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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.

Anonymous Referee #1

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General comments

The manuscript largely deals with temporal (diurnal, seasonal, interannual) and spatial variability (between- and within-plots in a disturbance gradient) of soil respiration in a miombo woodland. The authors also integrate soil and plant components of CO2 efflux to obtain ecosystem respiration estimates comparable to eddy covariance measurements. Miombo woodlands are a scarcely studied ecosystem in terms of carbon-related processes, and specifically, in the field of CO2 flux upscaling. Hence, the study is highly relevant to improve our knowledge on the processes driving CO2 efflux in such ecosystems.

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The main results of the study show that (1) across seasons, soil moisture controls soil respiration, with the magnitude of the fluxes being 75% less during the dry season compared to the wet season, (2) there was no detectable trend in soil respiration along a disturbance gradient and no identifiable factors controlling its variability between the plots, (3) soil organic carbon was the only factor driving spatial variability within undisturbed plots and (4) ecosystem respiration obtained from upscaled chamber measurements differed by up to 25% from eddy covariance fluxes.

While the results of the study are in general well presented, the analyses could explore in more detail the influence of different dominant vegetation cover on soil respiration. In addition, parts of the discussion and conclusions should be rewritten, as some there is some confusion about the performance of two of the models employed in the top-down estimates of ecosystem respiration that the authots should clarify.

Specific comments

Introduction

The introduction is well written and presents all the necessary background and ideas of the study. My only concern would be the structure, with your objectives split into two groups, while I believe they should be together in the same paragraph. I suggest re-structuring the introduction in a manner that the hypotheses/objectives of the study are in the same paragraph.

I would place your paragraphs 5 and 6 (p. 5760, l.9-23), without the first sentence in paragraph 5, after the end of line 16, p. 5759, linking the issues of spatial variability (previous paragraph) and your particular case in the miombo woodland. Then, I would combine l.17-27 in p. 5759 in a new paragraph, dealing with upscaling issues. Your current paragraph 4, combined with the paragraph 7, stating the hypotheses/objectives, should be at the end of the introduction.

Material and methods

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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.

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 field capacity?

Results

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.

In general, within- and between- variability in soil respiration is dealt with in detail, but I wonder whether the authors could explore more the influence 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 influence spatial patterns of soil respiration.

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

There is some confusion in the interpretation of Table 5. In the main text the authors

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state that:

'No trend of changes in soil respiration along the disturbance gradient was observed during the dry season 2008 (Table 5). In contrast, fluxes 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 significant in 2008 without showing a clear trend'

Please correct these discrepancies in the interpretation of results.

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

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 influence. p. 5771, l. 17-19. 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 (including over- and underestimation) of the process up-scaling'.

It makes little sense to me (word order?).

Discussion

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

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

p.5774, l.1-5. These lines are more appropriate for the methods section, as it is just a

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description of the plots.

p.5774, l.17. The hypothesis was falsified.

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.

Conclusions

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.1: Use a comma in: 'When comparing plots of different degrees of disturbance, spatial...'

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).

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).

Tables

Table 3: What does the subscript 'a' mean ?

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.

Interactive comment on Biogeosciences Discuss., 7, 5757, 2010.