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Biogeosciences Discussions

Interactive comment on "Spatial resolution and regionalization of airborne flux measurements using environmental response functions" *by* S. Metzger et al.

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Metzger review

This manuscript describes an innovative method to acquire high spatial resolution airborne eddy covariance energy fluxes in heterogenous landscapes using a variety of techniques. For the most part, it is a methods paper, but a very important one that will be highly cited in the future. The manuscript introduces several concepts for analysis of airborne flux data including application of wavelet cross-scalogram/flux unmixing to acquire high-resolution fluxes, application of a footprint model to these data to ascertain land surface contributions, and a boosted regression tree approach to determine

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Interactive Discussion

Discussion Paper

catchment scale environmental response functions. A thorough uncertainty analysis for systematic and random errors is conducted. Overall, the results show some expected patterns for H and LE, but demonstrate that the techniques can produce reliable spatially explicit fluxes.

The manuscript itself is lengthy as it covers a lot of ground. I have some minor comments that might help improve the flow.

1) It might benefit from a flowchart with a graphical description of the steps, showing the various data streams going into each step and their sources.

2) At some level, I'm still perplexed why the wavelet cross-scalogram works, but I see now that it comes at expense of high random error depending on how one defines the segment length and the importance of low-frequency contributions. Certainly, low altitude flights in the surface layer are essential for this method, as shown in the analysis of blending height and length scales. The variability in H and LE are very large across land cover types. It would have been nice to have some even literature based estimate of excepted values for variation in H and LE for typical land cover types observed in this region.

3) Also how about a top-down constraint? Couldn't net radiation be in principle remotely sensed? Does the estimate H+LE follow patterns in Rnet (yes I know variations in G matter)?

4) The paper claims that ERF from unmixing is more reliable than a model data assimilation approach, and that is possible. But there are some perplexing results. For example, it appears that solar incoming is only weakly related to H, but over the diurnal course, this is not the case. So is this because all the flights were mid-day only? The whole BRT and ERF approach, being somewhat new, may require a bit more discussion on how it works. In contrast, for example, the wavelet discussion is very well presented.

Minor:

1) The author mentions the importance of a constant AGL altitude, though given density fluctuations, wouldn't a constant pressure level be a better approach (would make the correction from T to theta less important)? Or are the WPL dry-air density corrections also being applied?

2) I'm curious why multiple heights were not flown in some cases to assess vertical flux gradients. On the other hand, I have little doubt that 50 m altitude flights should have little to no divergence.

3) Page 1592 "We hypothesize that airborne EC flux is a promising tool..." This is not a hypothesis. It's a claim. There are better hypotheses presented later that could be brought up here.

4) The authors a priori mask out slopes to avoid terrain generated mesoscale eddies. I'm curious on the justification and whether the data themselves indicate mesoscale eddy contribution near slopes?

5) The KL04 cross wind footprint functions are not published yet and thus fifficult to evaluate these functions. Should Include in supplement?

6) Page 15955 "The final BRT model..." re these five variables what the BRT model selects of all the variables or did the authors force the model to fit to these five only. This is not clear to me.

7) I applaud the authors for taking a serious stab at uncertainty along with the length scale analysis.

8) The use of continuous variables (LST/EVI) instead of land cover class is reasonable, but it assumes that all vegetation energy fluxes here can be uniquely described by these along with flight leg subset average meteorological variables. I suppose this might work here. how about somewhere where the variation might be corn crop to forest? Would it still work? The authors are very enamored of the method, but it

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is important to discuss where these methods might not work or what the maximum possible reliable altitude is possible.

9) Bowen ratio is useful to look at but it has known problems when Le is small or one term is negative. Would the results change appreciably if focused on evaporative fraction (Le/(H+LE))?

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