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Biogeosciences Discussions

Interactive comment on "Spatial and temporal variation of CO_2 efflux along a disturbance gradient in a *miombo* woodland in Western Zambia" *by* L. Merbold et al.

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First of all, we would like to thank both reviewers for their detailed and helpful comments and suggestions for improving the manuscript 'Spatial and temporal variation of CO2 efflux along a disturbance gradient in a miombo woodland in Western Zambia' by L. Merbold, W. Ziegler, M.M. Mukelabai, and W.L. Kutsch (bg-2010-138). Your help and feedback is highly appreciated.

Both reviewers suggested a restructuring of certain parts of the manuscript, which we did accordingly in the final version of the paper. We also tried to focus the manuscript more strongly on the spatial heterogeneity of soil respiration and removed some of the

figures and tables.

Each of the comments of the 2 reviewers will be answered separately at the bottom of this paragraph. Original comments are indicated with "R#1/2" and answers are indicated with "A".

Comments to reviewer #1:

A: We changed the structure of the introduction as proposed. The manuscript now clearly ends with the objectives (2) and clear hypothesis, combining the definition of the driving factors of soil respiration and additionally covering the variations of soil respiration along the disturbance gradient.

R#1: Results on the exponential relationships between soil respiration and temperature, its modulation by soil moisture and the disproportionate increases in CO2 efflux after rain pulses are not novel. Nevertheless, the authors might want to discuss the mechanisms leading to these sudden CO2 bursts after rain events.

A: Indeed, responses of soil respiration to temperature and moisture changes are already known for quite some time and so are pulses after rain events. However, it remains a major challenge to quantify and to model these pulses. As suggested by reviewer #1 we added additional information on the mechanisms lying beneath these bursts of CO2, e.g. a "solid" water layer penetrating the ground during the rainfall event and therefore pushing CO2 in the pore spaces in the soil towards the atmosphere, explaining the sudden peak. The strong decrease in efflux rates after the burst of CO2 can also be explained CO2 storage in the soil. Soil pores are "depleted" in CO2 and need some time to fill up again. Higher emissions after a rainfall event may be explained by higher microbial activity and larger CO2 efflux from decomposition processes taking place in the soil. Since the ms focuses primarily on the spatial heterogeneity of respiration, its changes along the disturbance gradient and a lack of data during rain events we did explore the effects of rain pulses in detail.

R#1: p. 5776, l. 15-18. The authors state that the Reichstein et al. (2003) model only considers temperature as a driver of soil respiration, when this applies to the Reichstein et al. (2005) approach, This invalidates the authors' reasoning attributing the mismatch between top-down and bottom-up approaches to the lack of a soil moisture control over soil respiration. Then, the authors should discuss why the model without a soil temperature response performs better.

A: This was actually a mistake. The Reichstein et al. (2003) approach includes soil moisture as a parameter influencing soil respiration and we changed this in the final ms.. However, since the Lasslop approach does not include soil water content as a driver of Rsoil, this does not completely invalidate our reasoning on the mismatch between the top-down and bottom-up approach due to a lack of soil moisture control. We also refer to the statistics for the different regression lines given in Figure 9. Our results may additionally depend on the different responses of soil respiration to soil water content, specifically in tropical ecosystems. Where the Reichstein et al. (2003) approach was adjusted to a minimum of biweekly periods of soil moisture responses of Reco (EC measured ecosystem respiration) we were able to model the general seasonal pattern. Though this approach is suitable for annual timescales, it does not account for direct responses after rainfall events and hence may include a significant underestimation of the respiratory fluxes. As proposed by Archibald et al. (BG 2009) a daily moving window to define the reference temperature may be more suitable to model/analyse the respiratory terms in tropical, specifically in African ecosystems.

R#1: p. 5776, l. 24: This statement does not seem to hold at the ecosystem level, as the model without soil moisture as an input variable (Reichstein et al., 2005) performed better than the one which did include it (Reichstein et al., 2003). p.5777, l. 11-14. Again, the model without soil moisture as an input variable (Reichstein et al., 2005) performed better than the one which did include it (Reichstein et al., 2003).

A: When looking at the statistics – which were added to Figure 9, we argue that both approaches Reichstein et al 2003 and 2005 perform similarily well, where one per-

forms better when not accounting for heterogeneity (2005) and vice versa the 2003 model performs better when including spatial heterogeneity as shown by the correlation coefficients.

