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Interactive comment on "Improving terrestrial CO_2 flux diagnosis using spatial structure in land surface model residuals" *by* T. W. Hilton et al.

Anonymous Referee #3

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In this paper Hilton et al focus an important topic in investigating the spatial coherence of terrestrial ecosystem fluxes of CO2 at different temporal scales. The current work relies on a semivariogram analysis to quantify the spatial structure of observed net ecosystem exchange (NEE) by the eddy-covariance method, as well as of optimized model simulations and model error throughout North America. The main findings in this analysis lead to challenge the plant functional type (PFT) paradigm for parameter distinction and to suggest a spatial structure in data-model residuals on average around 400km, but always below 900km, which is lower than usually assumed. Finally, this analysis supports the conclusion that the current network of 65 sites is sufficient to characterize the covariance matrix of land surface models.

It is my opinion that some general comments should be addressed in order to clarify and place an adequate perspective in the current results of the analysis:

VPRM is a very simple model, which may cast doubt on the generalization of these results to other land surface models with higher levels of complexity, especially regarding the challenge on the parametric prescription by PFT. Although other studies have shown intra-PFT variations in parameters [e.g. Groenendijk et al., 2011] and some associated it to local environmental conditions [e.g. Carvalhais et al., 2010]; see for example how Kuppel et al. [2012] showed that despite parametric differences between site-level optimizations for the same PFT a common parameter vector would be attained to explain most of the observational variability.

The conclusion that the "North American flux tower observation network is adequate for determining a land surface model residual covariance matrix" implicitly embeds assumptions on the representativeness of the network to the main factors controlling NEE fluxes and model errors. Given the significant dependence of site history in adequately simulating ecosystem fluxes [e.g. Kuppel et al., 2012] and the network representativeness being dependent of multiple factors [e.g. Sulkava et al., 2011], isn't this a strong assumption worthwhile discussing?

A strong relevance to the characterization of spatial correlations in NEE fluxes is given in the introduction. Given that the range found in this study for North America is much smaller than the considered in previous studies could something be said about implications in terms of regional estimates and associated uncertainties?

Some more detailed comments concern:

The large range in the length scale between 100km and 900km: these results could be shown before the conclusions and addressed in the discussion, especially the reasons behind the wide range.

Given the dependence of model and region to the current results, shouldn't this be more explicitly addressed in the conclusions and also reflected it in the title?

Some changes in the abstract to make it more quantitative would be appreciated.

In table 3, 2003 is shown as a very coherent year: the exponential variogram model is always better than the pure nugget model. Could the authors postulate reasons behind such singularity when compared to other years?

In page 4, line 124: “splace”.

References

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