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Biogeosciences Discussions

Interactive comment on "Precipitation as driver of carbon fluxes in 11 African ecosystems" *by* L. Merbold et al.

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Summary

The authors present analysis of eddy flux data from 9 sites distributed in semi-arid and seasonal savannas (<1000 mm/yr) of Southern and West Africa, and 2 sites (a mesic grassland and eucalypt plantation) in Congo (˜1100 mm/yr). The analysis focuses on the sub-hourly (i.e. 30 minute) measurement time-scale, with the following primary analyses: i) estimation of ecosystem respiration, and assessment of respiration responses to soil moisture and temperature; ii) estimation of photosynthetic uptake (i.e. GPP=NEE-R) and examination of GPP responses to light, vapor pressure and temperature; iii) estimation of a maximum photosynthetic uptake (the asymptote of an envelope-line fit to the GPP light response data); and iv) cross-site comparisons of max photosynthesis (and max conductance) in relation to rainfall and satellite-based estimates of PAR interception.

The paper presents some interesting comparisons and it is particularly interesting to see the comparisons across multiple sites. I therefore feel this is a worthwhile contribution. However, I am worried by the somewhat over-simplified approach to exploring the data. Firstly, the numerous simple regressions of GPP 'responses' to different variables: since rainfall, temperature and humidity are correlated in most locations (for example, most sites are in summer rainfall areas so rainfall, temperature and humidity co-vary) it makes causal inferences from linear regressions dangerous at best. Secondly, and possibly even more significant in my opinion, the use of ecosystem-scale fluxes (GPP and respiration) in highly seasonal environments with little or no reference to the most important variable (i.e. leaf area) threatens to make nonsense of the analysis. In my opinion, any comparison of ecosystem-scale fluxes, whether through time (site seasonality) or among sites, must first examine the extent to which any differences are 'driven' by leaf area changes.

I provide some examples in my points below where observed patterns and inferences are likely to be affected by LAI seasonality. I also understand it may be difficult (or impossible) to obtain LAI seasonality for all these sites. However, if inclusion of LAI in more extended (correct) analysis is not possible, the authors must at least acknowledge that their analyses are affected by varying leaf area and discuss how this omission affects the results and conclusions of this work.

Specific Comments

1. Introduction: The discussion of the 'important approaches to understand the variations of structure and function of African ecosystems' is a little confusing since structure and function are such different things. Furthermore, the examples provided for each of the three approaches omit numerous relevant papers. A restructuring of these paragraphs might be worthwhile. Reference to Williams et al., 2008, JGR 113, G04015

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would also be appropriate here.

2. P4075, line 1: I am not convinced analysis of eddy flux data can provide much understanding of the structure, perhaps analysis of vegetation structure can provide some understanding of the fluxes.

3. The Mali-Kelma is markedly different from all other sites since it sits in a depression receiving runoff water from surrounding watersheds and is flooded for long periods of the rainy season. Authors might consider deleting this site, or at least placing it at the 'wet' end of the rainfall gradient in the tables and plots. Effective rainfall for this site might be 3 or 4 times the regional average of 350 mm.

4. P4075, lines 5-15: The HAPEX-Sahel experiment and various eddy flux papers from that period are much more significant than it would appear from this discussion. You refer to general papers by J-P Goutourbe and Dolman, but do not refer to the papers by Levy, Verhoef, Hanan and several others (EC measurements were made at approx 15 sites in semi-arid shrub-fallow, tigerbush, millet near Niamey in 1991-92).

5. Equations 1-3: Respiratory fluxes vary strongly with LAI (in part because leaves respire, and in part because leaves provide substrate for root, and soil heterotroph, respiration). Failure to include these terms here means that much of the variability seen in Figure 3 is likely the result of changing LAI. Hanan et al. 1998, Global Change Biology 4, 523-538, did a rather comprehensive analysis of how leaf area, heterotrophic and autotrophic and soil respiratory terms impact nighttime eddy fluxes for a shrub savanna in Niger.