R#1: p. 5764, l.6-9: Please specify (1) the number of leaf samples measured, (2) the time of year when the measurements were made (dry, wet or both seasons?) and (3) how leaf measurements were up-scaled to ecosystem-level estimates of foliage respiration.

A: Leaf level measurements were undertaken in the wet season of 2008 and 2009, but not in the dry season, since all trees had lost their leaves. Photosynthesis and therefore also dark respiration was measured in all plots at the dominant tree species, where each species was represented by at least 5 leaves, including leaves in the shade or in the sunlight (crown leaves). A total of 30 leaves were measured in 2008 and 15 leaves in 2009. However, higher numbers of replicates were impossible, since the device measuring photosynthesis was the same as used for respiration. We did not include further information on photosynthesis measurements in this manuscript, since this would clearly be beyond the scope of this paper. Leaf level measurements were up-scaled using plot specific measured values of leaf area index (LAI) and the total area of our plots (2500m-2).

R#1: p. 5764, l.10-16: Meir and Grace (2002) is missing from the reference list. p. 5764,l.18. How was RPAW calculated? Was it based on measurements of soil water content at wilting point and ïňAeld capacity?

A: The reference Meir and Grace (2002) was added in the final ms.. RPAW calculation was based on soil water content at wilting point and field capacity. The information was added in the final revised ms.

R#1: In general, within- and between- variability in soil respiration is dealt with in detail, but I wonder whether the authors could explore more the inïňĆuence of dominant vegetation types on soils respiration at the plot and subplot levels. One option could be investigating whether, across plots, subplots with similar ground cover behave similarly in terms of the responses of soil respiration to short-term controls (temperature, soil moisture) and soil and cover physical properties. Another suggestion could be relating normalised soil respiration at the plot level with percent cover of trees, shrubs, litter or of combined categories on the basis of similar functional responses (trees+shrubs, grasses+ mosses...). For example, I suspect differences in root biomass between trees and grasses could inïňĆuence spatial patterns of soil respiration.

A: We intended exactly to do such an analysis of the data, however due to the experimental setup and the disturbance regime this was impossible to do. Moreover for the few cases where we could do such an analysis we were unable to find significant relations. The experimental platform of this study was a "natural" disturbance gradient, we had changing types of ground cover between the different plots (Fig. 1, table 1), only few vegetation cover types (n=3) were found in each plot. Here we checked and the response curves varied between plots. However we did not find a clear pattern along the disturbance gradient and explained this by the different species composition in the different plots, whereas we found certain tree species in Plot1 only (e.g. Guiburtia sp.) we found others in Plot 4 only (e.g. Brachystegia bakerana). We would like to thank reviewer #1 for the interesting thought on differences in root biomass between the tree dominated (plot 2-4) and the grass dominated plot (1) and the possibility of relating soil respiration to tree cover. However, belowground biomass did not vary significantly between plots (not shown), which might be explained by the disturbance regime. When producing charcoal, trunks are commonly cut aboveground, whereas the roots are not affected contrary to the conversion from woodland to agricultural land.

R#1: Regarding interannual variation in soil respiration (Figure 5), all subplot types in Plot 2 showed decreased ïňĆuxes from 2008 to 2009 wet seasons, whereas most of the covertypes in Plot 1 show increases or no changes in ïňĆuxes. Are these patterns consistent for the rest of the plots? Do you have an explanation?

A: We found a similar decrease in fluxes for Plot 3 and cannot provide information for

plot 4, since this plot was only measured in 2009. Possible explanations might be (1) the different vegetation cover, mainly grasses in plot 1, compared to trees in plot 2 and 3 and their varying responses to moisture inputs, where grasses may response faster to recent weather events (see also Merbold et al 2009, BG). Reviewer #2 asked for supplemental meteorological information during the campaign. Therefore we added a short table giving monthly means of air temperature, Rg, VPD, and cumulative precipitation for January – March 2008 and 2009. Another explanation (2) for different trends in soil respiration between the two years might be the ongoing disturbance regime of charcoal production and cattle grazing and the therefore resulting changes to the ground, showing higher carbon dioxide efflux rates.

