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Biogeosciences Discussions

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

L. Merbold et al.

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First of all, we would like to thank the reviewers for their helpful comments and suggestions for improving the manuscript. Your help and feedback is highly appreciated. General comments Both reviewers mentioned the issue of fAPAR and LAI and it seems appropriate to begin on this matter. Thereafter, we will go through each of the reviewers' comments step by step and how these will be addressed in the revised manuscript.

fAPAR data used in the manuscript were downloaded from the JRC database (http://fapar.jrc.it/). Data were available for all sites using precise geographic coordinates from September 1997 till June 2006. We used peak growing season 10 day averages (an additional column, containing n will be added to table 3) when available for the years of measurements. If data were not available for the exact year of measurement (e.g. Niger Wankama sites 2007) we took averages from the previous years.

Detailed information on the JRC product can be found on the official webpage (see above).

Referee 1 argues right when saying that LAI seasonality is important when interpreting flux dynamics. The problem of this study - like many integrative studies - is that there are only few ground measured data of LAI available. Therefore, we used satellite products as a proxy. The procedure to avoid a bias from seasonality was to go for the growing seasonal peaks of FP, GS, LAI (where available) and fAPAR. The procedure can be shown in Figure 2 taken from Kutsch et al. (2008, Biogeosciences 5: 1797-1808, this issue). In phase 3 the seasonal peak of FP occurs, and fAPAR was taken exactly for this period, even if there were slightly higher values some days before.

In the revised manuscript, we will include a more in-depth explanation of the procedure and we will include a Figure showing peak season LAI (derived from direct measurements) vs. Fpmax for 6 sites, giving a similar relationship as found by fAPAR.

Furthermore, the light response curves of GPP will be plotted only from these peaks in the analysis of the fluxes to avoid confusing graphs overloaded with unnecessary information.

However, it has to be mentioned that LAI as well as fAPAR is not the 'silver bullet' in explaining the seasonal or inter-annual variation of photosynthetic capacity. There is huge variation at the physiological scale of leaves that is not visible when measuring LAI.

Further on, Referee 1 comments: 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

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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).

We have re-analysed the complete data set during the past weeks and fitted the ecosystem model after Reichstein et al. (2005) for each site. This model varies the temperature response of REco with changing soil moisture and also allows varying temperature and moisture corrected respiration (R0). Therefore, we can analyse the seasonal variation of activity, biomass or - for my sake - LAI. However, since R0 is a theoretical parameter, it might not be a good tool to characterize the seasonal course of respiratory capacity of an ecosystem. However, we will compare the values to LAI dynamics where available and will present the results (if any) in the revised version.

GPP has been derived on the base of NEE measurements and REco from the re-analysis. In order to derive photosynthetic capacity, it will be analysed only for the peak season.

In this context Referee 2 states: Regarding the large standard deviation in Gsmax (Figure 8b), could you reduce this but still retain a physiologically representative Gsmax by excluding days with rainfall and the two days after? That seems simple enough to do, and though it risks underestimating Gsmax, there is an equally large or larger risk of overestimating Gsmax by including days on which evaporation of intercepted and ground ponded water is occurring.

We agree on that comment and will exclude days with or shortly after rainfall. However, for some sites rainfall data are not available. In this case we are going to present the full data set and will comment on that.

Answers to Specific comments:

Referee 1:

1. Introduction: The discussion of the 'important approaches to understand the varia-

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tions 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 would also be appropriate here.

The revised manuscript will avoid confusing phrases. We will also refer to Williams et al 2008, which was - by the way - published after we submitted this manuscript. From this paper we can derive an important motivation for our study, since Williams et al. provide a pure modelling study and shows the demand for real-world data.

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.

Well, the functions (fluxes), feed the structure while the structure controls the functions (e.g. Kutsch et al. 2001, Ecosystems). This comment is right insofar that the wording is confusing. We will provide a clearer introduction in the revised version.

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.

Up to now this was only discussed qualitatively. We discussed to change the corresponding graphs in adding the site Kelma at the end of the rainfall gradient, after getting the effective rainfall from the site PI. However, then we should integrate effective rainfall for all sites and this would require detailed information on site hydrology that has not been available everywhere. Finally we decided to keep the figure as it originally was but add an arrow marking a second point that represents the most probable rainfall + runoff value.

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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).

We added several of these references which might be important and help further understanding of this study.

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).

We agree with the reviewer and change this sentence in the revised manuscript.

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 is 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.

We already mentioned the LAI issue at the beginning of this final response and surely this effect is only clear for Skukuza. However, after having re-analysed all data and included seasonality in the respiration model, we compensated for the LAI effect.

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

OK. We removed calculated since analysed is more approporiate.

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

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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?

