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Biogeosciences Discussions

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Interactive Comment

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

Anonymous Referee #2

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General Comments: This manuscript investigates the degree to which annual rainfall, absorbed photosynthetically active radiation, and vegetation type (e.g. C3 or C4) explain between-site variation in ecosystem physiology with analysis from 11 eddy covariance stations across Africa. Though the basic, descriptive analysis offers little advance for process-level understanding, the work has strength in reporting observations of ecosystem productivity and respiration as they vary with aridity across an undersampled region. As such, this paper is a valuable contribution.

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 inputs... 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.

In the Introduction, the second approach mentioned as addressing variations of structure and function of African ecosystems seems to have the intention of describing the role of satellite remote sensing as a key vantage, but is weak in the way it fleshes out the true history of applications here. The third approach is suggested to be continental mass balance constrained by isotopes, which indeed makes a valuable contribution at a continental scale but is not of the resolution needed to get at spatial variation in ecosystem function across Africa, and it says very little about ecosystem structure or drivers. Finally, the fourth approach, eddy covariance observations of carbon and water exchanges between ecosystems and the atmosphere, is not suited, on its own, to addressing questions of structure and this feels like an unnecessary oversell. The question can also be raised about how this fourth method can be distinguished from the first, regarding "ground-based ecophysiological research trying to understand...". Overall, this frame is not very effective at communicating why the EC method offers something valuable and new.

In the abstract, and throughout the manuscript the authors refer to fAPAR as though it were a static constant, and reduce the wide seasonal and inter-annual variability in fAPAR to a convenient single value for a particular site to be used as an independent variable for describing ecosystem productive potential. However in section 1.4, the fAPAR is described as being derived from a growing season 10-day average. Did the authors chose a maximum fAPAR for the site? How are the fAPAR screened for cloud and aerosol contamination? What satellite sensor is used in the JRC product?

Correspondingly, is this fAPAR a reliable descriptor of structure and hence predictor of function?

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.

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

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.

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

Why label Figure8 as having parts a to d? Each figure seems to be independent really, and has its own caption and such.

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

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..."

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

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Discussion Paper

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