Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-182-AC2, 2017

Biogeosciences Discussions

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

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Received and published: 20 July 2017

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 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 then previously suggested in the literature. The manuscript concludes that

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

Main Points: R: (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.

A: We are happy to switch our use of terminology to "degree of coupling", noting that these terms are used interchangeably widely across the literature.

R: (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 sigma 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).

A: We do not fully follow the reviewer's lack of clarity on this issue. We define our use clearly in equation 1, 2 and in particular 3 (which outlines the use of u*). Our approach following Jarvis & others and takes a big-leaf approach. We clearly address potential issues in this approach in our Caveats section (2.3.1). The ecosystem scale is an integration of the leaf-level processes and thus, reference to leaf / canopy processes is appropriate.

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

A: We are happy to add such a table that summarises information from the literature we reviewed. We have already indicated we would add this information (i.e. a PFT summary) to Fig 1 in response to reviewer 1.

R: (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 (âĹij1000 vs. âĹij1500 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.

A: We will follow the reviewer's request and add a list of the sites used in the analysis following screening to the appendix.

The FLUXNET2015 release is being made progressively, and hence the data available continue to change on a regular basis. When we originally carried out our analysis, the quality assurance flags for latent heat flux were missing, meaning that we could not carry out our analysis on the new release (a patch has now been released). Owing to the fact that this dataset is still changing, and its properties have not been explored or tested yet, we felt that it was more appropriate at this time to work with the well-known and studied La Thuile dataset. We note that just because there is a newer release, it does not invalidate the approach taken here. We are not the only authors to continue to use the La Thuile data (see for example in Biogeosciences discussions: Mahecha et al. 2017, doi:10.5194/bg-2017-130; Marcolla et al. 2017, doi:10.5194/bg-2017-11).

We have run a similar analysis with the FLUXNET2015 dataset (see figure below). Our conclusions are similar across the two datasets. In particular, the reviewer highlighted the greater number of tropical sites, but as can be seen from our figure, the change in site years is small (n=16 vs. n=9). We will add this as a supplementary figure to demonstrate this.

R: (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.

A: In fact, we screened data 48 hours after rainfall, not 24. There is a discrepancy in our text where we mistakenly state 24 hours in the Caveats section, but 48 in the method; we will fix this error in the revised version. Of course, our choice of 48 hours is an assumption of the method, but as we highlighted in the Caveats section, it is one that has been widely used (see Law et al., 2002; Groenendijk et al., 2011; Dekker et al., 2016).

As suggested, we will extend our Caveats section to highlight the reviewer's point that this assumption may vary with PFT. However, it is not clear to us where the reviewer's soil evaporation figure of "easily up to 50%" originates; the literature we have read points to transpiration accounting for between 60-80% of evapotranspiration across the land surface (e.g. Miralles et al., 2011; Jasechko et al., 2013; Schlesinger and Jasechko, 2014, but see Schlaepfer et al., 2014).

R: (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.

A: The effect of VPD is already accounted for through its use in equation 2. With respect to the reviewer's point about soil moisture, the focus of this manuscript was on boundary layer controls on stomatal conductance. There is already ample literature on drought and soil moisture. However, in revision, we will explore the suggested soil moisture output fields but would not expect these to reflect plant water stress for many deep-rooted forest species (FLUXNET fields refer to "upper layer/lower layer" without stating the explicit depth). The reviewer also raises a question below about general variability and it is likely that looking at these data for this question may be more relevant.

R: 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.

A: The main goal of this work was to document the degree of coupling observed at FLUXNET sites and demonstrate how it differs from the literature. We feel that the manuscript submission, even with the addition of the new summary table of literature decoupling, sufficiently addresses this goal in its current form. The additions requested by both reviewers do not appear to warrant a substantial extension in length of the paper.

Specific Comments: R: - Page 1, Line 19: please consider adding short explanation why Gs is reduced with elevated CO2.

A: We will add this.

R: - 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.

A: We will add an explanation sentence of why these data were screened this way. We will also add the requested statement to the results.

R: Page 4, Line 29: why are open grasslands necessarily sites with low precipitation?

A: The reviewer is correct that the open grasslands are not necessarily sites with low PPT; we will reword this sentence.

R: - Page 4, Line 30: or are grasslands just more couple because of having just 1 canopy layer (compared to typically 2 in forest)?

A: This sentence refers to the fact that grasslands at low precipitation are more coupled than grasslands at high precipitation; it does not compare grasslands with forests. Forests are typically more coupled than grasslands.

