Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-598-SC1, 2020

Hydrology and Earth System Sciences
Discussions

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.

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Received and published: 17 February 2020

We are grateful to anonymous reviewer 1 for providing us with such a thoughtful and useful review. Here, we briefly respond to some of the major issues raised by reviewer 1 in the hope of some further discussion before the interactive public discussion period ends on 23rd February. We will then provide a detailed response to reviewer 1 along with a revised manuscript.

We absolutely agree with the reviewer that many interesting aspects related to hydrol-

ogy of heterogeneous semi-arid ecosystems were not either a) detailed in the model description and/or b) not elaborated on in the discussion. We will address these points thoroughly in our revised manuscript and in our detailed response to this review that we will submit after the discussion period has closed.

Another point raised was why we compared the 2-layer vs 11-layer model given that has already been examined a lot in the literature. We agree with this point and in fact there was some debate about this amongst ourselves. It is true that most land surface models do now have a more mechanistic Richards' equation-type approach to modeling soil moisture dynamics. It's also the case that it is hard to compare the 2-layer and 11-layer approach given how different the representation of soil hydrology is and that it is very difficult to compare the 2-layer version to observations (much harder than the 11-layer). However, despite these considerations we decided to keep the 2-layer vs 11-layer comparison in an initial first part to the results because of the following reasons: firstly, we are expecting that not all readers are land surface modelers and some of those people might either not be familiar with simple bucket models or they might be users of hydrological or other types of models that still use a simple bucket scheme. For these readers we wanted to show once again that the bucket model really does not represent the temporal dynamics of the soil moisture or ET well; therefore, they should actively not trust ET predictions from any model that uses these types of soil hydrology schemes. Secondly, the ORCHIDEE model CMIP5/IPCC AR5 simulations were based on the 2-layer version of the hydrology model. While this was a long time ago now and the CMIP6 simulations are being released, many people are still using CMIP5 to study various aspects of earth system processes, climate change impacts or to understand model deficiencies. Given the fact that CMIP6 results are $\sim$1 year delayed, we expect that people will continue to use CMIP5 simulations for at least another year. Therefore, we explicitly wanted to mention that the ORCHIDEE CMIP5 ET predictions might not be as accurate as previously thought for semi-arid regions, with consequences for predictions of other variables. The reviewer specifically mentioned CO2 fluxes for example, and with good reason. In fact, this paper is part of a series of papers that are addressing multiple aspects of modeling semi-arid ecosystem functioning, including CO2 fluxes. We have chosen not to include any aspect related to CO2 fluxes in this paper because the model evaluation has pointed to more significant model deficiencies that we are currently trying to address – so this will be the subject of a forthcoming follow up paper. If the reviewer is interested, this work was presented at AGU last year: https://agu.confex.com/agu/fm19/meetingapp.cgi/Paper/489913 and https://nmacbean.files.wordpress.com/2020/02/nmacbean_agu2019_swusnee_poster.pdf.

With all this being said, we are inclined to keep the comparison between the 2 vs 11 layer, but in our revised manuscript and detailed response to the reviewer we will attempt to outline this reasoning more clearly, including pointing out other fields/models that are still using bucket layer schemes. We will also de-emphasize the first part (2 vs 11 layer comparison) and instead emphasize that the second part (11 layer comparison to observations and remaining model-data discrepancies) is the key part of the paper, particularly in terms of land surface modeling. This will include a more comprehensive discussion of aspects of the 11-layer model that don't address some of the semi-arid issues outlined in this review. In the meantime, we welcome reviewer 1's thoughts on our reasoning behind retaining the 2 vs 11 layer comparison as part of this paper.

One final point we'd like to raise at this point in the review processes related to reviewer 1's specific comment "If LAI was identified to be important why is no local LAI data used? I found local LAI data in Scott and Biederman (2017) for some of the sites." In fact, in Scott and Biederman (2017) it is MODIS LAI (satellite-derived) that is used. Unfortunately, to date we do not have any timeseries of local LAI measurements related to specific vegetation types. This would be extremely useful – as the reviewer points out. The MODIS LAI cover 250m or more and are therefore considered landscape scale estimates – therefore, we cannot use them to validated specific PFT LAI simulations from the model. Furthermore, while the timing of satellite LAI estimates generally agree, the absolute magnitude of different satellite-LAI products varies widely. This is due to differences in the retrieval algorithms used to infer LAI from the raw radiance

data (e.g. D'Odorico et al., 2014; Garrigues et al., 2008; Pickett-Heaps et al., 2014). We were hesitant to use these data for these reasons; however, we will re-think this decision when we revise the manuscript, specifically in terms of whether it might be useful to normalize the satellite (and model) LAI and only consider their temporal dynamics.

Once again, we thank reviewer 1 for their detailed review and we apologize for not leaving much time in the interactive discussion period to reply to this initial informal response to their review. Otherwise, we look forward to providing a detailed comment-by-comment response to their review after the interactive discussion period has closed.

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