

Interactive comment on “Drought effects on soil CO₂ efflux in a cacao agroforestry system in Sulawesi, Indonesia” by O. van Straaten et al.

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We kindly thank Mr. Andreas Schindlbacher for his very comprehensive and thoughtful review of the manuscript. We have addressed each comment individually.

Major comments:

1.1. 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

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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 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.

Author's response: The reviewer is correct: after the roof was opened soil CO₂ efflux did not exhibit a pronounced rewetting flush. Accordingly, we have revised the manuscript to reflect this change.

1.2. 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 (chambers 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.

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This would explain why non-responsive chambers showed relatively steady CO₂ emissions although the leaf litter accumulated. However, all this remains hypothetical 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.

Author's response: We would have liked to make a more in-depth analysis as proposed by this reviewer; however, we unfortunately have little additional data to support the visual observations of the leaf litter accumulation and decomposition dynamics. It was only after the experiment was completed when we analyzed the data that we recognized the potential importance of the litter decomposition dynamics.

In the field we observed that 1) Leaf litter was relatively evenly distributed between trees and across the experiment plots and not concentrated around the tree stems 2) Prior to roof opening leaf litter had accumulated in the roof plots.

Beyond these observations however we can only speculate. We suspect that the strong drought of the litter caused it to display hydrophobic properties, which may partly explain the lack of a strong soil CO₂ efflux flush following rewetting.

Specific comments:

Introduction:

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

Author's response: Corrected

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1.4. 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. . .

Author's response: Corrected

Experimental design:

1.5. 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.

Author's response: The photo comparison was not included as the 20% difference was visually not very impressive or felt necessary for the understanding of the paper. Improvements made in roof closure entailed the construction of smaller panels which were placed in roof gaps where trees blocked the long panels from crossing between the parallel gutter system. Photos of the roof can be found in the Schwendenmann et al 2009 GCB paper.

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

Author's response: The chambers were evenly distributed across the plots and occasionally chamber bases were located near gaps in the roof. Since CO₂ efflux chambers were installed prior to roof closure, chambers could be found under a range of roof coverage conditions: from tightly closed to relatively open with more gaps. Gaps in the roof plots were often found near tree stems or where neighboring panels did not butt up against each other closely.

To explain spatial differences in CO₂ efflux, we measured soil moisture at each chamber using a handheld TDR (average of 4 measurements per chamber) during each CO₂ efflux measurement.

The following sentence was added to the Materials and Methods section: "In the roof

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plots, chamber bases were located under a range of roof closure conditions ranging from tightly closed to relatively open with more gaps.”

Soil surface CO₂ efflux measurements:

1.7. 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.

Author's response: The cumulative CO₂ flux in Mg C ha⁻¹ yr⁻¹ was calculated by a series of calculations: 1) CO₂ flux was recalculated from milligrams of carbon respired by 1 m² in one hour (mg C m² h⁻¹) to a daily CO₂ flux in Megagrams of carbon respired by 1 hectare in one day (Mg C ha⁻¹ d⁻¹). 2) CO₂ efflux was then linearly interpolated for all dates between two sampling campaigns. 3) The cumulative CO₂ emission is the sum of the daily CO₂ respiration from each day during the 579 day experiment.

Isotope analysis:

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

Author's response: We added the following sentence into the Discussion: “The δC¹³ isotope signature typically ranges between –22 ‰ and –30 ‰ for C₃ plants, between –10 ‰ and –14 ‰ for C₄ plants and between 0 ‰ and +5 ‰ for carbonate rocks (Trumbore & Druffel, 1995).”

Results:

1.9. 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.

Author's response: See response given for comment 1.1. We have changed the sentences to: “Within three day of opening the roof, in April 2008, soil CO₂ efflux had al-

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ready rebounded to control plot levels. No pronounced CO₂ efflux peak was measured and over the next five months the average roof plot CO₂ efflux did not significantly differ from the control.”

Discussion:

1.10. 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.

Author's response: Corrected

1.11. 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.

Author's response: We have reformulated this section. It was not written clearly and was mentioned by a number of reviewers

1.12. 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.

Author's response: Corrected

1.13. P11561 L 20: considerable amounts of litter. . . See above

Author's response: This was a visual observation made by the first author just prior to roof opening in April 2008. Unfortunately we do not have additional data to support this observation.

1.14. Rewetting phase: Please reconsider that paragraph - see above

Author's response: I have revised this section to reflect that there was no pronounced peak after rewetting but instead a rapid increase in soil respiration once the roof was

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opened.

Conclusions:

1.15. 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 Mg C ha⁻²) compared to roof plots (16.5 Mg) not accounting for that the roof plot CO₂ efflux was already 10% higher during pre-treatment. You may consider that roof plots naturally respire 10% more CO₂ (as in the pre-treatment) – then no compensation took place.

