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## ***Interactive comment on “Improving terrestrial CO<sub>2</sub> flux diagnosis using spatial structure in land surface model residuals” by T. W. Hilton et al.***

**T. W. Hilton et al.**

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Author responses to Anonymous Referee #5  
Referee comments in boldface, author responses in normal typeface.

**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 CO<sub>2</sub>-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 spa-**

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

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.

We agree with Reviewer 5 that it is possible that our findings will not hold for more complex models.

That said, we believe VPRM to be a useful model for evaluating the broad question of whether spatial structure exists in land surface model errors, and our conclusion that it does, at least for VPRM, to have important implications for flux diagnoses.

We do not view the similarity of our optimized VPRM parameters across PFTs as one of the central points we wish to make in this article, but rather as an interesting result that we found mildly surprising, and wished to include in reporting our results. We will revise the text to better reflect this.

(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 exponen-

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tial 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.

We believe that the large spread among estimated range values in table 3, along with the pseudo-data experiment's detection rate of 7 percent for a known exponential covariance structure, rather than suggesting that spatial coherence in model residuals does not exist, are also consistent with another situation: that coherence exists, and the North American eddy covariance tower network is minimally adequate to detect it some of the time. This idea is supported by two aspects of the pseudodata experiments reported here.

First, the very low “false positive” rate of the pseudodata experiments supports this interpretation. An exponential covariance structure was detected when no underlying covariance was present in only 2.5 percent of cases (25 of 1000). In light of this, it seems implausible that the real residuals were better fit by an exponential structure than a pure nugget structure in 26 percent of cases (table 3) if no underlying structure in fact existed.

Second, the spread among estimated exponential range values in the pseudodata experiments is quite similar to the (admittedly large) spread among range estimates from the real residuals (fig 5 left panel).

**(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 distance influences the lower bound**

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**of the correlation structure – especially at scales smaller than this distance).**

We believe this statement is supported by our detection of exponential covariance structure in observed VPRM residuals at a rate far larger than the rate at which we were able to detect a similar – and known – covariance structure from pseudodata (see above).

There exist in the set of flux towers used in this study a number of pairs of towers that within a few tens of km of one another. It is true that the set of towers cannot provide information about the error covariance structure at length scales shorter than the closest pair of towers in the set. This uncertainty shows up as the semivariogram nugget, and does not impact the conclusion that flux model errors covary in space at scales larger than a tower footprint.

**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.**

Indeed, the scale of 1000 km in the abstract is in error. This has been corrected to 400 km.

**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.**

We will note these findings in the discussion.

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

We will add this to the discussion of equation 8.

**Figure 4: This figure is created with binned semivariograms with a 300 km bin.**

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**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?**

The form of the semivariance curves and the approximate length scale are consistent across bin widths. We have added this point to the caption of figure 4.

**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.**

One of the points we wish to illustrate with figure 5 is that the the detection rate of covariance structure in the real VPRM residuals exceeds the detection rate among the pseudodata at the relevant length scales. We will revise the text referenced above to better convey this point.

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

**page 7079 line 4: typo in “splace”** Thank you – this error is corrected.

**BGD**

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