

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-160

Anonymous Referee #2

Referee comment on "Scale variance in the carbon dynamics of fragmented, mixed-use landscapes estimated using model–data fusion" by David T. Milodowski et al.,
Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-160-RC2>, 2022

This paper demonstrates the importance of accounting for fine-scale structure in heterogeneous landscapes to ensure ecological fidelity in modeling carbon dynamics. The authors designed two different approaches with model-data fusion to constrain estimates of model parameters and their uncertainty, and compared the difference in simulated carbon dynamics by these approaches with varying spatial scales over a mixed land-use region of the UK. The paper is overall well-written, and the experiment is well-designed. However, I have several concerns regarding the methods and conclusions. Please see my comments below.

Major concerns:

The model-data fusion framework (CARDAMOM) assimilated remotely sensed LAI and aboveground biomass (line 158), but soil organic carbon (SOC) extracted from SoilGrid2 was "used to set a prior constraint on the initial SOC stock. Is SOC a constraint in CARDAMOM? To set the initial SOC stock, how did the initial SOC be set for the baseline and stratified cases and for each spatial scale? Did the authors separately set the initial SOC for each sub-pixel type and constrain the sum/average of sub-pixel types to the SOC value derived from SoilGrid2? Please provide more information about this.

Is Cwood in the Results section the aboveground biomass or total woody carbon that includes both aboveground and belowground? Line 190, "Cwood pool is also a reservoir for non-woody structural tissues, for example in areas covered by crop and pasture." Does DALEC also have a woody carbon pool for pasture? Did the authors also infer belowground woody carbon for crop and pasture based on the allometric relationship in Eq. 3?

In table 1, it seems that Cwood and LAI are underestimated (i.e., negative bias) in both baseline and stratified cases across spatial scales. What are the possible reasons for it? Maybe the parameters were not fully constrained?

Several arguments in the conclusion section sound a bit misleading to me, e.g., "failure to account for sub-pixel ecosystem heterogeneity within MDF inversions leads to bias in the flux estimates", "stratification improves flux estimates", and "ecological fidelity of the calibrated model parameters is enhanced". The differences in RMSE and bias between baseline and stratified cases are not very significant, and sometimes the biases (absolute values) are even greater in stratified cases (e.g., -584 gCm⁻² in baseline Cwood and -627 gCm⁻² in stratified Cwood at 0.05deg scale). More validations of model estimates and constrained parameters should be included to draw such conclusions. Regarding "stratification improves flux estimates", the authors might want to say that "stratification reduces flux uncertainties". If yes, however, lines 287-289 already demonstrate that the reduced uncertainty is a result of assuming independence between strata, and uncertainty with full correlation across strata is comparable to the baseline uncertainty. Please clarify these arguments.

I was confused about the reason for invariance in biogenic fluxes with respect to both resolution and method. Does this indicate the biogenic processes in DALEC are (almost) linear ecological processes? The linearity in GPP estimation could be possible, but I am not sure if Reco should have a similar pattern.

Minor:

Line 123, should be 30,000 km²?

Line 293, how does DELAC simulate fire effects? Why fire was negligible?