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Hydrology and Earth System Sciences
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Discussions



# *Interactive comment on* "Incorporating remote sensing ET into Community Land Model version 4.5" *by* Dagang Wang et al.

**B. Martens (Referee)**

brecht.martens@ugent.be

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GENERAL COMMENTS

The paper describes a very simple approach to attribute model biases in the simulated states and fluxes of the latest version of the Community Land Model (CLM4.5). This is an important and interesting research area, as biases in modelled soil moisture or discharge can for instance substantially affect the prediction and analysis of hydro-climatic extremes such as droughts and/or floods. The approach introduced in the paper is not really innovative as it was first published by Parr et al. in 2015; but it is tested here for a larger study area and a different land-surface model. In general, the method and the results in this paper are well-described, but – to my opinion – not really surprising and rather straightforward. Substantial parts of the results and discussions are dedicated



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Discussion paper

to the differences in bias between the GLEAM-derived datasets and the CLM-runs with and without the bias correction. These results are very straightforward and predictable, as the bias-correction factors were first calibrated against GLEAM. Furthermore, most of the validations/comparisons are performed at aggregated variables (both in space and time), which might mask some of the potential issues. Summarized, I think the topic of this study is interesting, but I have the feeling that the paper (especially the results section) needs some improvements before final publication. Below I list some more specific comments.

SPECIFIC COMMENTS

1. In Section 4.2.1 it is claimed several times that the performance of CLMET is substantially better as compared to the original CLM. To my opinion, these statements need to be revised as they are not necessarily correct; especially not when the reference data is the GLEAM dataset itself. As the bias-correction factors are calculated using the GLEAM data as a reference, it makes perfect sense that applying these correction factors in the model brings the model closer to GLEAM (unless the assumption of time-invariance would not be fulfilled). Therefore, the results discussed from P11-L243 to P14-L305 (i.e. comparison of the bias-corrected CLM evaporation to the GLEAM dataset) only show the robustness of the correction factors. They do not show an improvement of CLMET in reference to CLM. To me, the evaluation of the runoff coefficients and the comparison against alternative datasets of evaporation (FLUXNET-MTE, MODIS) is a step in the right direction, but only a small portion of the discussion is dedicated to these results. Therefore, I would suggest to improve the evaluation of the results to really show the impact of applying the method. I would strongly recommend to (1) validate the modelled evaporation against in situ measurements (for instance data from single eddy-covariance towers) and, (2) extend the evaluation of the model against the alternative datasets of evaporation.

2. It is not clear to me how the statistics in Tables 1 to 4 are exactly calculated. This should be better documented in the manuscript. For instance, the temporal statistics

in Table 2: are these calculated per pixel and subsequently averaged over the different study areas (CONUS, NW ...)? Or is the modelled evaporation first aggregated for the study area, and the statistics calculated on the aggregated values? In addition, next to the comparison against the FLUXNET-MTE product, I would also suggest to at least include a validation of the products against actual FLUXNET measurements. Although there are different issues with eddy-covariance measurements as well, a lot of data is freely available, and these measurements are probably closer to the truth than any of the datasets currently used in the study.

3. I have the feeling that some issues of the method (e.g. the assumption of time-invariant scaling factors or the use of monthly scaling factors) might be masked by the spatiotemporal scales at which the results are analysed. For instance, why are only time series of the climatological cycle for the entire study area shown in Figure 6? It could be interesting to show some time series from individual pixels as well. Also, an analysis at shorter time scales might show some interesting results. E.g. why do the authors not show a time series of daily evaporation? The same holds for Figure 12: why are these time series not shown at daily time steps and on a pixel basis?

4. P6-L116-117: Could the authors be more specific here about what is meant by spatial correlation? Observations from FLUXNET are essentially point measurements. How are spatial correlations defined here?

5. P5-L107: I think it should be mentioned here at what temporal resolution the model is applied. From the results in Table 2, I can guess the model is run at a daily resolution. If the latter is the case, I think it should also be justified why the scaling factors are calculated at the monthly time scale. Given that both the simulations and the GLEAM datasets are available at a daily resolution, the scaling factors could as well be calculated at the daily scale. Would this also work? Did the authors test the effect of applying daily scaling factors in the algorithm?

6. P11-L244-245: Please revise this sentence: GLEAM data is not missing in this

period, but is probably masked out in this study as the Northern regions of CONUS are typically covered with snow during these times of the year. GLEAM estimates of sublimation are available for these regions, but I guess they are not considered here (at P7-L140-141, it reads that only interception loss, transpiration and bare-soil evaporation are considered).

7. P12-L261-262: If the term “significant” is used, it implies that a statistical test was applied to check this hypothesis. If this is the case, the test should be mentioned here.

8. Please note that the GLEAM datasets are no “observations” of evaporation. They are estimates of terrestrial evaporation, resulting from applying a simple conceptual model to observation-based datasets of different meteorological variables. GLEAM is kept as simple as possible to minimize the impact of the algorithms and maximize the impact of the meteorological observations on the estimates of evaporation. I would suggest to revise this throughout the manuscript.

TECHNICAL CORRECTIONS

1. Please use hyphens in “compound adjectives” such as “land-surface models” or “widely-used tools”.

2. I would suggest explaining all abbreviations upon their first use. E.g. P3-L68-69: SAC and VIC.

3. P5-L108: Given that no further details are provided in the paper regarding the land-surface model used, I would suggest adding a reference here for the CLM model.

4. P5-L111: Please define “PFT”.

5. P6-L124: I guess this should be section 2.2 instead of 2.3.

6. P7-L161: The fact that the GLEAM database has three subsets is not relevant here if you only use one.

7. P28-Table1: Please correct “COUNS” in the caption. Please also check this at other

places in the manuscript: e.g. P14-L315. . .

8. P34-Figure3: Please explain in the caption which areas are masked. I guess these are regions covered with snow?

Brecht Martens

Ghent University

Laboratory of Hydrology and Water Management

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