To the fAPAR issue once more. This is indeed maximum fAPAR for the years of measurements when available and if not for several previous years. Still, when averaging fAPAR values will become lower. However we checked this data again and found little deviations from the original coordinates. Finally the graph was adjusted to the new values

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

We changed the expression as proposed by the reviewer.

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 dominant 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...?

Well, is reducing everything to LAI really this straightforward? Flux control in biological systems happens at many scales and the physiological control at leaf level is by far more sensitive as the LAI. The reason is simple economy: building a new leaf is an investment for a plant. A species that dropped leaves during each dry period and build new ones every next rain would be extinct soon. Physiological acclimation by changing stomatal sensitivity is a more efficient way to bridge some days or weeks with dry conditions. Photosynthetic capacity is down-regulated in parallel mainly to decrease maintenance respiration and to avoid high irradiance damage during stomatal closure. To prove that this acclimation shows a consistent spatio-temporal pattern (as theoretically predicted) might be of minor interest for a micrometeorologist but of high interest for ecologists.

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.

Comparing the fAPAR vs annual rainfall graph to the tree coverage vs annual rainfall graph by Sankaran et al. is not this absurd since tree coverage, LAI and fAPAR are all depending on rainfall. However, we have to admit that we have discussed this in the wrong way: even though the fAPAR includes the grass component and the grass compensates the declining tree cover. The fAPAR should show the potential total cover and the sites should not deviate too much from the potential and indirectly somehow proving that the approach of Sankaran et al was right… We will discuss the comparison in that way in the revised version.

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.

Only few of the dominating species are given in Table 1. Of course there are more

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species abundant on each site.

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

Sonic type was changed. Overall specifications were made consistent.

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...?).

This was probably a misunderstanding of our Table. For the sites driven by rainfall the coefficient a; is relevant, whereas the coefficient m was used for the exponential relationship with temperature at the Congolese sites. Therefore m was explicitly written in the table for the concerning site. After having re-analysed all data, this table will be changed for the complete part of the calculation of the respiratory term.

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.

We included the distance rings, since this stresses the sparse available measurements over the African continent.

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

OK, done.

Referee 2:

1. Specific Comments: The most substantive concern regards the mismatch between what the introduction states is examined and what is actually investigated/analyzed. The final paragraph of the introduction mentions comparative analysis of seasonality but this is not in the manuscript. Similarly, "flux responses to variations in moisture in-

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puts... caused by changes from wet to dry season and vice versa" are similarly absent from the analysis. More importantly, it is stated that the authors seek to test whether precipitation and soil moisture are ... the overriding environmental controls on the seasonal variation in Net Ecosystem Exchange of carbon [dioxide] (NEE). This is not addressed! NEE is not analyzed except as 1) nighttime quantity, 2) the measured quantity from which ecosystem photosynthesis is examined. Same with "NEE-soil moisture relationships known from ...." This is not addressed and all of this needs to be revised.

We strengthen the introduction and replace NEE by GPP where appropriate.

2. While on the topic of fAPAR, the authors mention that they prefer fAPAR to NDVI because fAPAR avoids the problems of saturation at high values. Is this really true? If fAPAR is derived as a modeled transformation of ’observed’ NDVI, the degree to which the signal saturates is still defined by sensitivity of NDVI in distinguishing areas of high veg and leaf density versus less high. Please justify this claim or just exclude mention of NDVI vs. fAPAR and go with the one you chose.

We will exclude mentioning NDVI and be consistent using fAPAR.

3. Regressions could often be nearly equally well represented with lines rather than tanhyp (Fig 8c) or exponential (Fig 8a, Congo Tchizalamou outlier?)

Regression will be changed to line in the revised manuscript. Congo Tchizalamou is not an outlier, since vegetation consists of C4 grasses that are highly productive, in particular at this latitude receiving almost 1200mm of rain per annuum.

5. P4080, line 17: Is the lower bound from millet, or rather from the dry, open Acacia savanna as mentioned in the abstract? Inconsistent.

The lower bound is the millet site indeed assuming the thin stand density. High uptake rates were also observed in the open Acacia savanna, due to the C4 grasses between the trees.

6. Why label Figure8 as having parts a to d? Each figure seems to be independent

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really, and has its own caption and such.

Figure 8 is labelled a to d since these graphs go along with each other and should be printed together.

7. P4082, L26: The Scanlon 2006 reference is missing, and you might also use: Scanlon et al. 2002, Remote Sensing of Environment 82, 376-388.

OK. We added this to the references.

8. P 4084, Line 12: The geometry explanation about Kelma as an outlier is unclear. Maybe replace "This" with "This unique ordination of the Kelma data may depend..."

OK. We changed the sentence and added more information about this specific site.

9. Figure 6 could replace Fp with 'Canopy Photosynthesis'. Same for Figure 8 but with additional descriptor as maximum.

OK. Done

Interactive comment on Biogeosciences Discuss., 5, 4071, 2008.