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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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We kindly thank the reviewer for comprehensive review of the manuscript and in particular for highlighting areas of the text that needed additional clarification. We have addressed each comment individually.

General comments:

3.1. In the abstract no final conclusions are reported. Only the main results are presented but not real conclusions. A statement about the main findings would be really good.

Author's response: I have added a concluding sentence to the abstract: "The relatively mild soil CO2 efflux depression experienced indicates that this agroforestry ecosystem

is capable of mitigating droughts with only minor stress symptoms."

3.2. Line 6 page 11542. Please delete replicated.

Author's response: Corrected

3.3. The introduction is well presented and very interesting. The Material and methods section is accurate and the methodology described in details. Only few minor soil characteristics presented in the tables needs to be better addressed.

Author's response: I have included the additional soil characteristic as background information for readers. I do not believe it is important for the context of the paper to elaborate on this information in the written text.

3.4. Line 24 page 11545: close bracket – Moser et al

Author's response: Corrected

3.5. Line 16-21 page 11545 you report the soil classification and the texture referring to unpublished data from (Leitner and Michalzik). What about the soil features in Table 1 where are reported the texture, ECEC, pH, C and N with the relative errors. The table is really appreciated, could be worthy to refer to these data for soil characterization instead of the unpublished data non mentioned. Some of these parameters (e.g. bulk density) are not easily determined directly in the field and require a great accuracy.

Author's response: I think it is important to include both the soil classification type as well as the detailed soil physical and chemical properties presented in Table 1. The classification done by Leitner and Michalzik (unfortunately not published) allows the reader to quickly recognize what type of soil is present without having to study the table. The table on the other hand gives more detailed description of the soil characteristics.

3.6. Spend few words about the methodology used would be important. Also because later in the text you refer some differences in soil respiration between roof and control plots to the different bulk density.

Author's response: We used the "undisturbed core method" method to determine bulk density. Specifically: we used a metal cylinder to obtain the undisturbed soil sample from different sampling depths from one soil pit per plot. Samples were subsequently taken to the laboratory, oven dried and weighed. Bulk density was determined by dividing the oven dry weight with the cylinder volume. In the revised text I have made a reference to the bulk density determination method used: "measured using the undisturbed core method described by Blake and Hartge (2006)"

3.7. Line 5-15 page 1546, you mention that the roof was transparent. PVC panels placed on top of bamboo panels (0.5x4.5 m) that instead are not transparent. You state the roof was not influencing the temperature, humidity and incident radiation, but given the large numbers of bamboo panel used (1500) that can cover a large surface area probably you should support strongly your statement. You could explain where you placed the chambers to avoid misunderstandings.

Author's response: I have clarified to text to reflect that the bamboo was used to build the frames onto which the transparent polyethylene plastic sheets were attached to. Irrespective of the bamboo frames light could still easily penetrate to the plantation floor. Incident radiation measurements found minimal decreases caused by the roof. The text is now written as follows: "The roof consisted of 1500 individual long and narrow bamboo frames (0.5 x 4.6 m) onto which transparent polyethylene plastic sheets were mounted. The roof was built at a height of approximately 1.2 m."

3.8. Line 10-15 page 11548, when investigating the litter effect on soil respiration, you chose 2 chambers per control plots and in the surroundings of these two chambers per plot you installed other two chambers. From one of the new chambers you removed the litter and you placed it in the other. At the end you have: the main chamber, where I supposed you have the litter, and the new chambers installed, one with the litter and the other not. Can you explain how you deduce the effect of litter considering all the three chambers? Just making clear this part.

Author's response: I have attempted to clarify how leaf litter CO2 production was calculated: 'The carbon dioxide produced from the litter layer was calculated by subtracting the CO2 flux respired by the 'main' chamber with that respired by the litter-removed chamber." I should also mention that the litter decomposition dynamics as determined from the third "litter- doubled" chamber are not included in this paper.

3.9. Line 11 page 11548. They are not six sites, but six chamber in three plots.

Author's response: I have modified the text to make it more comprehensible (I hope): "To study the contribution of leaf litter to CO2 efflux, we randomly selected two experiment chambers in each control plot (in total 6 chambers). Then at each of the selected chambers, two additional chambers were installed directly adjacent to the 'main' chamber (<1 m away).

3.10. Line 11 page 11551. I suppose the three-time periods of the experiments are those clearly defined line 23-27 at page 11546

Author's response: I have modified both sections mentioned above to make these time periods more explicit:

"We divided the experiment into four time periods. The fist was the "pre treatment period" which started on 27 January, 2007 and lasted until the roof was closed on 1 March, 2007 – a total of 33 days. The period of roof closure was divided into two periods, the first being the initial ten months when the drought effect was very mild, hereafter referred to as "treatment period #1" (from March 1, 2007 to January 1, 2008; 306 days), followed by "treatment period #2", which corresponded to the time when the drought effect was more pronounced and ran until April 10, 2008 when the roof was opened (100 days). The fourth was the "post-treatment period" which extended until 27 August, 2009 (139 days).

Results and Discussion are well reported and the data clearly elaborated.

3.11. Line 19-22 page 11557. You attributed the differences in soil respiration between

roof and control plots to the differences in bulk density. From table 1 no great differences are observable to justify a difference in the flux. Taking into account the relative errors at each depth the bulk density between the control and roof plots are identical.

Author's response: On the reviewer's advice I have modified the text. I have removed reference to bulk density as a possible explanation of CO2 efflux differences between control and roof. Changes were made for the pre-treatment (as suggested) as well as for the post-treatment sections (Pg 11563 Line 20-23 in original version). Additionally, soil CO2 efflux averages were not significantly different during either time period (pre and post) and therefore was incorrect to even speculate.

3.12. Line 2-7 page 11559. Please make clear this part. Try to rephrase.

Author's response: We have tried to reformulated this section so that it is easier to understand.

3.13. Line 4-6 page 11561. Some reference to have a comparison with you high CO2 concentration are needed.

Author's response: I have included three additional references from studies where soil CO2 concentrations were measured in the tropics. Soil CO2 concentrations in these studies ranged from 1.2% (at 55 cm depth) reported by Davidson et al. (2006) to 7% (at 300 cm depth) reported by Sotta et al. (2007).

3.14. I think some stronger conclusion should be reported in the conclusion section. At the moment only a repetition of the main results is reported.

Author's response: On the reviewer's advice I have improved the concluding paragraph:

"Although, there were evidently some drought induced carbon responses, the net emission of soil CO2 over the duration of the 19 month experiment remained unaffected. The 13-month simulated drought, caused a slight decrease in soil respiration because of localized changes in root activity and declines in decomposition rates both above and belowground. The moderate soil CO2 efflux depression experienced during the

drought indicates that this agroforestry ecosystem is capable of mitigating drought stress for extended periods.

Bibliography:

3.15. Is missing the reference R-development core team, 2008.

Author's response: Corrected

3.16. Table 3: not really useful. Probably a plot could be more interesting to see.

Author's response: Table 3 was problematic for all three reviewers. We have therefore removed the table but modified the results section slightly adding values that were previously mentioned in the table. Specifically, we 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 $\pm$ 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 $\pm$ 8.6 mg C m-2 h-1, mean $\pm$ 1 SE)."

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