

Interactive comment on “Uncertainty analysis of eddy covariance CO₂ flux measurements for different EC tower distances using an extended two-tower approach” by H. Post et al.

Anonymous Referee #2

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Review on the manuscript by H.Post et al. titled ‘Uncertainty analysis of eddy covariance CO₂ flux measurements for different EC tower distances using an extended two-tower approach’, submitted for publication in Biogeosciences in May 2014.

The authors present an extended version of the 2-tower-approach originally introduced by Richardson et al. in 2006 to obtain a data-based estimate on random errors associated with eddy-covariance flux measurements. This study particularly addresses the ambivalent requirements for this approach to have uniform environmental conditions at both towers (so ideally they are located very close together) while at the same time their flux data time series need to be statistically independent (thus their footprints are not supposed to overlap). The new addition to this approach by Post et al. is to apply a

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simple filter that removes systematic flux differences from flux time series, which allows to compare also tower positions which are separated by longer distances. In this specific study, they compare 5 different separation distances (8m to 34km) and evaluate their suitability for random error assessment.

The study is well written and structured, though a bit lengthy in parts (in my detailed comments below, I have indicated several sections where significant cuts can be made to shorten the text). The scientific execution is good, i.e. the superior performance of the extended two-tower approach is demonstrated well in both text and figures, and also the effect of tower separation has been worked out well. However, there's a significant flaw in the application of footprint methods, and some settings used in the approach need to be validated by sensitivity studies (see below for details). The main drawback of this work, however, is its area of applicability – the authors need to make a clear case why the extended tower approach is needed overall, and in what cases it might be applied (see major comments below).

Summarizing, the authors need to make a clearer case on the relevance of their new approach. Also, a few methodological issues need to be straightened out, and the text needs to be shortened overall. Since this will probably modify the existing manuscript draft considerably, my final evaluation is to accept this manuscript for publication after major revisions.

MAJOR COMMENTS

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 approach? The authors use the random uncertainty assessment based on raw data processing (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 systems

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and running it for a few months, or alternatively work on the raw data to get (more reliable?) 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 approach 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 environmental conditions, acceptable horizontal separation distance). This, however, would emphasize that there is only a very small niche for the presented approach ..

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!

Sensitivity study on approach configuration The choice of the 12hr moving window to

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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.

DETAILED COMMENTS p. 11944: Abstract is of appropriate length, and informative

p.11944ff: Introduction overall well structured, but much too long. The current version may be OK for a thesis, but for a manuscript many of the 'excursions' are much too detailed. I added a few specific comments where paragraphs need to be shortened

p. 11945, 1st paragraph: no need to explain that many details of DA

p.11945, ll.8-11: there are many more reasons why a reliable uncertainty estimate of EC data is needed ..

p.11945f: The description of systematic errors is much too long! It is fully sufficient to mention a few sources of systematic errors (e.g. not well developed turbulence, energy balance closure, etc), then add the citations. No need to explain the details in an introduction when none of these effects are investigated further within the presented study

p.11946, ll.6ff: this entire paragraph can be deleted. Again, the manuscript doesn't treat EBD, so it's just another source of systematic errors.

p.11946, ll.20-22: I don't really like to see the effect of changing footprint areas being called an error. The changing field of view of an eddy system causes variability in the data, but the resulting effect is not an error. The error would be to assume that the footprint area is stable, as is correctly being stated here.

p.11947, ll.15ff: again, this is too much detail. It will be sufficient to cite that alternative approaches have been developed, which all have their own individual drawbacks.

p.11949f: The section on the sites is pretty much comprehensive. The only info that is

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missing is the data acquisition, i.e. what data acquisition devices (and frequency) were used?

p.11950f: Section 3.1 can be deleted. Anyone who wants to learn what eddy covariance is can check out a textbook. The only relevant information in this paragraph is your chosen sign convention for uptake and release, resp. (last sentences).

p.11952, ll.1ff: some of the given info as well as the use of citations are inconsistent. Correction of spectral losses in the TK3 package is based on Moore (1986); footprint analyses are not part of the quality flagging procedure; quality flagging is mainly based on tests for stationarity and integral turbulence characteristics, as well as the horizontal orientation of the anemometer; the citation for the quality flagging is Foken et al. (2004); the used flag ranges (e.g. 1-3 for high quality data) should be given.

p.11952ff: it's a bit confusing to read about weather filter and sfd-approach in this section (3.3) before these techniques are actually introduced. Would be nice if this could be improved. Otherwise OK.

p.11952, l.20: you mention earlier this distance is ~34km ..

p. 11956, ll.6ff: please add more details how this 12hr window was chosen. It also needs to be shown how the selection of this window influences the test performance! Separating between short-term variability and long-term trends is crucial for this approach!

p.11957, ll.1ff: the 50% threshold is indeed quite low. Has a sensitivity study been performed how stricter thresholds influence the performance? If not, this should be added.

p.11957f: Section 3.5 can be reduced to the statement 'filter for similar weather conditions followed Richardson et al. (2006)!

p.11960: Presentation of the results in this section reads OK

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p.11962, ll.14ff: there's no need to write down all the numbers per case study, since these are given in the table. Reduce this section to the range of values, and point out some outstanding examples!

p.11963, ll.1ff: 'nearly identical' is a bit exaggerated here. There's still considerable scatter in this comparison, and particularly for the far distances not all systematic bias effects seem to have been removed

p.11964f, ll.17ff: this whole section does not really belong into the discussion part, since it repeats the statements from introduction and methods that many factors can be responsible for small-scale ecosystem heterogeneity, and thus affect surface atmosphere exchange fluxes. All this should have been covered through your sdf correction.

p.11965, ll.22ff: this finding basically indicates that your current setup for the sdf correction does not fully cover all systematic differences in the flux measurements. Maybe you'll need a different configuration (e.g. averaging time) for larger distances? Or you are missing some influence factors when ecosystems show significant differences in their structure/properties?

p.11965, l.29: why counterintuitive? Do you really assume that the absolute distance is more important than the differences in ecosystem structure

p.11967ff: The conclusions sections reads OK overall, but some of the statements need to be toned down a bit

p.11968, ll.9-11: I don't think that your results database warrants the statement 'typically overestimated'. You would need more case studies to validate this. The only thing you found out so far is that in your tests, comparisons across distances of 20-30km resulted in an overestimation of random errors.

Interactive comment on Biogeosciences Discuss., 11, 11943, 2014.

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