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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.

Anonymous Referee #1

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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

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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).

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)?

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)? 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.

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.

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