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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.

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.

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 homogene 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 actually 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.

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 to authors find a good way to address all the issues of myself and reviewer 1.

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