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Biogeosciences Discussions

Interactive comment on "Drought effects on soil CO_2 efflux in a cacao agroforestry system in Sulawesi, Indonesia" *by* O. van Straaten et al.

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General comments:

The authors investigated the effects of throughfall water exclusion on the soil CO2 efflux from a cacao agroforestry system in Indonesia. Considering the large plantation area, potential drought effects on soil carbon under cacao agroforestry are of high biogeochemical relevance (beside the economical and other ecological implications). To my knowledge, authors were the first who extensively studied drought effects on the soil CO2 efflux from cacao agroforestry. Their results are surprising, as a 13 month drought only marginally reduced the soil CO2 efflux. Methods were state of the art and very thoroughly applied. Authors investigated the gaseous CO2 efflux from the soil surface in combination with soil CO2 concentration profile measurements and estimates

of dissolved CO2 leached in drained soil water. Results are clearly presented and I like the approach to compare parameters of drought responsive soil with non drought responsive soil. This approach allowed relating the drought effects to autotrophic and heterotrophic soil CO2 efflux components. The whole paper is clearly structured. The topic is well introduced. Methods are (overall) precisely described. Results are clearly presented and adequately discussed.

Major findings of the study were already published in GCB (Schwendenmann et al 2009). E.g. soil respiration chamber measurement data and soil water data. In the present BGD paper authors present the same chamber CO2 measurements plus many additional measurements that allowed drawing deeper conclusions about the functional drought-response of the soil CO2 efflux. One figure (Fig 4) is very similar to a figure already published in the GCB paper – except that the water contents are given there as vol% and not as pF value. Authors better remove this figure and instead present the function of the curve in the text (+ reference to the GCB paper).

Beside that the paper already is in a very good shape, there are some issues that should be solved before publishing it.

Major comments:

1) Several times you report on a flush of CO2 during the post-treatment period (or during the first 2 weeks of the post-treatment period). I don't see any post-treatment flush in CO2 (whether in Fig 1 nor in Fig 3). A flush of CO2 would mean a temporary, very pronounced CO2 emission from the soil – largely exceeding the control plot flux (see e.g. Borken and Matzner, GCB, 2008). A CO2 flush can result from the decomposition of accumulated litter as is suggested in the discussion. In this context, authors also report on "considerable accumulation of litter" at roof plots and you suggest that all accumulated labile C was mineralized during the post-treatment phase (discussion and abstract). I guess, if the accumulated litter really decomposed during the first two weeks after roof removal, you would have measured a real flush of CO2. Just as an

example; If only 100g litter C accumulated per m2, the mineralization within 2 weeks would have meant an additional CO2 efflux of $\sim$ 300 mg C m-2 h-1. However, such a flush in CO2 was not observed (Fig 1 and 3). Soil respiration rates just approached pre-treatment levels as the roof was removed. I strongly suggest to remove all parts dealing with CO2 flushes from the manuscript.

2) Some thoughts about C allocation. You suggest that drought mainly reduced autotrophic respiration of cacao roots. The reasoning behind that is clearly explained and comprehensible (cambers near cacao tree stems were more responsive to drought). After removing the roof, soil (autotrophic) respiration rates increase to pre-treatment level. In contrast to the responsive chambers near the stems, non-responsive chambers were not affected during the whole drought period (Fig 3). This however does not really fit with your observation of substantial litter accumulation beneath the roofs. If litter decomposition was reduced, non-responsive chamber CO2 efflux should have been depressed by between 10 and 40% as shown in Fig 5 (litter contribution). However the CO2 efflux remained stable. For me it seems that the sources of non-responsive (and responsive) chamber CO2 may have changed during the simulated drought. In Fig 6 you show a substantial decrease in soil-air CO2 concentrations at roof plots. As you mentioned in the discussion, the extremely high soil air CO2 concentrations in the control plots were likely caused by high water contents, which capture CO2 in deeper soil layers. As this diffusion barrier was removed during the simulated drought, deeper soil layers may have increasingly contributed to the surface soil CO2 efflux. This would explain why non-responsive chambers showed relatively steady CO2 emissions although the leaf litter accumulated.