R: - Page 5, Line 20: please consider removing “low” for consistency.

A: We will remove.

R: - Page 5, Line 21: SDGVM = Sheffield Dynamic Global Vegetation Model (add Global)

A: We will add the missing global.

R: - 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).

A: We will replace “all” with “175 sites and 634 site-years”.

R: - 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.

A: We will replace “species” with “PFTs”.

R: - Page 5, Line 31: consider limiting “..the FLUXNET network..” to “FLUXNET”.

A: We will remove “network”.

R: - 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.

A: We will fix the Knauer et al. reference

R: - Page 6, Line 32: “that” seems redundant here

A: We will remove “that”.

R: 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?

A: See earlier response about FLUXNET 2015.

R: Page 7, Line 8-9: what about general variability of environmental conditions and water availability?

A: We agree with the reviewer that anything that alters Gs and thus the ratio of Gs to Ga, will also affect coupling. As stated above, in revision we will attempt to look at whether the FLUXNET outputs for soil moisture provide any additional constraint upon this.

R: Page 7, Line 11: the BADM data of the new FLUXNET dataset is more extensive then previously and includes details on canopy height and LAI for many sites

A: These data are not sufficient to probe the questions we posed, in many cases, particular with canopy/tower height, this information is simply not available at all sites (presumably this is covered by: (i) "At present only the variables of Site_General_Info and Disturbance_and_Management are made available; and (ii) "Additional BADM variables such as LAI, biomass measurements and soil characteristics will be added to the BADM files over time"). The LAI information is also problematic: 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 included a figure in response to reviewer 1 to demonstrate some agreement with our figure 3. However, 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.

R: 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.

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A: We will add further text to develop the point about leaf to canopy scaling and how additional measurements could be used to improve our understanding. In terms of gs data at various sites: it is true that there is some (8 sites - Medlyn et al. 2017, New Phytologist). However, these data are few and not freely available as part of FLUXNET. Moreover, the temporal and spatial footprint is often not sufficient to address the question we are raising, in no small part because these data were collected with different research questions in mind.

R: 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.

A: We will remove C4 from the caption; this was a mistake, as FLUXNET data does not distinguish between C3 and C4 pathways.

R: - 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.

A: We will change the figure as suggested and add the missing caption text.

R: - 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.

A: We will explore the reviewer's suggestion when revising this figure.

R: 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.

A: The aim of figure 4 was to probe the relationship between wind speed and coupling for forest PFTs. We will make this distinction clearer in revision.

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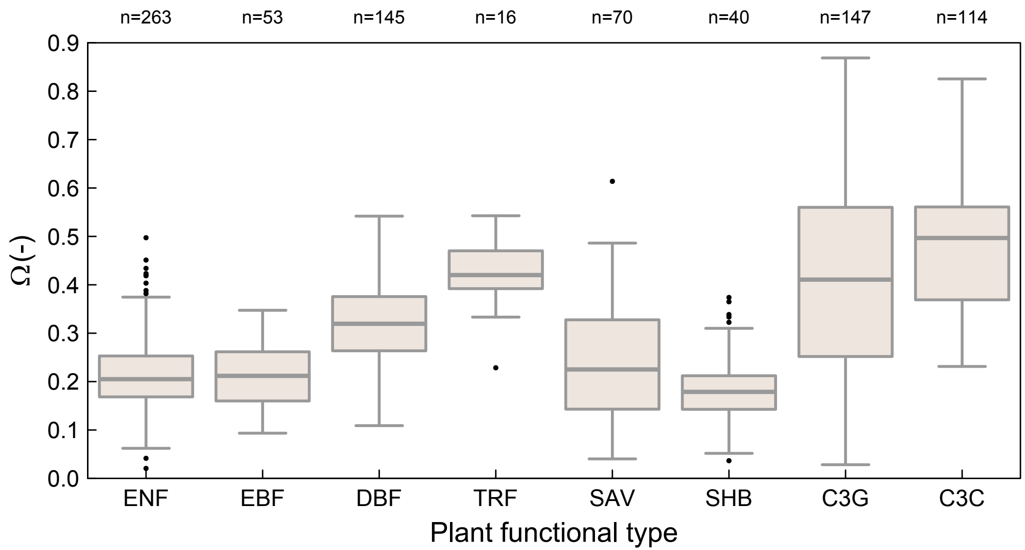

Fig. 1. As in figure 1 but using FLUXNET2015 data.