R#1: There is some confusion in the interpretation of Table 5. In the main text the authors state that:'No trend of changes in soil respiration along the disturbance gradient was observed during the dry season 2008 (Table 5). In contrast, ïňĆuxes varied along the disturbance gradient showing a clear trend in the wet season in 2008'While in the caption it says: 'Differences in average plot respiration were signiïňĄcant in 2008 without showing a clear trend'Please correct these discrepancies in the interpretation of results.

A: Certainly this might have been confusing and we have cleared this in the final manuscript. For clarification with reviewer #1 we are referring to different season within 2008 (wet and dry).

R#1: ith respect to this Table 5, is there a reason why plots 1 and 3 show a more signiïňĄcant decrease in soil respiration from wet to dry season in 2008 than plot 2? (do they differ in vegetation composition?)

A: This is indeed an interesting result and reviewer #1 is correct when mentioning vegetation composition, primarily Brachystegia spiciformis - very tall tree and big in diameter – were found in plot 2. These large trees may access deeper water sources than small trees (Plot 3) and grasses (Plot 1) and therefore show higher rates of root respiration, resulting in larger rates of soil respiration.

R#1: Could the authors discuss in more detail why the relationships between soil respiration and LAI/soil C content at the subplot level (Fig. 7) disappear when data is analysed at the plot level (Fig. 8)? Maybe the fact that data from 2009 only is used for Fig. 8 has an inïňĆuence. p. 5771, l. 17-19.

A: First of all, we decided to choose the year 2009 since data from all plots were available and if had included data form 2008, we would include further variables in the analysis such as annual climate variation. Secondly the data presented in Figures 8 and 9 (figures 7 and 8 in the final ms) show large variations, specifically visualized in plots 1 and 3 covering only one year of data. Therefore it is always difficult to find relations at larger scales.

R#1: Check the sentence: 'When analyzing the results of the 3 different top-down approaches were the night time based models during all seasons (black and white dots) within a 20% range (includingover- and underestimation) of the process up-scaling'. It makes little sense to me (word order?).

A: The sentence was changed in the final ms.

R#1: p. 5773, l. 10-12. Can you look for a relationship between soil respiration and percent cover of litter to support this statement?

A: We tried to find such a relationship, but were unable to find significant relations for single subcategories. Unfortunately we were unable to install litter traps at the installed grid. Therefore we had to rely to the classification, based on visual observation.

R#1: p. 5773, l. 16. Could you roughly estimate the contribution to LAI below 1 m height?

A: LAI below 1m is negligible within the undisturbed plots, due to only very few grasses and the shrubs are commonly higher than 1m. At the disturbed plot our data may be significantly biased. However parts of the grass layer are included in our data, since

the grasses easily reach 1m of height or more during the wet season. We assume an underestimation of LAI of approximately 25-50%.

R#1: p.5774, l.1-5. These lines are more appropriate for the methods section, as it is just a description of the plots.

A: These lines were included in the methods section in the final ms.

R#1 p.5774, l.17. The hypothesis was falsiïňĄed.

A: Done

R#1: p.5777, l.1: Use a comma in: 'When comparing plots of different degrees of distur- bance, spatial...'

A: Done

R#1: p.5777, l. 2. According to your results, the only soil property related to soil respiration variability was soil carbon content, so your text here leads to the idea that other soil properties are also involved (which probably are, but they are not shown in the results).

A: This was rephrased in the final ms., focusing on soil carbon content primarily but also mentioning charcoal content and adjacent carbon content as an influence in the disturbed plot.

R#1: Table 3: What does the subscript 'a' mean ?

A: 'a′ stands for the explaining the small r2 for plot 4, originating from only half the samples compared to all other plots and therefore a less reliable correlation. This information was added in the final ms.

R#1: Table 1: Check table caption, where it says 'CFG in plot 4' it should say 'CFG in plot 1', according to what it is shown in the table itself.

A: Done

Comments do reviewer #2:

A: As suggested by reviewer #2 we re-elaborated our results, erased figure 2 and table 4 and included the statistics in Figure 7. As suggested by both reviewers, we also restructured the introduction aiming at the strengths of the ms and a more focused storyline: spatial heterogeneity of soil respiration and changes along the disturbance gradient. Specific comments are answered below.

