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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.

Anonymous Referee #2

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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.

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.

Second, the figures are also difficult to interpret, often because there are too many

panels. Some ideas for clearer presentation include:

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.

Figure 3: Again, is it necessary to show each method's results?

Figure 4: Since you've already shown some of the diurnal dynamics, perhaps this figure could present daily-averages?

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).

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.

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?

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.

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 tower-derived WUE estimates?

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

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