not explicitly demonstrate that CO2 is a driver for N2O, we simply observe that both fluxes have similar annual trends and data correlate, which can also mean that they are both driven by common mechanisms. We hence make some hypotheses, which could be tested with adequate and specific experiments. Indeed the method used which is better suited for N2O and CH4 determinations, is not generally used to quantify absolute values of CO2 fluxes because the closure time is long for a CO2 analysis, which using IR is in the order of few minutes when the increase of CO2 is steeper. Thus the flux expressed per minute and measured in the first 2 minutes is higher than the flux expressed per minute but measured over one hour interval. However, assuming the same type of error is made when samples are compared in time and space we can make comparative measurements and annual trends, which is what is usually reported in literature also by groups who have long time experience on gas analysis. No attempt is made to produce, however, annual budgets.

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p. 16572, l. 4: Is this the correct formula for calculation of the error of the mean value?It seems odd. Shouldn't it be sqrt[(1/(N(N-1))(_(f-fmean)ËĘ2)], with f being the single fluxvalue and fmean the annual mean flux value? And why don't you use the greek letter sigma for the standard deviation?

Author: yes there is a mistake in the greek symbol,delta rather than sigma. Instead for what concern the formula, in the text we phrased the sentence wrongly. What we calculate is the error associated to the annual cumulative flux. So we summed the flux values and we applied the error propagation formula to have the error associated to the sum. However, given that we only had measured error for the days of sampling, we hence calculated an average standard error and then assumed that for the 365 days of the year the daily error was the average standard error and by applying the error propagation formula we get the square root of 365 multiplied the average standard error squared.

p. 16576, l. 15-16: see my first two specific comments. Fig. 3c: The monthly mean air temperature looks strange, with four 3-month periods with exactly the same mean temperature, and with incremental changes being either 0.5_C or 1_C.

Author: right, it is a graphic mistake, we will correct it.

Fig. 4: Have only the upland data been included in the regressions? Anyway, the regression graph seems to represent only the upland data points.

Author: yes, it is only the upland, the lowland has a scattered distribution which cannot be fitted by any specific equation.

Fig. 5a: Is the strong increase of N2O emissions beyond 24_C not perhaps due to a covariance with soil moisture? I suggest combining Figures 4 and 5 in x-y-z plots, with soil temperature and soil moisture as independent variables, and N2O flux as dependent variable.

Author: When we applied multiple regression we did not find an improvement of the ability to predict N_2O emissions combining soil moisture and temperature. On the contrary we found a significant result combining soil respiration and soil moisture. As soil respiration is very sensitive to temperature variations, and being in an aerobic soil, the increase of soil resp with increasing temp could help the development of microsites at lower O_2 tension where denitrification can occur. Thus we interpret the data rather than just a direct influence of T on N_2O, like a combination of direct and indirect effect, so we find more meaningful to leave the two graphs separated. We might have included the x,y,z graph using soil water, resp, N_2O emission, but to save space we just reported the equation.

Interactive comment on Biogeosciences Discuss., 9, 16565, 2012.

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