R#2: The terminology for the subplot classiïňAcation needs to be improved: I could not follow the assignment of categorical numbers (1...14) to the categories: for each plot, the numbers (e.g. 1) represent different categories, which makes a comparison in the subsequent ïňAgures very difïňAcult. For example No 1 presents a different vegetation type in plots 1, 3, and 4 (being FHE, ECI and ABE). Indeed, the numbers could be fully omitted. Further I would suggest to stick to a more intuitive abbreviation, such as T – trees; G- grasses: L – litter etc.

A: We changed all tables and graphs to a more apprehensive assignment of the categorical number and an intuitive abbreviation (T=trees, G=grasses etc.). Any confusion caused by similar categorical numbers but different categories were corrected. Though, we decided to leave the categorical numbers in the tables/graphs, since a single number is easier to remember and to compare than a three letter code (which remains important to identify the different subclasses of ground cover).

R#2: The current form of presentation does not allow a comparison of different vegetation types (e.g. tree cover versus grass cover) on soil respiration and I fully agree with reviewer 1 that this is an important aspect to investigate.

A: We agree with both reviewers that soil respiration is likely to be affected by tree or grass cover. In general our results showed no differences between the two major vegetation types, e.g. Plot 1 mainly covered by grasses in comparison to plot 2 – 4 with very sparse amounts of grasses (Figure 8). We did not find any significant correlation between soil respiration and single ground cover classes. Therefore we argue, that

carbon content in the first 10 cm may be one of several factor explaining the spatial heterogeneity of carbon emissions. We added this information in the final ms.

R#2: The same applies to ïňAgure 3: identical vegetation types should receive the same color in all plots. It is not just an issue of data presentation: it should be analyzed to what extent the between-plot differences are due to changes in vegetation composition.

A: In the original figure, identical vegetation types were assigned to the same color in all plots. Since we changed the categorical names to more intuitive abbreviations we believe this further improved visualization. We also intended to do a vegetation type specific analysis of Rsoil, since we could barely show any connection for single subclasses. Therefore we combined the 3 most abundant ground cover categories for each plot. Further information was added to the final manuscript.

R#2: Similarly, the data for similar vegetation types across plots should be compared. The analysis of heterogeneity in soil respiration data and up-scaling to ecosystem ïňĆuxes could be better linked throughout the manuscript. Currently, it reads as if these are two different lines of investigation.

A: Our study had 2 major objectives: one was to study spatial heterogeneity of soil respiration within and between plots (including a disturbance gradient) and the second was to scale (1) our results for comparison with EC measurements (2). We argue that these are two dependent investigations, where the first is of crucial importance for the second. Therefore the analysis and discussion of the top-down and bottom-up approach for scaling can be found at the end of each chapter.

R#2: The authors make a strong point that heterogeneity in soil respiration was significantly linked to soil carbon content. However, even though this relationship is significant in 3 plots (due to high number of replicates), the amount of variance explained by the correlation between Rsnom and C is extremely low: only 3 and 8% of the variance can be explained by this correlation in plot 3 and 4, respectively. Thus there is little ecologically relevant information in this relationship. Therefore it should be interpreted more carefully and the results and discussion need to be modified accordingly.

A: Heterogeneity in Rsoil was explained by soil carbon content, giving small coefficients of variation but significant results. We argue this is not due the number of replicates (n approx. 100), which is relatively small for the area of 2500 m-2 per plot and when compared to eddy covariance datasets. We still argue that this correlation gives ecological relevant information, pointing at the productive hotspots as mentioned in the discussion section. Though parts of the results and discussion were modified.

R#2: Be more precise on the time which has elapsed between logging and measurements How many soil collars were inserted per subplot?

A: Logging still continuous, though the most valuable trees were cut during the two previous years of the study. Each subplot was represented by at least three respiration collars. The information was added in the final ms.

R#2: Some information on the weather conditions (variability) within each of the sampling campaigns should be provided

A: Both 2008 and 2009 were rather wet years, receiving approx. 100mm more in annual precipitation as the longterm mean (Meteo data was added in Table 2 - new). The wet campaigns took place between February and March during both years (defined as the peak wet season) with regular thunderstorm and heavy rainfall events. The dry season campaign took place during the peak dry season at the beginning of September, approx. 4 to 8 weeks before the first rains.

