

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-355

Anonymous Referee #1

Referee comment on "Local-scale evaluation of the simulated interactions between energy, water and vegetation in ISBA, ORCHIDEE and a diagnostic model" by Jan De Pue et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-355-RC1>, 2022

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:

- 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.
- 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.

- 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.
- 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 parameterization features that could explain the different response in the three considered models.
- 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 should 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.