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Biogeosciences Discussions

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 #5

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1 General comments

The work of Hilton et al. analyses the spatial structure of the residuals of modelled net ecosystem exchange compared against Fluxnet observations for North America. This information is important in constructing covariance matrices to be used in atmospheric inversions of CO2-fluxes. The authors use the relative simple model VPRM and geostatistical methods to analyse this spatial structure. The three main conclusions of this work are: (1) Plant functional types (PFT) demonstrate little skill as classification of model vegetation. (2) The spatial correlation length scale of the residuals is in the order of 100 – 900 km (with a median of 400 km). (3) The North American Tower network is sufficient to create a VPRM residual covariance matrix. My impression is that all of

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these conclusions are not sufficiently developed and need at least a more profound discussion as explained in more detail in the specific comments section.

2 Specific Comments

The three points mentioned in the general comments are discussed in the following: (1) The main argument for this conclusion is derived with ranges of different model parameter values after the application of parameter optimizations that do not show a clear separation between plant functional types. Even if this questions the concept of plant functional types for VPRM, this has little implications for other more complex models. Also the authors themselves argue that more rigorous statistical analysis would be adequate to definitely ascertain this finding.

(2) The authors test the method with pseudo-data and find 74 out of 1000 cases to be able to detect the exponential covariance structure. Given this relative poor detection rate and the large spread of the length scales I wonder whether the reported results are robust and have any significance. The authors should discuss this in detail. And they also should discuss, why they think an exponential behaviour is adequate to describe the spatial dependence of the residuals. Figure 4 might suggest something more complicated. Finally, given table 3, the range of the correlation length should be something like 0 – 4130 km which is substantially different from 100 – 900 km reported in the conclusions.

(3) I don't see a proof of this statement and I suggest to add a detailed discussion on how the authors deduce from the reported results that the North American Tower network is sufficient to create a VPRM spatial covariance matrix. Specifically the authors should discuss the points already raised in (2) and how this might influence uncertainties in such a covariance matrix. The authors could also discuss limitations of the use of tower network to construct a covariance matrix (e.g. how the minimum separation

BGD

9, C3141–C3144, 2012

distance influences the lower bound of the correlation structure – especially at scales smaller than this distance).

The following points should also be considered by the authors:

The abstract states a correlation length scale of 1000 km which is not consistent with the 400 km of the conclusions. The authors should use consistent statements about this length scale throughout the manuscript.

Sections 2.3 and 4.1: Uncertainties of eddy-covariance flux measurements has also been studied by Lasslop et al. (2008). The authors might include the findings of Lasslop et al. (2008) in their overview on flux measurement error characteristics.

Equation 8. I suggest to explicitly explain what R and I are.

Figure 4: This figure is created with binned semivariograms with a 300 km bin. An increase is detected until roughly 800 km. Does this result change when using other binning and what is the relevance of the maximum of the semivariances at roughly 1500 km?

Figure 5: The authors describe the distribution as similar. But at scales relevant for the discussion (400 km), the distributions seem to be clearly different.

Section 4.2; last sentence : I think this statement should be reformulated in the view of aggregation errors (e.g.: Kaminski et al. 2001)

3 Technical comments

page 7079 line 4: typo in “splace”

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9, C3141–C3144, 2012

4 References

Kaminski, T., Rayner, P.J., Heimann, M., Enting, I.G., On aggregation errors in atmospheric transport inversions, J. Geophys. Res., 106(D5), 4703 – 4715, 2001.

Lasslop, G., Reichstein, M., Kattge, J., Papale, D., Influences of observation errors in eddy flux data on inverse model parameter estimation, Biogeosciences, 5, 1311 – 1324, 2008.

Interactive comment on Biogeosciences Discuss., 9, 7073, 2012.