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Biogeosciences
Discussions

Interactive comment on "Source Partitioning of H_2O and CO_2 Fluxes Based on High Frequency Eddy Covariance Data: a Comparison between Study Sites" *by* Anne Klosterhalfen et al.

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Thank you very much for your review of the abovementioned manuscript. We have carefully inspected all reviewer comments. Below, you will find our responses to the comments (italic) and we describe how we will try to implement the suggestions made by the reviewers. As primarily suggested by Reviewer #2, we will review our writing thoroughly for a better communication of our findings, if this manuscript is permitted to further revisions. We will also improve the figures as suggested by both Reviewers.

We hope that you will find the result satisfying.

Sincerely,

Anne Klosterhalfen, Alexander Graf, Nicolas Brüggemann, Clemens Drüe, Odilia Esser, María Pat González Dugo, Günther Heinemann, Cor M.J. Jacobs, Matthias Mauder, Arnold F. Moene, Patrizia Ney, Thomas Pütz, Corinna Rebmann, Mario Ramos Rodríguez, Todd M. Scanlon, Marius Schmidt, Rainer Steinbrecher, Christoph K. Thomas, Veronika Valler, Matthias J. Zeeman, and Harry Vereecken

Referee #2

This study evaluates two approaches for partitioning eddy covariance fluxes into principle components (NPP and Soil respiration for carbon, and Transpiration and soil respiration for water). Both of the approaches (SK10 and TH08) rely on information contained in the raw, high frequency flux data, interpreted with assumptions about how the deviations in wind and gas concentrations should be correlated/coordinated for air parcels emerging from the canopy versus subcanopy. The developers of these approaches (Scanlon, Thomas) appear as co-authors on the paper, and the literature describing the approaches has been described elsewhere. Thus, while neither SK10 or TH08 is a perfect partitioning approach, I will focus my comments specifically on this effort to compare them (as opposed to comments about the underlying assumptions of each).
I applaud the authors for this ambitious undertaking; it is not easy to handle raw data from so many flux sites. Methodologically (with one exception I'll address later), the work is sound. While it's may be a bit disappointing that the results weren't in better agreement, I think the paper contains information that will be of interest and useful to the flux community.

However, in its present form, I'm not certain that information is being successfully conveyed. Following are some comments on presentation, analysis, and methodology that may help to make the more accessible to others in the community who are seeking ways to better partition their tower-derived fluxes.
Thank you very much for this constructive feedback.

2.1
First, the paper is hard to read at times. This is due to many factors, including:
1. heavy reliance on acronyms,
2. very detailed explanation of methodology (i.e. the description of the 'GMM' approach on page 5),
3. Very nuanced description of some results that isn't organized around clear themes or patterns, (for example, the site-by-site analysis of performance in section 3.1.1),
4. some issues with grammar, and
5) a few very long paragraphs (i.e. page 9), and a few very short and choppy ones (page 13).
I urge the authors to carefully edit the writing with an eye towards: 1) moving information that is tangential to understanding the results to the SI (e.g. the GMM method description), 2) organizing results around clear patterns, and reducing words spent on detailed description of the site-by-site, or method-by-method results, and 3) carefully reviewing the text for language.
We will review our writing thoroughly considering the above mentioned points.

2.2
Second, the figures are also difficult to interpret, often because there are too many panels. Some ideas for clearer presentation include:

2.2.1

Figure 2: Could the authors include fewer days of data, and perhaps consider omitting some of the different methods from the panel (for example, show TH08_REA_Q1 or TH08_REA_H, but not both). They seem quite similar.
Done. Fig. 2 shows now only 4 days of the considered time period and following methods: SK10 with WUE$_{meanT}$, WUE$_{MOST}$, and WUE$_{OLR}$, and TH08 CV Q1, REA H, and CV GMM (cf. comment 1.4.3 by Reviewer #1).
We added a figure for Loobos with results of all days and for every method version to the supplementary material.

2.2.2
Figure 3: Again, is it necessary to show each method's results?
Done. Fig. 3 shows now only following methods: SK10 with WUE$_{meanT}$, WUE$_{MOST}$, and WUE$_{OLR}$, and TH08 CV Q1, REA H, and CV GMM.

2.2.3
Figure 4: Since you've already shown some of the diurnal dynamics, perhaps this figure could present daily-averages?
With Fig. 4 we wanted to show at least once results of all study sites next to each other in the manuscript. Otherwise, we only show selected sites in the manuscript. We assume that daily averages would give a similar picture as Fig. 5.

2.2.4
Figure 5: This figure is nice! It might be helpful (in a separate figure) to also show the estimated ratio of E:T, as this is often reported in the literature (see, for example, Good et al. 2015, Li et al. 2019).
Thank you for this suggestion. Done. We changed Fig. 5 as suggested in comments 1.4.11 by Reviewer #1 and 2.2.4, also showing the partitioning factor E/ET. Also, we will include the suggested literature in our discussions comparing our partitioning

factors.

