

Interactive comment on “CLASSIC v1.0: the open-source community successor to the Canadian Land Surface Scheme (CLASS) and the Canadian Terrestrial Ecosystem Model (CTEM) – Part 2: Global Benchmarking” by Christian Seiler et al.

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RC1: This manuscript describes the results of a global benchmarking exercise in which the CLASSIC land surface model is compared with observation-based estimates of various quantities. Uncertainty in driving meteorological data and in the observations is assessed using an ensemble of runs and in the analysis procedure. I recommend that this manuscript be accepted for publication with minor corrections (it is bit of a toss up between no corrections or minor corrections, but I will go for minor as there is some small room for improvement). This is clearly a long manuscript! Despite that I have few comments to make. The nature of the material (model benchmarking) almost inevitably leads to text that is descriptive and repetitive as different aspects/variables/datasets are considered in turn. This level of detail is mainly appreciated by people using the model, or by other modelling groups who wish to understand how models compare, or by someone with a particular interest in one sub-area (say energy fluxes) - but in general it does not make for a paper that is particularly easy to read or to review. Thus I am quite happy to admit that I have not studied every line in every table, nor every figure. However, I am reassured by the quality of what I have seen and am fairly confident that the many details provided would also prove satisfactory (if I had the time and inclination to study them). This might sound like slightly veiled criticism but is intended as an endorsement of the manuscript!

I particularly value the treatment of uncertainty, with three meteorological datasets used to drive the model and more than one "observation-based" dataset used to assess most of the variables. This should really be standard practice but often lack of time encourages modellers to consider a reduced set of possibilities. The value of the approach taken here is shown by the headline result that for 10 of 19 variables the sign of the bias depends on the datasets used. Narrowing these uncertainties is a key chal-

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lenge for the land surface community, but beyond the scope of this paper. In general I think this is an excellent attempt to benchmark (or evaluate; some people distinguish these terms) a land surface model, with a commendable level of detail presented (without being completely overwhelming). I think some other modelling centres will look at this with some envy!

Reply: Thank you for your positive evaluation of our manuscript and the very encouraging remarks!

Specific points

- **RC1:** Is it possible to make more of the comparison with the results of Part 1 (Melton et al.)? At present I think this is limited to the brief observation in the Discussion that the "overall statistic" is similar. Given that a holy grail for this kind of work is to get a coherent picture of model performance across multiple variables/sites/scales/datasets, it might be good to pursue the comparison to the results of Melton et al. in slightly greater depth (though I appreciate that it might be difficult to draw insightful conclusions beyond the fact that the results are similar).

Reply: We now compare the overall score values between both studies in the Discussion section. Given that results are similar, we conclude that an evaluation of global model outputs against eddy covariance data is meaningful, despite the fact that the present study does not use site-specific model inputs. We emphasize that this is particularly important for interpreting the low scores for NEE, which cannot be explained by using non-site specific forcing. We have now added two paragraphs in the Introduction and Discussion section.

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In the Introduction section: "A first evaluation of CLASSIC has been presented by Melton et al. (2020), who ran the model for 31 FLUXNET sites using locally observed meteorological data. Model outputs were compared against observed net surface radiation (RNS), latent heat flux (HFLS), sensible heat flux (HFSS), GPP, ecosystem respiration (RECO), and NEE. Results showed that CLASSIC reproduced the RNS, HFLS, HFSS, GPP, and RECO observations reasonably well. Reproducing NEE fluxes proved to be more challenging. While for some sites modeled NEE fluxes were in good agreement with measurements (e.g. site FI-Hyy, Figure S22 and S23 in Melton et al. 2020), for most sites NEE differed considerably from observations. Assessing the annual mean NEE value for each station showed that modeled and observed annual mean values were not correlated across stations. The poor model performance for NEE may be related to the fact that the disturbance history of the site is not reflected in the model. A comparison between results presented in the present study and Melton et al. (2020) is provided in the Discussion section."

In the Discussion section: "The overall score ($S_{overall}$) values documented in this study are similar to the ones reported by Melton et al. (2020). The values are 0.66 (0.69) for GPP, 0.71 (0.66) for HFLS, 0.68 (0.66) for HFSS, 0.47 (0.44) for NEE, 0.64 (0.65) for RECO, and 0.8 (0.82) for RNS, where values in parenthesis correspond to results from Melton et al. (2020). The fact that both approaches yield similar results suggests that a comparison of global model output and eddy covariance data at the grid cell level provides meaningful results. This finding implies that the low scores for NEE documented here are not due to the fact that we used global rather than site-specific model inputs. However, it must be noted that interpreting the differences between both studies is challenging, given that our evaluation is based on 204 sites, while Melton et al. (2020) used 31 sites. Furthermore, our approach averages observations across sites that are located within the same model grid cell and cover the same time period."

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- **RC1:** Similarly, there is little or no mention of benchmarking studies using other models (e.g. CLM) and any common conclusions. Again, this might be difficult, but some brief mention would be warranted.

Reply: We now included the key findings by Lawrence et al. (2019) and Bonan et al. (2019) who used ILAMB to benchmark CLM using three different model versions and three different meteorological forcing data sets.

