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Discussions

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Interactive comment on "Uncertainty analysis of eddy covariance CO$_2$ flux measurements for different EC tower distances using an extended two-tower approach" *by* H. Post et al.

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Thanks for this comprehensive review and the constructive comments. Before our final point-to-point reply we would like to take up the 3 major comments posed by anonymous referee #2:

(1)"Relevance of the extended two-tower approach. The inherent problem of the presented research (formulated provocatively) boils down to the question: Who will need this ap- proach? The authors use the random uncertainty assessment based on raw data pro- cessing (implemented by Mauder into the TK3 software) as a reference to validate their results. Of course the statement is correct that access to raw data is

sometimes limited, and extra processing to retrieve random uncertainty from raw data requires additional work. However, if one has the choice of either setting up an additional eddy system nd running it for a few months, or alternatively work on the raw data to get (more reli- able?) random uncertainty estimates, the latter still seems to be the more convenient choice. So why bother with an extended two-tower approach? I see two pathways how to deal with this issue: Ideally, the authors can clearly point out where their own ap- proach goes beyond what alternative approaches provide, i.e. where is the extra piece of information that cannot be obtained e.g. by analyzing the EC raw data? I'm sure there are some assumptions and uncertainties associated with each of the alternative approaches that can be used to highlight the benefit of this new method. In case it is not possible to claim any advantages of the extended two-tower approach over the raw data analysis, the authors need to clearly point out under what circumstances their approach might be applied: As I see it, this is only when a) no raw data access (or processing) is possible, and b) there is a nearby site in the chosen EC database that can be used as a reference site within the two-tower approach (i.e. similar environ- mental conditions, acceptable horizontal separation distance). This, however, would emphasize that there is only a very small niche for the presented ap- proach ."

First of all, possibly it was a somewhat misleading choice of ours to call the raw-data based error estimate, for short, the "reference". Uncertainty estimates for measured values differ from the measured values themselves in that they cannot be easily validated against a known truth, but at best against a well-established consensus. For EC fluxes, even this is not the case: Both the raw data based approach and the two-tower approach are comparatively new, the majority of reported flux data still being without any uncertainty estimate. Since both approaches exploit completely different properties of the data to arrive at what should ideally be the same concept of uncertainty, comparing them to each other is an interesting way to either gain confidence in both (due to agreement), or detect a deficiency in at least one of both (due to disagreement). To our knowledge, this has not been done before (although it should be

noted that Billesbach et al. 2011 included the single tower approach in their comparison). Second, we think that the practical obstacles for performing a raw-data based uncertainty estimation encountered by a (possibly non-micrometeorologist) researcher planning a synthesis study on past data of several sites obtained from FLUXNET are indeed tremendous. The two-tower approach, on the other hand, is not necessary limited to deliberately running a second tower for uncertainty estimation. With our suggested extensions to reduce the effect of systematic differences between the footprint ecosystems on the error estimate, it can also be applied to pairs of nearby towers from local clusters. Such clusters play an increasing role in the monitoring strategies of ICOS and NEON, and have already been employed in case studies (e.g. Ammann et al. 2007). It is understood that for many such clusters and case studies, more research would be needed to test the applicability to a wider range of differing ecosystems and tower distances, but the presented manuscript appears a good start to us.

Ammann C, Flechard CR, Leifeld J, Neftel A, Fuhrer J: The carbon budget of newly established temperate grassland depends on management intensity. Agriculture, Ecosystems & Environment 121:5-20.

(2) "Footprint filtering: The footprint filtering concept as presented in Section 3.6 is severely flawed! Simply comparing the fractional composition of land use types in the footprints of two towers doesn't give you ANY information on whether or not these footprints overlap. It can be total coincidence that these fractions are nearly identical, while the respective towers 'see' completely different areas (and are therefore statistically independent in the context of your study). And as you describe correctly in a different section, even a homogeneous patch of land can host totally different environmental conditions at the microscale that may affect the flux rates - the same is true for a footprint area that is composed of two land use types, so you cannot claim that the towers 'see' the same simply because they have a similar land use composition in their footprints. You need to analyze what the actual overlap of the footprint positions is, the land cover within doesn't matter!"

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We now started to re-run the footprint model, such that we will be able to quantify the overlap based on the spatial distribution of source weights of the two stations during each half hour. This additional study will be presented in the response letter to the reviewers after the revision of the manuscript and, if needed, also included in the revised version of the manuscript. It should also be noted that it was not our intention to suggest that convergence of the land use type contributions is a measure of footprint overlap in general. Rather, it was intended as a proxy enabled by the specific spatial distribution of mapped land use types and EC stations in our case. We did not invest more time to an elaborate footprint estimation because the respective analysis was not part of the main thread of the manuscript, but rather intended to shed light on the causes of the dependence of uncertainty estimates on tower distance (cf. p. 11958 L 19).

(3)"Sensitivity study on approach configuration The choice of the 12hr moving window to filter out the systematic errors, as well as the 50% data coverage threshold for valid moving window averages, need to be supplemented by sensitivity studies. Both of these settings seem rather subjective, so the authors need to demonstrate how results might change with different settings, and why the chosen ones are the best option."

As pointed out in Sect. 4.3, we tested stricter criteria (70%, 90%) for the amount of measurement data available in a certain window. However, finally we presented results for a less strict criterion in the paper. Applying stricter criteria resulted in too little data to calculate reliable statistics. We admit that it would have been better to be able to apply a stricter criterion. In the revised version of the manuscript this point will be clarified. Although not specified in the manuscript, also tests with different moving window sizes were made. As pointed out in Sect. 3.5 we found a moving window of about 12 hours to be the most appropriate because a) it considers diurnal changes, b) it is long enough to reduce the impact of random fluctuations. Larger window sizes of 24 and 48 hours were hampered by many data gaps so that often the 50% criterion was not fulfilled. A much shorter window was also problematic because in case of

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50% data availability too few data are left and the estimate of the mean NEE over that period is too uncertain. We also tested for slightly larger or smaller window sizes and can present the results in the revised version of the manuscript.

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