2.2.5
Figure 6: Averaging across sites (or at least across plant functional types) would make it easier to understand the performance of the different partitioning methods.
We agree that Fig. 6 is quite crowded, but averaging a performance metric/error quantity is not straightforward. It would probably require different strategies for the different error quantities and involve some arbitrary decisions. We see a high risk that the figure would be condensed at the cost of a much more difficult documentation of the methodology behind the figure. We would therefore prefer to keep it as it is.

2.3
Third, the authors focus most of their analysis on understanding differences in the magnitude of the partitioned fluxes (across a day, across sites). In my view, the magnitude of tower-derived fluxes will always be uncertainty, but as long as the sources of biases don't change too much in time, we can be more confident in using tower data to understand trends. How do these different partitioning methods agree in key functional relationships (for example, NPP versus PAR, Surface Conductance versus VPD)? Are the recovered trends as expected?
Thank you for this nice idea. We will have a closer look at such key functional relationships. Unfortunately, we cannot yet estimate, if such key functional relationships can be easily identified in our data because of too narrow ranges in the data or many additional and confounding factors (e.g., the relationship between NPP and PAR is also dependent on vegetation water status). As an example, Fig. 1 (below) shows the relationship between the averaged partitioning factor E/ET and LAI.

Fig. 1: Relationship between averaged partitioning factor E/ET (fraction of evaporation in evapotranspiration) and leaf area index LAI. Left diagram shows partitioning results

of the method versions after Scanlon and Kustas (2010, SK10), and the right diagram of the method versions after Thomas et al. (2008, TH08). Green markers indicate forest sites, blue grassland sites, and yellow cropland sites.

2.4
Fourth, I was confused by the HiP GPP and TER metric. . .it seems like the authors are filtering the data to consider only periods when the partitioned fluxes are similar in magnitude to those expected from conventional partitioning approaches (which are highly uncertain), and then using those filtered data to evaluated the partitioned fluxes? This seems like an approach that may obscure problems in one or the other partitioned fluxes. . .I would suggest a more straightforward comparison between the NPP and GPP (without the HiP) filtering.
We are sorry if the first manuscript version gave rise to a misunderstanding. The "Hit in Range" (HiR) criterion was solely used as one of three evaluation criteria (partitioning results in reference to Rsoil chamber measurements, HiR with respect to the approach after Reichstein et al. (2005), Esoil estimation according to Beer's law). It was NOT used to filter the data before any of the other analyses presented in the paper. We are aware that all of the abovementioned reference methods have their issues, which is why we used multiple of them and discuss them carefully.

2.5
Finally, are there any independent estimates of WUE (for example, from gas exchange data) in these sites, or similar biomes, that could provide a reality check on the towerderived WUE estimates?
We will conduct a more thorough literature search concerning estimates of WUE on leaf-level and extend our discussions. Unfortunately, no direct measurements of WUE were conducted at any study site.

Work cited: Li et al. 2019. A simple and objective method to partition evapotranspiration into transpiration and evaporation at eddy-covariance sites. Agricultural and Forest Meteorology. https://www.sciencedirect.com/science/article/pii/S016819231830371X?via%3Dihub
Good et al. 2015. Hydrologic connectivity constrains partitioning of global terrestrial water fluxes. Science. http://science.sciencemag.org/content/349/6244/175

Thank you very much for your comments and your time!

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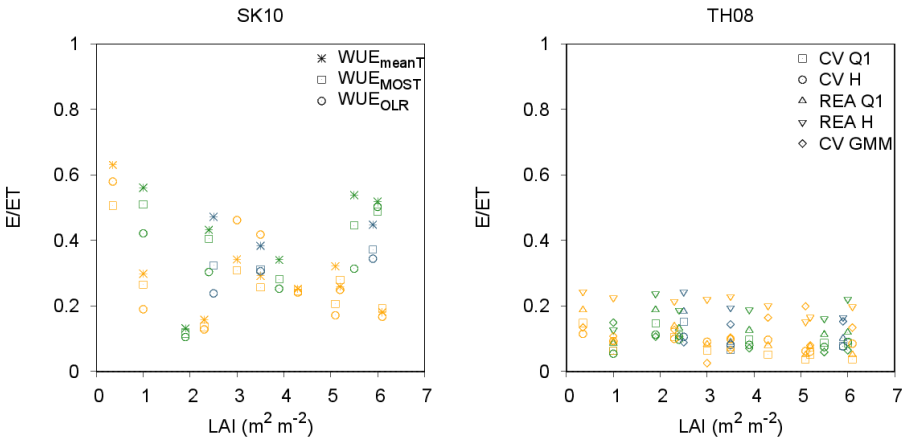

Fig. 1.