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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.

**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.*



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Discussion paper

*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 #1**

This manuscript presents a comparison of two turbulence-based flux partitioning methods across multiple sites representing a range of vegetation types (forest, grassland, and crop) and geographic zones. These emerging flux partitioning methods represent an effort to develop partitioning strategies that do not require assumptions about functional relationships, and this comparison between two methods across sites is a highly valuable contribution to the continuing development of new flux partitioning strategies. I have not seen a comprehensive comparison of two turbulence-based partitioning methods like this, and I think it represents an important step forward in understanding the performance of these methods. The comparison of multiple variations of each method and the analysis of specific site factors such as LAI and canopy height and how they affect the methods are especially valuable contributions to development of these partitioning strategies. I thought the manuscript was clear, easy to follow, and well written overall. I only have a few comments for areas where the manuscript could be improved:
*Thank you very much for this positive feedback.*





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*1.1*

1. The manuscript refers several times to a manuscript by the same first author that is still in review in another journal. Until that manuscript is available to readers of this manuscript, I don't think it's useful to cite it. In particular, methodological details that have a bearing on this manuscript should be included in the supplemental material or main text, and not only cited to another manuscript that is not available at this time.

*The cited paper was accepted just recently and is now available online. We updated the reference in this manuscript.*

*1.2*

2. Tables 2 and 3 and A1 highlight the highest and lowest values of the metrics that they show. This makes it easy to ignore cases where there are multiple high values. It would be better to color code all the cells in the table based on their values, so readers could tell at a glance how the values looked. In addition, I think the correlations in Tables 2 and 3 should show whether they were statistically significant using bold text or asterisks.

*Our original versions of Tables 2, 3, and A1 were in color. But as far as we know (after contacting the Journal's Typesetting Department) tables in color are not possible.*

*In Tables 2 and 3, we also added asterisks for statistically significant correlations. Because the sample sizes were small and the data was often not normally distributed, the results have to be handled with care.*

*1.3*

3. The analysis used the ratio of LAI to canopy height as one of the predictors because "LAI can correlate with hc of a study site" (page 11, line 1). But LAI does not appear to be strongly correlated with hc for the sites in this study. Unless there is a strong relationship, this ratio seems difficult to interpret and I'm not sure I would include it in the analysis unless there is a clear interpretation.





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*The ratio of LAI to canopy height ($h_c$) was used because it corresponds to plant area density. Considering only the study sites of one ecosystem type (forest, cropland, or grassland), correlations between LAI and $h_c$ can be found (see Fig. 1 below). For croplands, the correlation was weak, because for maize and sugar beet $h_c$ increased and LAI decreased with increasing maturity. Also, in this subset of sites in this particular study the maize crop in Dijkgraaf (DI_CL_MA_07 and DI_CL_MA_08) was a special case regarding its large (and expected) $h_c$. The correlation for grasslands was negative because of the very small sample size and different management strategies (dates of cutting) for each grassland, which influence both, LAI and $h_c$.*
*For clarification, we rephrased the following section: "For the chosen study sites, LAI correlated with $h_c$ when considering a specific ecosystem type (forest, cropland, or grassland). Thus, LAI $h_c^{-1}$ was also considered to distinguish between their impacts on partitioning performance."*
*We also think that this LAI-$h_c$-ratio may be useful for comparison to additional study sites. Thus, we would like to further include it in our analysis.*

*Fig. 1: Correlation between canopy height ($h_c$) and leaf area index (LAI) for each ecosystem type (FR: forest; CL: cropland; GL: grassland; R: Pearson product-moment correlation coefficient). Lines show reduced major axis regressions (after Webster 1997, European Journal of Soil Science 48:557).*

*1.4*
Technical comments:

*1.4.1*
Page 7, line 5: Does "two models" refer to the two partitioning methods? They are not referred to as models elsewhere in the manuscript
*Yes. For clarification, we rephrased the sentence to: "Again, within this evaluation step*





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*two source partitioning approaches (SK10 or TH08 versus the approach after Reichstein et al., 2005) were examined and compared including their different assumptions and uncertainties,. . . .”*

*1.4.2*

Page 7, line 21: What distribution were the random numbers sampled from? Normal? If so, what were the mean and standard distribution?

*Yes. For clarification, we modified the sentence to: “To each generated data point of w’, q’ and c’ a random number, sampled from a standard normal distribution and rescaled to a standard deviation of 5% of the magnitude of the variable, was added to simulate additional sources of variance not related to the degree of mixing.“*

*1.4.3*

Page 8, lines 7-8: This should include a brief explanation of why that site and those methods were chosen for the examples. Presumably because those methods had the best performance?

