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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.

Anonymous Referee #1

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MacBean and colleagues compare the land surface model ORCHIDEE against six semi-arid flux sites, using the old 2-layer soil hydrology scheme and the new 11-layer scheme of ORCHIDEE.

The study is certainly done correctly and the comparisons are fine. Specific remarks and questions are below.

However, one asks him/herself why one needs another validation of a Richards model in an LSM, showing that it performs better than on old bucket or 2-bucket version?

Specifically the multi-layer soil model of ORCHIDEE was tested quite a number of times already.

But semi-arid ecosystems are interesting because quite a few model assumptions of LSMs get challenged there. Unfortunately the paper does not talk about it nor tries to advance in this direction.

For example, ORCHIDEE uses tiles or fractions to deal with different land cover within one grid cell. To my knowledge, if a grid cell is vegetated then there is only transpiration (T). Evaporation (E) is from a special bare soil fraction only. There is no below-canopy E, which experiences lower wind speed, higher humidity and a litter layer compared to bare soil. This might have changed in the 11-layer version. Would be interesting to know. If the bare soil fraction mimics below-canopy E, then it is just a modelling concept and should be treated like this.

Semi-arid ecosystems are probably the only ecosystems where this model structure is valid for soil evaporation. However, the rest of the model structure with fractions comes to its limits. If there is a shrub-encroached grassland, the shrubs (trees in this study) get all crammed into a small tile, shading each other and competing for soil moisture. Or is there a gap fraction in ORCHIDEE? Does it allow for shrub (tree) roots to forage in the grass tile? The grass in semi-arid ecosystems dies off during the year. This changes the LAI as discussed in the paper. But does the grass fraction stay constant? Should LAI rather stay constant in the grass tile but the tile should shrink, leading to more bare soil fraction? I think that one cannot discuss semi-arid ecosystems without talking about vegetation (dynamics). The CO_2 fluxes could be interesting in this respect as well. They are omitted in the current paper.

The paper discusses quite a few shortcomings of ORCHIDEE, or even LSMs in general. But there is no assessment of the importance of each point. They all seem to be similar important. I would have loved to see either prioritisation for model development or at least a guidance to the reader how to evaluate model shortcomings. The model might already be fine from an atmospheric perspective, or it might lead to a wet bias in spring.

Specific remarks are:

- I would change the tile. "Multi-variable" and "flux and storage" is tautologic. "Multi-configuration" is a bit much for two configurations.
- You should only cite one paper in preparation for CMIP6 and not once Ducharne et al. (in prep.) and once Peylin et al. (in prep.).
- There are three personal communications, which are all from co-authors. Which co-author talked to which co-author?
- The description of "Richards and Darcy's equation" is strange. Darcy is part of Richards. The description is strange at two places (l.110 and l.211ff). I think that Richards equation is known sufficiently so it is only interesting which form is solved, the saturation-based or the head-based form.
- 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.
- Why are different T/ET algorithms used for different sites?
- T/ET is seen as a measurement in the manuscript. But it is not. Any validation is missing in the Scott and Biederman (2017) paper, because it is pretty impossible to validate it. So T/ET should be seen only as an estimate. There are quite some algorithms in the literature to calculate T/ET and it is hard to tell why one should be more correct than the other.

- l.255: what is the subscript j on c_j?
- l.255ff: R(z) is explained but not n_{root}. If n_{root} were explained then one does not have to (confusingly) start the sums from 2 because n_{root}=0 in ν=1 and i=1.
- l265ff: Why is the relative water content weighted with n_{root}? This formulation is an empirical observation and the beta term is never weighted by root length density (or similar) in the data papers (e.g. Keenan et al. (Biogeosci 2009)).
- l.268: Should W be in kg/m^3 instead of kg/m^2? Why is W used and not volumetric soil moisture theta?
- l270ff: Why is p% = 0.8? There is quite some literature that it should be around 0.4 (e.g. Granier et al. (AFM 2007)), at least for forests?
- l.276f: The references are missing. And only the Keenan et al. paper actually supports this claim. The Zhou et al. papers do something very different and act only on stomatal conductance.
- l.303f: I wondered if this claim means that you have a near perfect energy balance closure?
- l.315f: why are there no site-specific soil characteristics? They must have been done at some point in the past.
- Fig. 1: Where are the observations?
- Fig. 1: Harmonise scales of ET, Runoff and Drainage, as well as of Upper SM and Total SM so that one can compare the fluxes/stocks. For example, why is Total SM up to 1000? If kg/m^3, then Upper SM and Total SM could have the same scale. If kg/m^2, they should be scaled according to layer depth.

- Fig. 1: Why is there (almost) no drainage at forested sites with the 11-layer version? Is this realistic? There is only a very small mention for US-Fuf in the text.
- Fig. 2: I think the titles of the y-axes of row 3 and 4 are swapped.
- Fig. 4: please put the 2 cm, 20, cm and 50 cm plots on the same scales.
- Fig. 5b: Data stays low during much of the snowfall period. This can happen if the data is measured inside a forest whereas the model assumes open space. Much of SnowMIP's model intercomparison, at which ORCHIDEE probably participated, focussed on open sites. We might not know well the behaviour of our models at forest sites.

  It looks like that the data is even decreasing at the beginning of the snowfall period. This could point to soil freezing. Some soil moisture sensors measure only liquid water, so low values are measured during frozen soil conditions. So sites also do not include possible ice phases in their transformations from voltage to soil moisture.

  Both processes were not discussed.
- l.384f: This is a "false friend" to me. Evaporation is water vapour but the Richards equation (as used in ORCHIDEE) does not include vapour transport in the soil. So the model has to compensate for this omittance. This is one of the primary reasons why the Richards solvers need very thin layers at the top of the soil. These layers cannot be seen as physical layers because they have to compensate for all the model deficiencies on top of possible litter layers. It is thus doubtful that these first few layers should be compared satellite measurements.
- l.396: Isn't his a contradiction to Whitley et al. (2016). You state in the introduction that Whitley et al. (2016) found that T of the vegetation is mostly too low in

  the models. Is 2-layer ORCHIDEE different so that 11-layer ORCHIDEE can decrease T during the warm season?
- l.448ff: There also seems to be a problem with infiltration. At the model attenuates precipitation peaks too much at forest sites, while it is almost not attenuating at the grassland sites. Could you explain that please. There seems to be a difference in the model why water can flow quickly to deep layers in grassland but not in forests. Or is it the bare soil fraction?
- l.454: I was wondering why the model was not tested with more layers, say 100?

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