Comments to the Authors:

Review of "Tree-grass phenoloy information improves the light use efficiency modelling of gross primary productivity for an Australian tropical Savanna".

Firstly, I would like to congratulate the authors to a very interesting and well written manuscript. I truly enjoyed reading it and I learned a lot. However I have some questions that I would like to get answered before I can recommend this manuscript for publication:

This manuscript is not focusing on the EC based understory estimates of CO_2 fluxes, but I am still confused. The eddy covariance method is based on the assumption that measurements are done in the inertial surface layer, i.e. in the layer within the atmosphere where there are no vertical changes in fluxes depending on height of the sensors (Foken, 2008). This is not the case inside a canopy. Inside a canopy turbulence is very chaotic, and turbulent transport is much more efficient than above a canopy (Denmead and Bradley, 1987; Foken, 2008; Kaimal and Finnigan, 1994; Raupach, 1989). Additionally, there are sinks and sources in all directions in space. Fluxes can thereby basically come from any direction; the NEE estimated by the EC system is thereby not only a result of fluxes from the understory, it can equally as well be a result of respiration or CO2 uptake by the canopy cover above the EC system.

(P8 L20) In case you do not include the reflected PAR in the estimates of fraction of absorbed PAR (FAPAR), it is not FAPAR, it is the fraction of intercepted PAR (FIPAR). This is not the same thing. Generally, FIPAR is much more stable over the seasons than FAPAR, and this can make a difference in the estimate of the seasonal variation in GPP. Why did the reflected PAR data result in negative values during the dry season? It indicates some issues with the calibration of the sensors. Is there no way to inter-calibrate the sensors and recalculate the data? FAPAR is generally estimated as:

$$\mathrm{FAPAR} = \frac{\mathrm{PAR}_{\mathrm{in}} - \mathrm{PAR}_{\mathrm{ref}} - (1-\alpha)\times \mathrm{PAR}_{\mathrm{tr}}}{\mathrm{PAR}_{in}}$$

Where PAR_{in} is incoming photosynthetic active radiation (PAR), PAR_{ref} is reflected PAR, α is PAR albedo of the soil, and PAR_{tr} is PAR transmitted through the vegetation.

I do not understand how the model can overestimate the GPP? You estimate a maximum LUE based on an average LUE for Dec-Mar. Then you use scalars with a value of between 0 and 1 to downscale the maximum LUE to a lower value. But since maximum LUE is based on the same time series of GPP as you use for the evaluation, it should not be possible for modelled GPP to be overestimated. Or did I misunderstand something? Please clarify.

Specific comments:

L11, it sounds like all grass in savannas is C4 species, which is absolutely not the case. Please just rephrase a bit.

P6 L1 Please describe very shortly the partitioning method used. Was it based on a light response curve or night time NEE-temperature curves?

Generally in the method section there are very many technical details. These are nice to have, but I think they could be moved to supplementary material to ease the reading of the manuscript. But, it is ok the way it is now as well, it is just a suggestion.

P9L18 APAR is in MJ **d-1**.

P5 please indicate the study period of the EC measurements, and other measurements by the way.

P9 L24 Why is n=8? In the figures it looks like the measurements started in January 2013, which would men n=7?

P9 L22 Why did you bin the LUE to months, this does not necessarily give the best indicator of maximum LUE. I would say that better would be to use a running mean for the estimates of seasonal dynamics in LUE, and then use the maximum value. Why should the average of 3 months give the best estimate for a maximum?

P16 L 17 Why did you use GCC as a proxy for FAPAR, and not as a scalar for LUE? There is strong seasonal variability in LUE depending on phenology of the vegetation, so I would think that it is more realistic to use the phenology as a direct scalar on LUE.

P10 L29 I assume that the regression was not used to replace APAR, but to replace FAPAR?

P12 L34 What limitations?

P13 L4 I would not consider a R2 value of 0.09 and 0.23 a well correlated relationship. These relationshis are not well correlated just because the p-value is significant. The assumptions for testing of significance is not fulfilled; there is high auto-correlation present in eddy covariance time series, so the true N is nowhere near the observed N. For example, Desai (2014) addresses this issue using a reduced degree of freedom calculation to show that the vast majority of flux tower regression is actually over-confident.

Fig 8-10. I suggest to incorporate subplots just like you did in Fig 7. Where you include a subplot with modelled GPP on the y-axis and the measured GPP on the x-axis. This really helps to see how well the models perform.

P15 L25-L27 Are you certain that RMSE is higher for the GCC included model (RMSE =1.43) than for the GCC and EF combined model(RMSE=1.36)? When looking at Fig 8 it does not look like RMSE can be higher. In Figure 8, it looks like the errors are much smaller; this should also be seen in the RMSE values.

References:

Desai, A.R., 2014. Influence and predictive capacity of climate anomalies on daily to decadal extremes in canopy photosynthesis. Photosynthesis Research, 119, 31-47, doi:10.1007/s11120-013-9925-z.

Denmead, O.T. and Bradley, E.F., 1987. On Scalar Transport in Plant Canopies. Irrigation Science, 8(2): 131-149.

Foken, T., 2008. Micrometeorology. Springer-Verlag, Berlin, 306 pp.

Kaimal, J.C. and Finnigan, J., 1994. Atmospheric boundary layer flows: their structure and measurement. Oxford University Press, Oxford.

Raupach, M.R., 1989. Applying Lagrangian fluid mechanics to infer scalar source distributions from concentration profiles in plant canopies. Agric. For. Meteorol., 47(2-4): 85-108.