*Done. We included following explanation: “In the following, figures are shown for some selected sites, which represent the overall results of all study sites the most, and/or for some selected method versions of SK10 and TH08, which usually presented the best partitioning performance.”*

*1.4.4*

Page 8, line 21: It should be “fewer data points”

*Done.*

*1.4.5*

Page 8, line 28: “TH08 REA H performed best” needs more explanation. Based on





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what metric? Did it perform best for all sites and metrics, or a subset?
*Done. We included following explanation: "Regarding the error quantities in Fig. 6, TH08 REA H, among all TH08 method versions, yielded the best result for the largest number of sites and error quantities."*

*1.4.6*
Page 9, line 3: The title of this section suggests that the following text will focus on comparing partitioning results to published analyses, but only a couple of the sites compare directly to publications. It might be more accurate to describe this as a detailed description of results for each site.
*We changed the header to "Evaluation for Each Study Site" and we will try to include further descriptions and valuable information of the partitioning results for missing sites.*

*1.4.7*
I think this paragraph should include a reference to Figure 5, since the bar plots are a helpful summary for many of the results described here.
*Done.*

*1.4.8*
I think this paragraph would be easier to follow if the supplementary figures were in the same order that they were referred to in the text.
*We organized the figures in the supplementary material as the study sites are listed in Tab. 1 (organized by first canopy type and second latitude). Based on your comment, we will try to reorganize the description and evaluation of the study sites in the text after the same scheme.*

*1.4.9*





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Page 10, line 13-14: “both methods converged”: It’s not clear how they converged, or how that is shown in Fig. 6c and d.
*For clarification, we rephrased the sentence as follows: “When using the gap-filling model after Reichstein et al. (2005) as a reference, high HiR GPP were relatively frequent for TH08, with a minimum of 66.7% for SE_CL_SB_06, while HiR GPP for SK10 were considerably lower (Fig. 6c). For HiR TER, such a clear difference in performance could not be observed (Fig. 6d).”*

*1.4.10*
Page 11, line 29: It’s not clear how this was contradictory. Contradictory relative to what?
*We rephrased the sentence as follows: “Also, the correlation between partitioning performance and LAI $h_c^{-1}$ at forest sites contradicted our assumption that a higher plant density would have a negative effect.”*

*1.4.11*
Figure 5: It is difficult to compare the two partitioning methods to each other across panels b and c. I suggest putting the two partitioning methods in the same panel so they can be directly compared, given the importance of these comparisons to the results. Perhaps panel b could show C fluxes and panel c could show LE, with bars for the two partitioning methods side-by-side in each panel.
*Done. We changed Fig. 5 as suggested in comment 1.4.11 and 2.2.4 by Reviewer #2.*

*1.4.12*
Table 1: The abbreviations in the site column need to be defined (NL, ST, DE, PNL,: : :). Some of these are countries, some are regions, and some I didn’t understand at all.
*We adjusted the Tab. 1 mentioning only the countries. For a more pleasant reading, we will also change the acronyms of the study sites as suggested in comment 2.1 by*





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*Reviewer #2.*

*1.4.13*
Page 29, line 3: The blue and red lettering is not in the table. As I said above, I think color coding would be a good idea but it would be better to reflect the actual values rather than just where the highest/lowest value is.
*Thank you for noticing this mistake. The reference to the blue and red lettering in the table's caption was the description of the original colored table and was forgotten to be removed while changing the table format (cf. comment 1.2). As far as we know, tables in color cannot be included in this journal.*

*Thank you very much for your very constructive comments and your time!*

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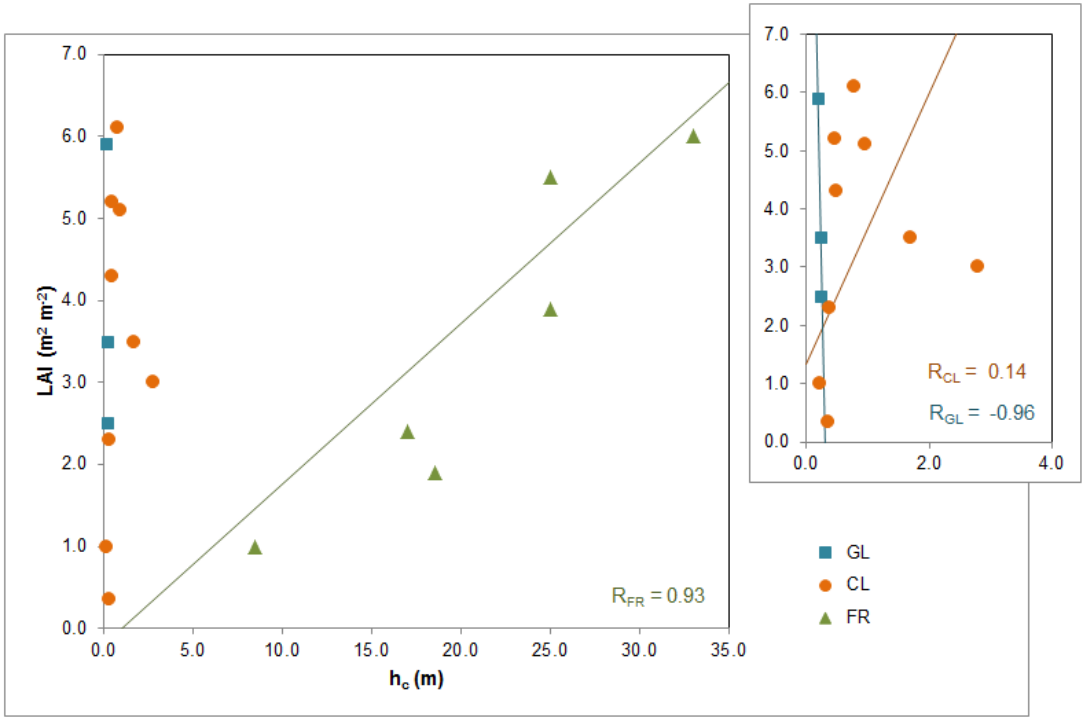


**Fig. 1.**



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