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Biogeosciences Discussions

Interactive comment on "On the choice of the driving temperature for eddy-covariance carbon dioxide flux partitioning" *by* G. Lasslop et al.

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Response to reviewer 2

We thank referee 2 for the review and constructive comments. We thoroughly edited the manuscript to improve clarity as suggested. In addition we have been advised that all manuscripts in BG undergo copy-editing by default.

In the following we cite and reply to each comment of the review. The comments of the reviewer are cited in italic font.

". . . wonder if it not also possible from their results to try to reach a second simplerconclusion - to use the Lasslop partitioning method with Ta as the driving variable."

We suggest using an ensemble of approaches as the divergence of estimates can serve as an indicator of uncertainty. Another question is of course which of the methods gives the best estimate. We do not see a way to answer this question with our analysis (nor how we could extend it for that purpose). Soil respiration data can give additional insights but it is also not possible to interpret them as a reference or "truth", as they also have uncertainties and they don't include the autotrophic respiration of vegetation. The answer to the question which estimate is better will be strongly site dependent. One reason for this is that the light response curve has the disadvantage that it needs more driving data, therefore the quality of the results depends additionally on the quality of radiation and VPD observations. The Reichstein et al. (2005) algorithm only needs temperature observations.

"I am surprised that the evaluation of time lags was limited to the comparison of Ta and Ts, rather than evaluating lags in the NEE-T correlations, including the possibility that Ta may lead NEE. Did you evaluate lags in NEE-T?"

We did not evaluate the lags in NEE-T. Mainly because this manuscript focuses on the use of temperature observations for two flux partitioning algorithms. Determining a lag for each data window and considering it in the partitioning algorithm would cause gaps in the temperature time series at the edges of the windows if the lag differs. Having to deal with these gaps would introduce artefacts into the time series of the flux components.

"When Topt was used, was the weighting parameter allowed to vary in time or was asingle value computed? If it was allowed to vary, did the weighting parameter show any consistent or meaningful seasonal variation?"

The weighting parameter was allowed to vary in time, this variation was very noisy and for the large majority of sites did not show any consistent variation. We mention this in the manuscript now.

"Too much emphasis is put on the weighting being less than 0.5. It is in fact very close

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to 0.5 showing similar weighting."

We rephrased this part as follows :

The weights for the soil temperature are close to 0.5, but for a large fraction of the sites (83%) the weight is smaller than 0.5. This indicates that the optimized temperature is slightly stronger influenced by the variability in T_{air} for many sites.

"Section 3.3 is dense. . . . It needs a thorough editing."

We edited the section to make it less dense,easier to read and understand.

"Section 3.4 It is not surprising that the temperature sensitivity of respiration varies for Ta versus Ts; the temperatures have different diurnal and seasonal ranges. The critical point for modeling is that parameters are ïňĄt using the same driving variables as themodel."

We think this refers to section 3.5. We included the point that models need to use the same driving variables as the analysis that derived the parameter.

"Fig. 8 and 9. The two figures should use the same order of Lasslop Reichstein."

The order has been changed as suggested.

Interactive comment on Biogeosciences Discuss., 9, 9829, 2012.