

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

Anonymous Referee #2

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This manuscript presents results from a FLUXNET based analysis on vegetation-atmosphere coupling of transpiration using the omega factor by Jarvis & McNaughton. Aggregating daytime data during the peak growing season across plant functional types (PFT), it was found that evergreen needleleaf forests (ENF) have a lower degree of coupling, and that evergreen broadleaf forests (EBF) and shrubs were more coupled than previously suggested in the literature. The manuscript concludes that this decoupling analysis based on FLUXNET data can be used for benchmarking to test models. The manuscript is overall well written (particularly the Discussion section) and the presented research is of significant scientific interest to improve model estimates of biosphere-atmosphere exchange. Nonetheless, I do have some concerns regarding the argumentation and analysis presented here and would strongly encour-

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age the authors to consider the following points, before a revised manuscript could be recommended for publication.

Main Points:

(1) While the manuscript is overall focused on the coupling of vegetation and atmosphere regarding transpiration, the manuscript incoherently switches between the use of the degree of coupling and decoupling, which all refer to omega values between 0 and 1. Although this is linked to the original work by Jarvis & McNaughton (i.e. the decoupling factor), it seems rather confusing for readers of this manuscript and I would suggest using a consistent terminology throughout the manuscript, e.g. the degree of coupling with high omega values referring to a lower degree of coupling.

(2) As the manuscript heavily relies on turbulence based measurements from FLUXNET, there is a high chance that the coupling terminology might be misunderstood. It would help and strengthen the manuscript to more clearly differentiate in the Introduction section, if your terminology of coupling is referring to turbulence conditions above the plant canopy (e.g. quantified by u^* or σ_w) or to plant physiological coupling at the leaf level or within the canopy, or between different layers of the canopy such as in forests and woody shrublands. This seems also important to differentiate between the leaf and ecosystem scale in this manuscript as EC flux measurements are at the ecosystem scale, yet some of the presented concepts here are referring to the stomatal coupling at the leaf scale (typically measured by leaf chamber).

(3) The manuscript currently relies substantially on comparisons of FLUXNET derived values to the literature, yet the literature values are not presented and analysed quantitatively. I would suggest considering a figure or table comparing both by PFT and documenting details of the so heavily referred to values from the literature, e.g. on how these were assessed/derived (single site/plant experiment, multiple sites, chambers, EC, season etc) to give readers a better idea of their origin and meaning. The manuscript draws substantial conclusions from the comparison to the literature values

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and these needs to be justified accordingly in a quantitative way that is clearly visualized.

(4) The FLUXNET La Thuile data used here is relatively outdated (from 2007) and only includes a limited number of sites (as Free and Fair use subset). Yet the newer and more extensive FLUXNET2015 dataset is available since late 2015 (same website as referred to in Methods section), but including many more sites and site years compared to the 2007 La Thuile dataset (~1000 vs. ~1500 site years) and also including a subset with a similar data policy (TIER1). I am wondering what the reasoning behind this choice of older dataset was and if the manuscript would not benefit from the larger sampling available in the newer dataset, particularly in terms of important PFTs (e.g. TRF) that were poorly represented in the 2007 dataset? It would also benefit the manuscript to have a table of the eventually retained sites (after data screening – see Section 2.1), their used site years and PFT etc. in the Appendix, something that is typically recommended when using the FLUXNET dataset.

(5) The manuscript correctly states (Section 2.3.1) that soil evaporation would bias the coupling estimates, yet it is assumed that this only matters 24 hours after rainfall. In fact soil evaporation is a substantial component of the measured ET at almost all sites and except in closest canopy forests with high LAI, easily contributes up to 50% of total ET, particularly in grasslands and shrublands. Consequently, the bias of soil evaporation on the results of certain PFTs is likely much higher and this needs to be addressed in the interpretation of the Results.

(6) The analysis on the controls of omega is largely focused on wind and precipitation, yet soil moisture and VPD seem much better and more direct controls of plant water stress affecting stomatal conductance. These data are available for most of the sites in the FLUXNET dataset and I would encourage the authors to consider extending their analysis to these controls, and linking these results to the recent literature on stomatal conductance.

Overall, I am aware of the length limitations of Opinion & Perspectives papers, yet a full length manuscript might be more fitting for this study to sufficiently document the analysis and the Conclusions that could be drawn from it.

Specific Comments:

- Page 1, Line 19: please consider adding short explanation why Gs is reduced with elevated CO₂.
- It would help to add some details in Section 2.1. why the flux data were screened this way and how this affects the interpretation of your Results. It would also be helpful to specify that your analysis is presenting mean decoupling values during the peak growing season somewhere in the Results.
- Page 4, Line 29: why are open grasslands necessarily sites with low precipitation?
- Page 4, Line 30: or are grasslands just more couple because of having just 1 canopy layer (compared to typically 2 in forest)?
- Page 5, Line 20: please consider removing “low” for consistency.
- Page 5, Line 21: SDGVM = Sheffield Dynamic Global Vegetation Model (add Global)
- Page 5, Line 30: it seems incorrect to write “all” FLUXNET sites her, as you are (i) only using a subset from the 2007 dataset and (ii) further reduce this subset by data screening (see Section 2.1).
- Page 5, Line 30: I would argue that “forest species” is not the correct term here as you are referring to PFTs, not species groups, and the flux measurements are at the ecosystem scale.
- Page 5, Line 31: consider limiting “..the FLUXNET network..” to “FLUXNET”.
- Page 6, Line 26-27: Ref. Knauer et al. missing in Reference list, and similarly the incomplete citation of Knauer et al. in Line 31-32.

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- Page 6, Line 32: “that” seems redundant here
- Section 2.3.1: what about the limitations arising from the use of an older dataset (despite availability of newer dataset, which poorly represents some PFTs?)
- Page 7, Line 8-9: what about general variability of environmental conditions and water availability?
- Page 7, Line 11: the BADM data of the new FLUXNET dataset is more extensive than previously and includes details on canopy height and LAI for many sites
- Page 7, Line 16-17: please specify how process understanding from leaf to canopy scale can be improved, if all the listed measurements are referring to the individual plant and ecosystem scale. Furthermore, such targeted Gs measurements have been performed at various sites already and it is not clear to me what new aspects the authors are suggesting here.
- Figure 1: C4 PFTs in caption but not displayed in Figure? Please add missing data or specify why these are not displayed. Ditto in Figure A1.
- Figure 2: please consider (i) moving site names outside graph as axis caption (i.e. this is a categorical axis), (ii) separating the three groups a-c by vertical lines, (iii) removing selective ticks on x-axis OR adding one for every single site, and (iv) adding details on the meaning of the whiskers in the caption text.
- Figure 3: please consider changing the colours so that these are easier to differentiate, and to change the symbols (i.e. different symbol for each PFT, and potentially increasing size). It could also help to differentiate each regression line with dashed/dotted display.
- Figure 4: why are the C3 grasses displayed in Fig. 3, yet not here? Also, what about croplands? I would also suggest to consider add the slope values here and in Fig. 3 for the regression lines.

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