

Interactive comment on “Multi-variable, multi-configuration testing of ORCHIDEE land surface model water flux and storage estimates across semi-arid sites in the southwestern US” by Natasha MacBean et al.

Anonymous Referee #2

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I would like to thank the authors for their openness and the discussion, below I tried to reply to their questions in the informal response.

[Thank you very much for considering our informal response and getting back to us so quickly. These additional comments helped to clarify our edits to the manuscript and our responses to the formal reviews.](#)

I can see the difficulties the authors raise with regard to comparing the 2LAY and 11LAY soil moisture values, and also understand why the soil moisture values are not compared to observations for the 2LAY-model. For me, it is not a problem that you cannot use the 2LAY-values, but I just wonder what the point is of comparing 11LAY- results with soil moisture if you cannot do the same for the 2LAY-model. This also depends on the goal of the comparison, because you cannot use it to assess which of the models is better (which I believe is the main goal of the paper, and also how I interpreted this section). I believe it could serve as an explanation why the ET-values are better, but some textual changes may be needed to clarify this. In the current version, this comparison seems rather important, and relates to some conclusions, whereas it is merely an additional and supportive explanation for some other more important findings.

[We agree with the reviewer and in fact we did originally have a comparison to the 2LAY moisture but removed it for reasons we detail in the response to their formal \(original\) review. We have now proposed adding in a comparison to soil moisture observations to Figure 2, which compares the 2 vs 11LAY model. Please see our full response and updated figure in the response to the formal review.](#)

Regarding the second point of the authors, and I am sorry for not making it easier, but I strongly disagree with reviewer 1 that you should remove the 2-layer versus the 11-layer comparison. This is for me the key-point of the manuscript, and this relates also to my comment in my review that the authors sometimes show already a preference for the 11-layer model. It is not carved in stone that a more detailed model is better, and it should objectively be assessed which one is better. Even though reviewer 1 points out that more detailed Richards' equation approaches often improve LSMs, there is also an important reason bucket-type models are still often used especially in catchment hydrology. The Richards' equation approach does not include macro-pores, which in more sloped areas plays an important role. In addition, the parameterization often assumes a homogeneous soil, which is also not true. The fact that LSMs often perform better with Richards' approach also relates to how they are parameterized, bucket-type models need actual calibration as the parameters are less physically based, whereas the Richards' approach uses more physically

based soil parameters that are often measured. In general, the hydrological schematization in LSMs is in my view still rather poor, even with more detailed Richards' equation approaches, whereas it actually has a strong influence on the outcomes of the models, so I believe it is important that the authors show this. In addition, for a strong modelling experiment, you always need a benchmark, which is here the 2-layer model. Leaving it out leads to a manuscript that is just a model application, and the reader can never see what the 11-layers actually add.

We thank the reviewer for outlining further reasoning for keeping the 2 vs 11 layer comparison. To address both reviewers concerns/suggestions on this matter, we propose outlining our reasoning for this comparison more clearly by including the following statement in the introduction (after original lines 120-122):

“Although there have been many previous studies comparing simple bucket schemes versus mechanistic multi-layer hydrology based on the Richards equation, we include such a comparison in the first part of our analysis for the following reasons: a) the simple bucket schemes were the default hydrology in some CMIP5 model simulations and these simulations are still being widely used to understand ecosystem responses to changes in climate; b) variations on the simple bucket schemes are still implemented by design in various types of hydrological models (Bierkens et al., 2015); c) there has not yet been extensive comparisons of these two types of hydrology model for semi-arid regions, and especially not for the SW US; and d) so that the 2LAY can serve as a benchmark for the 11LAY scheme.”

Bierkens, M. F. P.: Global hydrology 2015: State, trends, and directions, *Water Resources Research*, 51(7), 4923–4947, doi:10.1002/2015wr017173, 2015.

We do completely agree that it is not necessarily the case that a more complex model is needed. We hope that by addressing the reviewer's original comments and suggestions (including adding soil moisture observations to figure 2) that we have made our case for why we think the 11LAY does a better job at capturing the temporal dynamics of the upper layer (root zone) soil moisture and evapotranspiration. We have tried explicitly not to go beyond that specific conclusion regarding any preference for the 11 layer. We also hope that it is clear from our analyses on remaining model discrepancies, as well as other topics that we have highlighted in the discussion, that we agree that there are still many issues (missing or inadequately represented processes) in the more mechanistic versions included in LSMs that still need to be addressed. For example we have mentioned, as the reviewer discussed above, the fact that soil texture and most hydraulic parameters are fixed both vertically in the soil column and that spatial heterogeneity is not well captured. We also mention the need for parameter calibration. We did not include an exhaustive list of all the LSM hydrology model issues simply because these comparisons are still point-based, whereas many of the issues that remain are related to modeling spatially distributed hydrological budgets, which is beyond the scope of our present study.

I hope my thoughts are useful, even though it is probably not making it easier. I still look forward to an improved manuscript and hope the authors find a good way to address all the issues of myself and reviewer 1.

We sincerely thank the reviewer for their second round of feedback. It was certainly both insightful and useful in making a decision on this issue.