One particular effect of omitting LAI in estimating respiration is that, when you then calculate GPP, you will tend to bias respiration low when LAI is high (thus GPP will bias low) and bias respiration high when LAI is low (thus GPP will bias high).

6. Equation 5: The asymptote in this relationship also varies with leaf area. As such, the cloud of points in figure 4 may largely reflect that seasonality.

7. P4079, line 18: I would think the dry season is >7 months at the Agoufou site.

8. P4079, line 21: the variation in temperature is significant. In particular, West Africa summer temperatures are markedly higher than temperatures at similar rainfall in southern Africa, making these sites effectively much drier (see Williams et al., JGR, 2008).

9. P4080, line 19: Skukuza shows an increasing, then decreasing, 'response' of of GPP to vapor pressure in Figure 5. Likely an LAI effect again, given high humidity prior to and just after the start of the rains (humidity build before the rains set in) the low photosynthesis at low vapor pressure may simply reflect the fact that there's little of no leaf area before the rains'. Admittedly this effect is only clear at Skukuza, but I think still argues that you need to consider LAI when examining net ecosystem exchange.

10. P4080, line 27: 'calculated analysed' repetition.

11. P4081, line 20: As a general statement ALL the fpar seem low to me. (Note: please specify that these are the MAXIMUM fpar observed using satellite data during, what? The year of measurements, or averaged over several years). We are talking about a maximum LAI of approx 1 at Agoufou, increasing to at least 4 (probably 5-6) in the Congo grassland site. Thus we should have a range of fpar that is higher than in Figure 8c. If asked to guess I would suggest we need fpar ranging 0.3 to 0.95 across these sites. It might be a good idea to check these fpar data. Are they maximum annual? (they should be if you compare to maximum photosynthesis which occurs near the time of maximum green LAI). Would another fpar source (e.g. MODIS) be better?

11. P4082, line 6: 'climate-change-driven' does not read well here. Do you mean 'climate-change pressure'? In which case delete 'driven'.

12. P4083, lines 15-20: Is the discussion of correlation between maximum photosynthesis and maximum stomatal conductance really that interesting? Both are mainly driven by LAI, and more particularly, we know that CO_2 and H_2O follow a similar dom-

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inant pathway in leaves, thus they are sure to correlate. Perhaps the extent to which the slope deviates from the theoretical (stomatal diffusion coefficient) ratio may provide some information on how much these fluxes are dominated by leaf, as opposed to soil, fluxes...?

13. P4084, line 1-2: This suggests a misunderstanding of the Sankaran et al. analysis. Only a small fraction of savanna locations in Africa appear to 'make it' to the upper (potential) rainfall-related maximum. Most points are far below that potential. If your relationships are 'similar' to the Sankaran relionship I suspect it is superficial. Firstly, your sites are not necessarily anywhere near their potential maximum cover (I suspect they'd be well below it, but it would be interesting to plot them on the Sankaran plot to see), and your fpar measurements (though apparently too low) also include the grass component. You may be finding a near-linear relationship primarily because grass cover tends to increase with declining tree cover, so grass may be taking the place of trees when a site is below the 'potential' tree cover line.

14. Table 1: Agoufou site: check with Eric regarding species composition. These days I'd wonder if Leptadenia pyrotechnica and Callotropis procera are likely as, or more, important than C. glutinosum and Acacia.

15. Table 2: Skukuza Sonic was a Gill WindMaster Pro. until replaced by a Campbell in mid 2006.

16. Table 3: write a, m and b, d. As currently written these appear to be 'a divided by m' and 'b divided by d'.

I think all references to fapar (in this table and in text and figures) should specify that (I think this is true) you have selected the maximum value from one year (or across many years...?).

17. Figure 1: the distance rings obscure the vegetation type colors. The distance rings appear to serve no purpose in the paper and should be removed from this plot.

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18. Figure 6. Move the () to enclose all of the following '(Rg$>$500 wm-2 and water vapour pressure deficit $<$ 2kPa)';

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