R#2: Unclear if stem respiration was considered constant or scaled to temperature changes. The implications and potential errors during up-scaling should be shortly discussed The interpretation of the eddy covariance data for the different plots could be biased if the predominate wind direction changes during seasons. Some additional information should be given.

A: Stem respiration, considered to be the smallest term contributing to ecosystem respiration was assumed to be constant. For the period under observation (12 daytime hours, temperature changes were negligible, moreover we were unable to measure stem respiration and decided to choose a relationship specifically found for tropical trees, instead of choosing a set percentage of Rsoil and most of the time derived from temperate ecosystems.

R#2: There is some sort of discussion already in M&M (e.g. line 264 ff; or line 281ff), which would be better placed in the discussion

A: Since this only focuses on data treatment and the eddy covariance method has become a widely used during the last decade, this is clearly methodological. We chose not to discuss this separately in the discussion section but rather mention the issue in the material & methods section.

R#2: Many (complex) ïňĄgures are only brieïňĆy described with one or two sentences.

A: One Figure was removed and the figure caption were extended and improved in the final manuscript.

R#2: The number of ïňĄgures should be reduced with those remaining being better described.

A:Done

R#2: The effect of rain pulses on soil respiration is only brieïňĆy mentioned but I think this aspect needs a bit more consideration as it can be an important phenomenon in these systems.

A: We agree with reviewer #2, that the effects of rain pulses are an important phenomenon in these systems and further information was added in the final ms.. Though we argue, that this is a small part of our results but the major objectives of our study were the spatial heterogeneity of soil respiration, changes in Rsoil due to disturbance and also up-scaling of chamber measurements, therefore a detailed analysis of such rainfall pulses would clearly expand the ms. to an unreasonable extend. Moreover, measurements including rain pulses (only few) were removed for the spatial analysis of carbon emissions from the soil.

R#2: There is a lot of recent literature on the underlying courses (e.g. Inglima et al. 2009, Global Change Biol. 15, 1289-1301; Borken & Matzner 2009, Global Change Biol. 15, 808-824; Unger et al. 2010 Soil Biol Biochem, 42: 1800-1810).

A: We thank reviewer #2 for pointing out the three publications, which clearly explore the effects of rain pulses on respiration.

R#2: Fig.2 is not strictly required as the results are mentioned in the text.

A: Figure 2 was removed from the final ms..

R#2: Line 373: should probably read "same categories" instead of "different categories". This sentence is unclear

A: This sentence was removed from the final ms. since it did not provide additional valuable information.

R#2: Line 373-374: this is not visible from Figure 6: e.g. category 4 has the lowest efflux rate in plot 1 and the highest in plot 2.

A: This shows exactly what we tried to tell with the before mentioned sentence. Same categories were showing different efflux rates in different plots, whereas differing categories were showing similar magnitudes of CO2 efflux in different plots.

R#2: A different way of presentation should be chosen to allow direct comparison of vegetation classes (see comments above). Adjust the scale on the y-axis (currently 20 while the highest rates are below 14 μmol m-2s-1) to facilitate the comparison Information in Fig. 7 is given in Table 4 and can thus be omitted Line 391ff – the trend along the disturbance gradient is not very clear and needs a better explanation Line 421-423 – rephrase sentence, unclear

A: We removed table 4 and added the information (p values and correlation coefficients in Figure 7) and changed the scaling of the y-axis as proposed by reviewer #2.

R#2: Line 429-430: it is difficult to judge the quality of the regressions ("best ïňĄt"), please indicate r2 and p-levels

A:Done, statistics were added to Figure 9 (now Figure 8).

R#2: More care is needed in the interpretation of the effect of soil carbon content on soil respiration (see comments above). I do not ïňĄnd convincing arguments for "hot spots of soil carbon"

A: We did a more careful interpretation of the soil carbon content influence on respiratory efflux. The hot spots of soil carbon were unfortunately only represented by a limited number of collars (n=6), though showing high efflux rates. We also argue for soil carbon hot spots since soil at these places used to be used for fertilization from the local communities over decades (personal communication).

R#2: Discussion could be shortened by 10-20% and focused, taken the above mentioned comments into account.

A: We shortened the discussion section in the final manuscript.

Yours sincerely

Lutz Merbold

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