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Interactive comment

Interactive comment on "Evaluation and uncertainty analysis of regional scale CLM4.5 net carbon flux estimates" *by* Hanna Post et al.

Anonymous Referee #1

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In this manuscript, the authors apply the well-known CLM land surface model to simulate ecosystem carbon fluxes at the catchment-scale. The main novelty of this article is the uncertainty analysis that is undertaken, using state-of-the-art MCMC procedure to evaluate model parameter uncertainty, but also introducing uncertainties in environmental drivers and initial conditions. For each step, the uncertainty in the predictions is matched with a default simulations that uses "out-of-the-box" parameter values for the various PFTs that can be found over the Rur catchment.

This article is relevant to the readership of Biogeosciences as it aims to bridge a gap that exists between eddy-covariance based approaches, and large-scale modelling contributions. However, I am concerned by the shortness of the discussion, and the lack of references to other groups' work therein. It makes it hard to put this piece of work within the current context. As the authors correctly mention, regional-scale un-

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Discussion paper

certainty analyses of land surface models are rare, but the various points they raise in the results sections have already been addressed in various studies. For example, the sensitivity of net carbon fluxes to Q10 value, or the various environmental response functions that affect turnover rates, has been addressed in detail by Davidson and Janssens (2006), Todd-Brown et al. (2013, 2014), Exbrayat et al. (2014a,b). Furthermore, the transferrability of PFT parameters has been studied by Kuppel et al. (2014).

Hereafter are some more specific points:

p3 l25: see also Xia et al. (2012) / Exbrayat et al. (2014a);

p3 l27: please consider adding a reference to the Global Carbon Budget / TRENDY intercomparison project here

p5 l2: please indicate time series length here

p5 l10-12: although this has been reported in a previous paper, can you please develop a bit more on how parameters were optimised for these sites?

p6 l13: can you indicate the respective fraction of each PFT here instead of l25

p7 l10: how representative of mean climate were the three years used for spin-up?

p7 l11: what is an "exit" spin-up? does that simply mean that the spin-up was 1203 years repeated meteorological drivers for 2008-2010

p7 l24: what value of the convergence diagnostic did you use?

p8 l9: please add reference for Fontainebleau's site

p8 l26: compared

p10 l1: please add full name and references for the two sites

p11 l19: are 15 years enough for this perturbed spin-up?

p12 l8: I am not clear what "deterministic initial states" mean here p13 l1: why are results for ENSp outside the range from ENSpai? from the experiment description it feels that ENSpai's uncertainty should envelop that of ENSp (and ENSpa)

p13 l21: there is a problem with figure numbering.

p13 l23: daytime NEE is not GPP, please replace/revise in the rest of the paragraph

p 15 l17: the discussion is very short in regards of the amount of results that are described.

p16 l1: a new alternative to the PFT approach is to rely on more continuously distributed trait maps (eg Castanho et al., 2013, Reichstein et al., 2014)

Figures 3,4 and 5: please correct the legend

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