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Biogeosciences Discussions

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

Anonymous Referee #1

Received and published: 18 January 2013

Bg-2012-500 Comments

General comments

This paper by Castaldi et al. presents data on soil N2O and CO2 emission from a rainforest in southwest Ghana, close to the border with Ivory Coast. Fluxes were measured manually with static chambers from April 2009–November 2010 (19 months) at an upland site and from May–November 2010 (7 months) at a lowland site, with eight chamber replicates at each site. Flux measurements were conducted on six consecutive days in each month. Soil temperature and soil moisture were recorded during each measurement campaign, and soil was analyzed for its texture, bulk density, pH and C and N content. Higher N2O fluxes were found at the upland site as compared to the lowland site, and a strong correlation was found between N2O and CO2 fluxes,

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with N_2O fluxes increasing exponentially with increasing CO_2 fluxes. Highest N_2O and CO_2 fluxes were found at relative low WFPS (between 30–40%), whereas a strong increase of N_2O and CO_2 fluxes was found with increasing temperature, although the temperature range observed in the study was quite narrow (approx. 2°C). The annual average N_2O emission, calculated from the data of the two sites was 2.33 kg N_2O-N ha-1 yr-1, taking into account the proportion of upland and lowland parts of the study area. This value was similar to values for other African rainforest sites in Congo and Kenya.

The present study adds valuable information for constraining the contribution of tropical ecosystems to the global N_2O emission budget. With more and more information from tropical ecosystems, a picture emerges that tropical rainforests definitely are a significant source of N_2O of similar magnitude in different parts of the world, obviously due to generic mechanisms at work in this ecosystem type. Another new piece of information added here is the exponential relationship between N_2O and CO_2 fluxes found in this study, and the explanation presented, i.e. that increasing soil respiration – no matter whether autotrophic or heterotrophic – reduces the oxygen content of the soil and promotes the development of anaerobic zones in the soil, favoring N_2O formation. However, for corroboration of this hypothesis it would have been good to have some information about CO_2 or O_2 concentrations or both along the soil profile.

The paper is well in the scope of Biogeosciences, and is in principle well written and presented. The figures and tables are mostly useful, only combining Figs. 4 and 5 might be taken into consideration (see specific comment below). The English needs to be slightly polished, ideally by a native speaker. Although the paper does not significantly enhance our mechanistic understanding of N_2O fluxes in tropical forests, as no process studies have been performed, it still adds valuable and sound information on tropical N_2O emissions, urgently needed for constraining the global N_2O budget. Therefore, I recommend publication of the paper after the specific comments below have been addressed.

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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.

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.

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?

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.

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

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?

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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 flux values below a certain r2 value?

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 CO_2 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 CO_2 fluxes were considered as drivers of N_2O fluxes in this study?

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))(Σ(f-fmean)ˆ2)], with f being the single flux value and fmean the annual mean flux value? And why don't you use the greek letter sigma for the standard deviation?

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.

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

Fig. 5a: Is the strong increase of N_2O 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 N_2O flux as dependent variable.

Technical corrections

p. 16568, l. 20 and 21: Replace "mg“ with "Mg“.

p. 16570, l. 2: Write either “slightly overpressurized” or “with slight overpressure”.

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p. 16570, l. 4: In line 1 of the same page it says “0, 30, 60 min”.

p. 16570, l. 17: Write “to allow for monitoring instrument drift”.

p. 16572, l. 15: Remove % after space.

p. 16573, l. 1: Remove % after WFPS, and write “30% to 35% WFPS”

p. 16573, l. 6: Remove % after WFPS

p. 16576, l. 10: Replace "extremely“ with "extreme“

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