Author's response: Although the average CO₂ respiration in the roof plots was slightly higher during both the pre-treatment and rewetting phases, differences were not significant. Therefore it was incorrect to imply that a 'compensation' took place during the post-treatment phase of the experiment offsetting earlier declines. Accordingly, I have removed reference of a compensation effect.

Table 1:

1.16. Do you have data about litter? E.g. C and N contents. You could add it. Please explain ECEC.

Author's response: Although interesting, I do not feel that this information is particularly important to include in the context of this paper.

1.17. Please be consistent with SE in the caption. - Once you write _ 1 SE, once _ SE.

Author's response: Corrected

Table 3:

1.18. 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.

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Author's response: Table 3 was problematic for all three reviewers. I have therefore removed the table but modified the results section slightly adding values that were previously mentioned in the table. Specifically, I added: "A weak diurnal pattern was detected in soil respiration, whereby CO₂ efflux was lowest early in the early morning between 6 am and 8 am (107.6 ± 12.6 mg C m⁻² h⁻¹) and rose steadily throughout the day reaching a maximum in the mid-afternoon between 2 and 4 pm (142.0 ± 8.6 mg C m⁻² h⁻¹, mean ± 1 SE)."

Figure 2:

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

Author's response: When we added the location of the 545 trees located in the roof plots to the map, the map became cluttered and illegible. Additionally, the relatively coarse scale of the map and the sheer number of trees made it difficult to discern any 'distance to stem' – 'CO₂ drought response' relationship that the reviewer may have hoped to see.

Figure 4:

1.20. 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.

Author's response: Although I agree it would be interesting to compare modeled versus the actual CO₂ respiration, I do not believe it will contribute much in terms of substance or quality to the paper.

1.21. The figure as it is now was already published in the GCB paper and should not be duplicated.

Author's response: Figure 4 as it was presented in this paper originally used an inverse parabolic curve to describe the CO₂ efflux - soil moisture relationship. I have now

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adjusted this figure and instead now use two linear trend lines. One linear fit is used to describe the drying / rewetting end of the moisture spectrum and the gradual change in soil CO₂ efflux (gradual slope). A second linear fit (which has a much steeper slope) describes how soil CO₂ respiration decreases rapidly when soil moisture increased slightly beyond optimal.

In our opinion this graph is extremely important in understanding the ecosystem CO₂ efflux dynamics described in this paper and should not be removed.

The new revised Figure 4 is attached

Technical comments:

1.22. P11542 L6: you may delete “replicated”

Author’s response: Corrected

1.23. P11543 L9: please delete “stock”

Author’s response: Corrected

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

Author’s response: Corrected

1.25. P11545 L13: experimental site

Author’s response: Corrected

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

Author’s response: Corrected

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

Author’s response: Corrected

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

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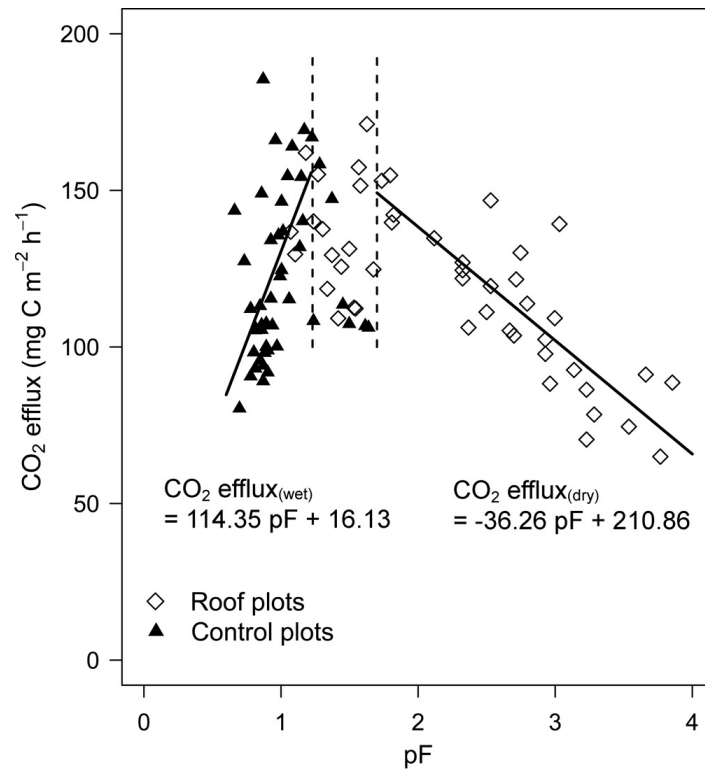


Fig. 1. Revised Fig 4. The relationship between soil CO₂ efflux and soil moisture expressed using two linear regressions (one for the wet end of the moisture spectrum and a second at the dry end)