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

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Dear authors,

first - my apologies in the delay of the review process. After failing to receive sufficient reviews, I now provide a brief assessment myself.

I find this a very interesting and robust study, which unfortunately still needs a bit of fine tuning, in particular in the presentation. By reading the abstract I was under the impression that this study was "merely" an application of a data assimilation method result to larger scales, whereas in truth the study expands to uncertainty in met-forcing and initial conditions, which is typically ignored in large-scale applications. I think this achievement deserves a bit more presentation in abstract, results and also discussion.

In the introduction, the text could be streamlined to focus on the current study and avoid side-lines as the use of atmospheric inversions or the future predicted climate of the Rur valley. I've been missing an introduction section that addresses in some more details the motivation for not only using DA to obtain model parameters, but at the same time use of perturbed boundary conditions (meteorological and soil), and a discussion of this in Section 4. I think this is really a novel contribution, which deserves somewhat more place.

Methodologically, I am unsure whether the assimilation procedure included the spin-up period, or not? In the latter case, one should caution the interpretation of the effect of the assimilation on the cumulative NEE?

The discussion is simply too cursory and needs to better take account of existing literature and explaining the added insights gained from this study. This could be partially achieved by taking considerations from the conclusions and expanding these ideas in the light of the existing literature (or potential applications for regional modelling).

Minor comments:

The abstract is quite long, consider shortening.

P1 L19: give average error reduction in μmol CO2 / m2 / s ?

P1 L21: a) is this in agreement with the observations (the fact that the NEE goes positive); b) add "simulated" before regional carbon balance estimates

P1 L22: here and elsewhere it would be important to understand whether you have brought the model into equilibrium in terms of the carbon cycle with the parameter set used, or whether you relied on a different way to initialise carbon stocks

P1 L26: It would be helpful to add in brackets the difference in annual integrated NEE in μmol / m2 / year

P1 L29: If the uncertainty was indeed reduced, then this would need to be demon-

strated by comparing prior and posterior distributions of the regional NEE. I guess you rather would like to state that the accuracy of the projection has increased because the model better fits site-level observations?

P2 L11: define what "conventional interpolation methods" are

P2 L12ff: all correct, but I don't think this paragraph is necessary for the sake of this paper.

P3 L2: please define more precisely what "error" is here. There is no principle error in averaging the input data, and then obtaining model output from this. The question is whether the aggregation method is sufficiently representing the average regional flux, is of course relevant.

P3 L32: I disagree that the uncertainty of carbon fluxes has been overlooked so far. I agree that the uncertainty hasn't been sufficiently quantified, and/or reduced.

P4L1: well, if you counted conference papers, it actually has. I don't think the question of "who was first" is really relevant, and would focus more on the value and design of the regional set-up

P4 L6: define what a "validated parameter" is. I assume that this is a parameter derived from data-assimilation? It is unclear to me why new estimates of parameters have been obtained for one PFT. Is this because you extended your PFT set to a new PFT (from 2016), or because the DA method of 2016 did not yield good parameters. In the latter case, would it not have been appropriate to recalibrate the model for all sites?

P6 L24: add "average" before percentage PFT cover.

P7 L11: Please briefly explain, why it was necessary to add a second spin-up

P7 L22: The statement of robustness of the method either requires a proof or (more likely) a reference

P11 L 25: I make this note here, although this probably needs mentioning later in that

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by not manipulating the "stable" nitrogen pool you basically conserve the amount of N available from net N mineralisation across the ensemble. That's fine (in particular, because the system are fertilised), but probably implies that you underestimate the true uncertainty in the net N availability at the sites.

P15 L7 (and elsewhere): please refer to other parts of the manuscript as sections, not chapters.

P15L11: Please be more precise, this is not really a general finding, it is specific to Q10.

Please have the revised manuscript cross-checked by a native speaker, and remove unnecessary words (such as "however" in the middle of a sentence, e.g. P15 L8).

P15: This discussion section is a bluntly speaking a bit too skinny. More could be made for instance of the extremely relevant and very nicely demonstrated point that minor modifications in input meteorology lead to markable differences in modelled NEE. However, generally, a critical assessment of the novelty of the study approach and finding compared to the existing literature is missing.

Table 3: Please arrange the table such that it is immediately clear which CLM4.5 default simulation belongs to which site

Table 4: please briefly explain the abbreviations used to denote "grid cells", and how the "n" were calculated (or what they are).

Table 5: define "n"

Figure 2: define "evaluation period". It would also be preferable to somewhat make a clearer distinction between different model ensembles and the observations

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