Rebuttal BG-2021-355

Local scale evaluation of the simulated interactions between energy, water and vegetation in land surface model

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Handling Associate Editor: Ivonne Trebs

May 19, 2022

Anonymous referee 1

This work presents a systematic point-scale evaluation of two prognostic land surface models (LSMs) and one observation-driven diagnostic model across several measurement sites of the FLUXNET monitoring network. The assessment of models' performance is focused on the simulated latent heat flux (LE), gross primary production (GPP), soil moisture, and leaf area index (LAI). Overall, this manuscript aims at disentangling the relative role of soil moisture and leaf area index in explaining the key models' weaknesses in the simulation of the land-atmosphere water-energy-carbon exchanges.
This work addresses a subject of interest for the broad audience of BG and it has the potential to shed additional light and provide guidance to the LSM modelling community on the simulation of water-energy-carbon interactions and involved feedback mechanisms. Having said that, I think there are substantial improvements to be implemented in the manuscript before making any consideration for publication.
I provide a more detailed list of comments below:

Comment 2.1 — The **introduction** of the manuscript is **too weak**, lacking the clear definition of the unresolved research questions that are behind this work. Those provided between lines 70-74 are, in my opinion, not scientifically relevant to justify the publication of this manuscript. In addition, **a mere models vs observations comparison is not per se a strong objective**; see lines 79-84. On the other hand, if the main objective of the manuscript is included in the last sentence of the introduction ("Given the degree of coupling in the current LSM, we try to disentangle the relation between key facets of the terrestrial vegetation in a holistic way"), authors should put **more emphasis on this aspect and less on the evaluation** of the models' performance.

> As the reviewer points out, the manuscript balances between 2 aspects: model intercomparison and an analysis of the internal interactions. Rather than strictly reporting the model performances, we attempt to extend the analysis with some statistics to provide more insight in the origin of differences in the model performances.

The soil moisture and LAI were identified as primary explanatory variables. With the given analysis, we find that some meaningful conclusions can be derived, though -as the reviewer points out- a more extensive analysis would allow more solid statements. We agree with this remark, it would be very relevant to test more model configurations, in which we impose soil moisture from various sources (observed or prognostically simulated) to the models. However, there are a few limitations associated with the current implementation of the models:

- LAI and soil moisture cannot be imposed in ORCHIDEE
- only LAI can be imposed in ISBA

To address the remark of the reviewer, given the model limitations, we are investigating the following:

- On a sub-selection of 2 sites, the simulated soil moisture and LAI are improved by calibration of soil/vegetation related parameters in ORCHIDEE. The indirect effect on the surface fluxes will be evaluated.
- Similarly, ISBA simulations for these sites are repeated with imposed LAI and improved soil moisture simulation (by calibrating soil hydraulic parameters).

Additionally, as the reviewer suggests in point 2, the diagnostic model can be used as a vehicle to evaluate the prognostic variables from ISBA and ORCHIDEE. This interesting approach will be tested for all sites. However, to derive meaningful conclusions from this experiment, it needs to be verified that the diagnostic model reproduces similar surface fluxes as the prognostic models, using the prognostic soil moisture and LAI from those models.
This work is still ongoing, but will be undoubtedly a valuable addition to this manuscript.

Comment 2.2 — I found the relative role “assigned” to the diagnostic model in the intercomparison exercise not fully clear and justified. Specifically, If the scope of the work is to compare the coherence with respect a LSM prognostic approach (see lines 73-74), authors should have structured their comparison in a different way. That is, they should have complemented the results of the **observation-driven (i.e., remote sensing for LAI and ERA5 for soil moisture) diagnostic model with those obtained assuming the output of the two LSMs** (i.e., soil moisture and LAI) as “observations”. In this way authors should have been able to provide more stringent interpretations on the different models’ performance and/or deficiency and eventually coherence.

As mentioned in the previous point, we agree that this would be a relevant experiment in this context. We will run the diagnostic model with prognostic soil moisture/LAI from ISBA/ORCHIDEE. The model configurations are listed in Table S-1. Some preliminary results are shown in Fig. S-1 and S-2.

