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# *Interactive comment on* "Ideas and perspectives: How coupled is the vegetation to the boundary layer?" *by* Martin G. De Kauwe et al.

**Martin G. De Kauwe et al.**

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We thank the reviewer for their constructive comments and we address their various concerns below. Referee comments are highlighted (R), with our response below (A) in each case.

R: This paper leverages the new FLUXNET2015 dataset to estimate differences in the decoupling coefficient across plant funcitonal types, with some additional discussion of how the coefficient varies in response to canopy structure and meteorological condition. The work builds off a previous study that highlighed the decoupling coefficient as a significant source of uncertainty in some model predictions (De Kauwe et al. 2013). The authors report that evergreen forests are more decoupled than previously thought,



and that the decoupling of grasslands depends on mean annual precipitation (among other results).

Overall, I think this analysis will be of interest to members of the observational and modeling communities, and the article is generally well written and the figures are clearly presented. I do have a few suggestions for the authors that would allow them to bridge what I perceive to be a bit of a gap between the rational/objective of the paper and the interpretation of results.

First, the authors aimed to "examine if decoupling coefficients from FLUXNET were consistent with the literature values." However, the comparison of the decoupling coefficients derived from FLUXNET data and literature values was largely qualitative. The comparison would be more informative if values reported in the literature (or assumed by the models) were presented alongside those derived from the Flux data (for example, by including a bit more information in the box and whisker plots of Figure 1).

A: We agree with the reviewer's suggestion about adding information about values from the literature to the graph and will add this in our revised version. We also agree with the reviewer that information on what is currently assumed by models would be a nice addition to the figure; however, this information is not available as it is rarely (if ever) reported. We recommended this information be reported following our analysis of several models in a recent model intercomparison (De Kauwe et al. 2013, Global Change Biology) but our insight into model assumptions is still limited to the models considered there.

R: Second, the authors aimed to "develop a benchmark metric against which to test model assumptions about decoupling." Presumably this "benchmark metric" is the range of decoupling coefficients presented in the results. Would it be possible for the authors to demonstrate, at least at a few sites, that using a decoupling coefficient informed by the results of this study indeed improves agreement between the predictions of at least one model, and observations (for example, flux tower observations of ET)?

A: Whilst we obviously see value in what the reviewer has suggested, we feel this is actually a very separate piece of research. What we have aimed to do here was to raise the issue again in the literature by developing a potential benchmarking metric (Fig. 1) for models. We have written this up as an "ideas and perspective" and not a full research paper specifically for this reason. The next logical step would be to determine what state-of-the-art models are currently assuming by comparison, but that needs to be an exhaustive study. Little insight would be gained by playing with one model quickly in isolation: models often get the right answers for the wrong reasons, and we would want to guard against this. We will add a suggestion along these lines to the discussion to indicate directions for further research.

R: I was also curious about the author's choice to limit the analysis to relatively windy periods between 800 and 1600 hours. Coupling should be greater during these condition when compared to relatively stable conditions, for example those experienced from late evening to sunrise. Do the models similarly use a decoupling coefficient that is most appropriate for those conditions, or do they perhaps employ a lower value that is representative of daytime and nighttime periods (particularly if the models run at a daily timestep)?

A: This is an interesting question. The simple answer is that we deemed daylight hours and timesteps with $u^* < 0.25$ to be the period when stomatal control over transpiration would be strongest. Additionally, we would assume that in more stable conditions the errors in FLUXNET data would be greater (due to reduced turbulence), so have avoided this period.

R: Further, I though the authors might have missed an opportunity to leverage the high-frequency data from FLUXNET to say something about temporal variation in decoupling over the course of a typical day.

A: This also would be a valuable direction for further research. Decoupling factors are not constant: both models and data will show variation during the course of the day.



Our goal here was simply to examine how long-term average decoupling factors compare with literature values. Future more detailed work could examine the sub-diurnal variation in both models and measurements. We will also add this recommendation for future research.

R: Finally, in paragraph 10, the authors state that LAI information for many sites is not available. Many FLUXNET sites have high-quality ground-based LAI measurements that are not reported to the network. Sometimes an email to the site PIs can turn up useful ancillary data.

A: Firstly we note that the recommendation to email site PI's is unrealistic given our analysis covered 175 sites. We did carry out the suggested analysis using an ancillary file of LAI data from the FLUXNET sites. We have a number of concerns about presenting these data however. The file we used is no longer available online, and we have little information about what the data represent. We do not know how or when these data were measured (LAI-2000, hemispherical photography, other?), we do not know if they are LAI or really plant area index (i.e. not corrected for a woody component, or clumping), we do not know the sampling footprint these data represent and finally we cannot trace the origins of these data. For these reasons, we chose to instead analyse the decoupling in relation to precipitation (a proxy for LAI). We show the reviewer this figure below and as they can see there is some agreement with our figure 3, but due to the issues we raise above we feel it is more appropriate to stick with our analysis framework. If the reviewer wishes we could include this in the supplementary.

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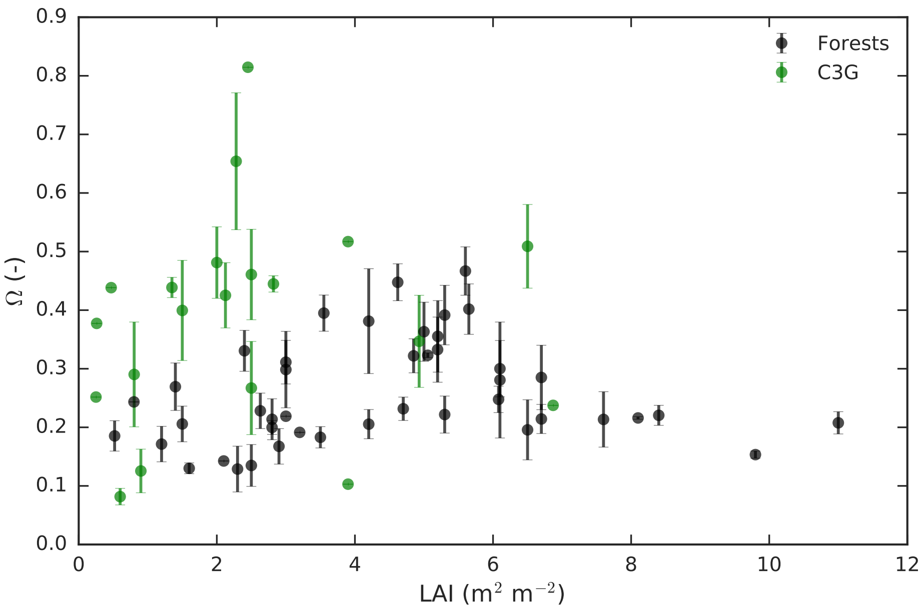


**Fig. 1.** Values of the estimated decoupling coefficient (Ω) for forest (ENF, EBF, DBF, TRF) vegetation and C3 grasses as a function of LAI. Plant functional types are defined as: C3G - C3 grass, ENF - evergree