However, all this remains hypothetic unless you don't report on the litter layer development. In the results and discussion you mention that "considerable amounts of litter" accumulated during the 13 month drought simulation. Please quantify "considerable amounts" – if you didn't measure the exact accumulation, please give at least some information e.g. visual observations like: a...cm litter layer accumulated.... . Also

give information about the litter distribution. Was it evenly distributed over the whole area? – Or was it similarly distributed as the roots (more litter around the stem and less in openings)? If litter was distributed as roots, the story about reduced autotrophic respiration may be reconsidered.

Specific comments:

Introduction:

P11544 L10-15: to determine how belowground CO2 production and surface soil CO2 efflux reacted to a simulated ....

P11544 L15-20: I would delete the sentence "Furthermore, if the drought becomes so severe..." if it really becomes so dry, then dead roots likely would not be decomposed either.

Next sentence: delete Finally and start with During....

Experimental design:

I suggest adding two pictures of the roof plots (one at 60% closure, one at 80% closure). This would amp up the paper and give the reader a clue how a 60% roof closure looks like.

Where were the chambers placed? – Below the roofs or in the roof openings as well?

Soil surface CO2 efflux measurements:

How was the cumulative CO2 flux calculated? In the results you come up with cumulative CO2 efflux from the experiment. How did you calculate that? Please describe in the method section.

Isotope analysis:

Please add a sentence about the delta 13C values of C3 plants and other potential CO2 sources.

Results:

P11553 L25 onwards: there is no flush in CO2 efflux, CO2 levels just went back to pre-treatment levels or below (see above). The roof plot flux was even not statistically significant above the control plot flux.

Discussion:

P11557 L15-20: “Like most of these studies. . ..” and next sentence “The cacao plantation exhibited. . ..” Please delete these sentences – they are already in the results – and belong there.

P11559 L5-10: “First, the nature. . ..” You can delete this sentence. It is not clearly formulated and not really needed – just start with the next sentence.

P11559 L15-20: “Second, we suspect that different CO2 production sources reacted differently to the drought stress. . ...” Please delete the whole sentence, including “the confounding results” (it was rather masking than confounding). The next sentence says exactly the same again but is much clearer.

P11561 L 20: considerable amounts of litter. . ...see above

Rewetting phase: Please reconsider that paragraph - see above

Conclusions:

Are you sure that the decline in soil respiration was compensated during the post-treatment-phase? – the cumulative flux during the whole 19 months was still higher at control plots (17.5 MgCha-2) compared to roof plots (16.5 Mg) not accounting for that the roof plot CO2 efflux was already $\sim$10% higher during pre-treatment. You may consider that roof plots naturally respire $\sim$10% more CO2 (as in the pre-treatment) – then no compensation took place.

Table1:

Do you have data about litter? E.g. C and N contents. You could add it. Please explain ECEC. Please be consistent with SE in the caption. - Once you write $\pm$ 1 SE, once $\pm$ SE.

Table 3:

For me, this table is a bit problematic. You made much more measurements in the morning, compared to the afternoon – how can you compare them? I suggest to completely remove this table – it is not really necessary in the context of the paper either.

Figure 2:

You may add the position of the cacao trees (or does this overload the figure?)

Figure 4:

Did you try to model the CO2 efflux with the relatively simple function of the curve? – Would be interesting how the modeled estimates fit with the measured data. – and could be a nice explanation of the strong temporal variations in control plot soil CO2 efflux. The figure as it is now was already published in the GCB paper and should not be duplicated.

Technical comments:

P11542 L6: you may delete “replicated”

P11543 L9: please delete “stock”

P11544 L4: new paragraph before “In a replicated experiment,. . ..”

P11545 L13: experimental site

P11545 L24: close bracket – Moser et al; add “maximum” before depth

P11546 L5 and rest of the text: please consistently use “roof plots” as introduced in the intro

P11548 L19: delete surface

P11552 L11: “permit” – you mean allow – don’t you?

P11555 L14: delete “respired”

P11555 L24: overall control plot CO_2 flux

P11561 L27: showed that. . .was sensitive. . .

Interactive comment on Biogeosciences Discuss., 6, 11541, 2009.