Label	LAI FAPAR	Soil Moisture
LAIcop_SWera	CGLS	ERA5
LAIisb_SWera	ISBA	ERA5
LAIcop_SWisb	CGLS	ISBA
LAIisb_SWisb	ISBA	ISBA
LAIorc_SWera	ORCHIDEE	ERA5
LAIcop_SWorc	CGLS	ORCHIDEE
LAIorc_SWorc	ORCHIDEE	ORCHIDEE

Table S-1: Model configurations. CGLS: Copernicus Global Land Service (Camacho et al., 2013), ERA5: Hersbach et al. (2020)

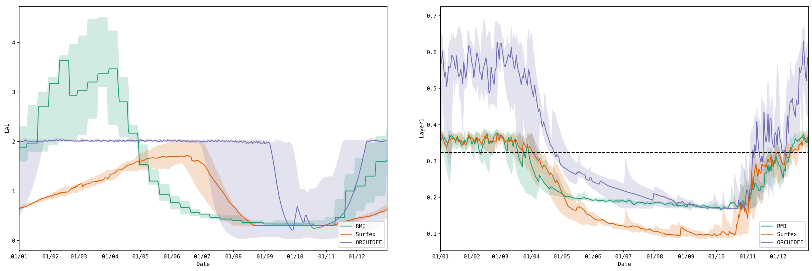

Figure S-1: Example of LAI and SWC time series (mean annual cycles, AU-DaP), used in the analysis with the diagnostic model. The dashed line in the soil moisture plot (layer 1) indicates the value of field capacity.

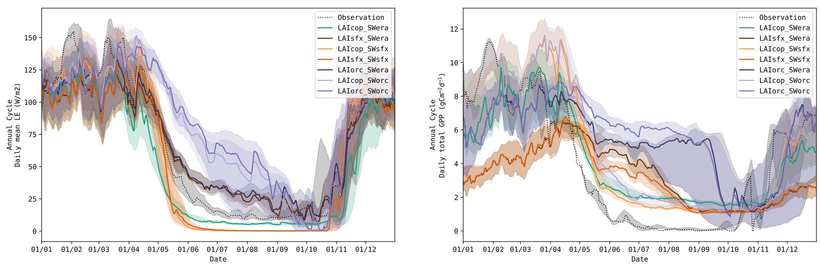

Figure S-2: Resulting LE and GPP mean annual cycles with different model configurations.

Comment 2.3 — In a similar vein to the previous point, If the objective of the work is to understand how changes in the state variables (i.e., soil moisture and LAI) propagates to the surface fluxes (i.e., LE and GPP) and vice versa (see Figure 1), I think authors should **add additional LSM configurations** in the matrix of the conducted numerical experiments. That is, on top of the current ("free") configuration, each prognostic LSM should be also run using a **prescribed LAI** (i.e., from satellite products), prescribed soil moisture (if the two LSMs have this functionality), and with both LAI and soil moisture prescribed. I think this comprehensive numerical framework could allow authors to get a "holistic" picture of the schematic shown in Figure 1 from three different LSMs.

> Indeed. As mentioned in Point 1, there are some limitations to the models, but a work-around is proposed, by calibrating the soil/vegetation parameters to improve the prognostic variables. However, due to the required time and energy for this test, we will limit ourselves to 2 sites, to demonstrate the impact of changing these variables. Performing this analysis for the full list of sites would require too much effort.

Comment 2.4 — I would recommend **adding a table** summarizing the key differences between the models that could help interpreting/explaining results shown in the manuscript. In the current form, it is difficult to get a clear picture on what are the structural and parametrization features that could explain the different response in the three considered models.

> Such a table will be prepared for the next revision of the manuscript.

Comment 2.5 — The **discussion section remains a bit too vague** in explaining the key reasons of the different model performances. I think the suggestions that I have provided in point 3 could help addressing this issue. As an example, the statement done between lines 438-440 could be validated using prescribed LAI values. The same apply to the sentence between lines 479-481. Overall, I think authors show make clear **what's the real objective** of their work. If the scope is to present a mere model **validation** exercise, the set of simulations presented in this study are sufficient, but they should try to **justify the novelty** of doing this in the introduction and better highlight the new insights gained by the large number of statistics. On the other way, if the purpose is to investigate how different LSMs resolve the **water-energy-carbon interactions**, I think the **current numerical setup provides not much information**.

> We do not fully agree with the reviewer. The current analysis of the sensitivities and error correlations allows to draw some relevant conclusions. However, we will follow the suggested approach to make a more in-depth analysis. The discussion will be adapted appropriately.

References

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