In the Introduction section: "Lawrence et al. (2019) applied ILAMB to quantify how model development affected model performance for the Community Land Model (CLM). While the authors found a general improvement from CLM version 4 to version 5, they also identified variables where model performance would improve or worsen, depending on the choice of reference data. A second source of uncertainty identified by the authors was related to the choice of meteorological forcing. Driving CLM with three different data sets (GSWP3, CRUNCEPv7, and WATCH/WFDEI) showed that model performance was generally better when forcing CLM with the Global Soil Wetness Project forcing data set (GSWP3), and that the uncertainty associated with the forcing is too large to be neglected (Bonan et al. 2019)."

In the Discussion section, we now write: "Our results show that model performance is very sensitive to the choice of meteorological forcing and reference data, confirming recent findings presented in Lawrence et al. (2019) and Bonan et al. (2019)."

Also, please note that we do discuss benchmarking results for all land surface models that participated in TRENDY in the Introduction and Discussion section.

- **RC1:** Are all simulations carried out at the same spatial resolution and/or at the resolution of each meteorological dataset? I suspect all the datasets are 0.5deg.

Reply: The section *Reference data* mentions that "All gridded reference data are

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spatially interpolated to the spatial resolution of a CLASSIC gridcell ($2.8125^\circ \times 2.8125^\circ$)."

- **RC1:** Figures 1 & 2 (and some others, such as Fig.7) - the colour scale could be better, in particular to distinguish the darker tones (e.g. >5 and <-40 in Fig.1a). This is particularly important for Figs. 1 & 2 because of the small size of the symbols - they pretty much all look the same dark colour to me. Can the symbols be made larger and/or the panels larger? Without that these plots are of very limited value.

Reply: We have now scaled the size of the symbols in proportion to the size of the bias, where larger biases lead to larger symbols. To avoid that the larger symbols hide smaller symbols, the order of plotting is such that smaller symbols are plotted on top of larger symbols. Furthermore, we changed the colors of the extremes, and replaced the "vik" color scheme with the "roma" color scheme (Crameri et al. 2020). While both color schemes are color-blind friendly, the latter seems better for distinguishing darker tones. Please find an example plot below.

- **RC1:** Line 214: "it's" should be "its"

Reply: Thank you, done.

- **RC1:** There is repeated but not ubiquitous use of italics when numbering lists (e.g. i, ii)- which looks a bit odd.

Reply: Thank you, done.

- **RC1:** Fig.A1 - Eyeballing this it looks like CRUNCEP is often rather anomalous compared to the other two datasets (e.g. SW, Tas, precip, huss, wind). Is that

because the other products are based on more similar data sources, and/or might this suggest that CRUNCEP is an anomalous (possibly inferior) dataset?

Reply: Yes, with the exception of surface pressure, CRUNCEP differs most, while CRUJRAv2 and GSWP3W5E5 are more similar. This finding was counter-intuitive, as CRUNCEP and CRUJRAv2 both rely on CRU. Understanding why these data sets differ and identifying which provides more accurate values is beyond the scope of this manuscript. We have now added the following sentence in the *Meteorological forcing* section:

"With the exception of surface pressure, the data from CRUJRAv2 and GSWP3W5E5 tend to be more similar compared to CRUNCEP."

In the *Conclusion* section we added that:

"Future research could explore the question why meteorological forcing data sets used in this study differ and how this uncertainty could be reduced."

References

Crameri, Fabio, Grace E. Shephard, and Philip J. Heron. 2020. "The Misuse of Colour in Science Communication." *Nature Communications* 11 (1): 5444.

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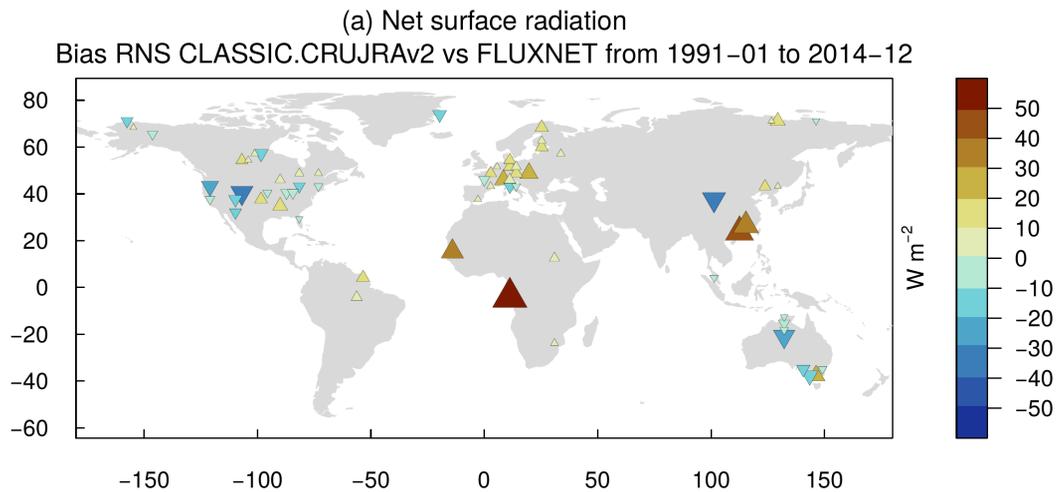


Fig. 1.

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