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Geoscientific Model Development Discussions

Interactive comment on "Evaluating the performance of the land surface model ORCHIDEE-CAN on water and energy flux estimation with a single- and a multi- layer energy budget scheme" *by* Yiying Chen et al.

Anonymous Referee #2

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Chen et al.: Evaluating the performance of the land surface model ORCHIDEE-CAN on water and energy flux estimation with a single- and a multi- layer energy budget scheme

Ryder et al. (2016) described a new canopy parameterization for the ORCHIDEE land surface model that allows for a multi-layer canopy. Here, Chen et al. apply that model at 8 forests sites of different species composition, height, and leaf area index. They compare the model to observations at the site, both of fluxes above the canopy and profiles of temperature and wind speed within the canopy, and also compare the model

to a standard big-leaf canopy model. These comparisons show that multi-layer canopy models are a viable path forward and can be used in land surface models. The paper makes recommendations about further research needs. These are all important points and will help advance the land surface modeling community.

1. My primary concern with the manuscript is that the model has 10 or 12 free parameters that the authors optimized by fitting the model results to the observations at each site. These parameters lack a physical basis and are in effect tuning knobs. The optimization procedure produced significant improvement compared with the non-optimized parameters. This fitting of the model to the data does not test the theory in the model. The model uses the second-order closure model of Massman and Weil (1999) to calculate the vertical diffusivity. The Massman and Weil model has not been widely used. How robust is the theory? The authors introduce a weighting factor that modifies the diffusivity based on friction velocity (not in the Massman and Weil model). What is the basis for this? The authors also calculate the canopy drag coefficient using a parameterization developed by Wohlfahrt and Cernusca (2002) for grassland. Should we expect this to work in forests? It is important to note that Massman and Weil used a different parameterization for the drag coefficient and did not have the weighting factor. The use of numerous free parameters to fit the model to the observations obscures whether these parameterizations are theoretically sound and applicable to forests. The authors acknowledge this with the statement that "a set of twelve parameters need to be prescribed and calibrated regarding the physical processes within the canopy" (page 16, line 11). One is left wondering how robust the parameterization of physical processes is given this many parameters used to tune the model.

2. The vertical diffusivity (ki) is described by equations (3) and (6), which are different. Which one is used to calculate ki? How does equation (6) relate to equation (3). How is the Lagrangian timescale (TLi) in equation (3) calculated? More generally, where does equation (6) come from? I do not see it in either the Ryder et al. (2016) paper that describes the model or the Haverd et al. (2012) paper that is given as a reference.

3. Line 13, page 6: Deff should be CDeff

4. Page 7: Explain how ksurf is used in the model.

5. Figures 3 and 4 are nice summaries of overall model performance, but it is unclear how the Taylor scores relate to the magnitude of biases. Sensible heat flux and latent heat flux have low Taylor scores at particular times of the year or times of the day. It would be helpful to have plots of model and observed fluxes for both the annual cycle and the diurnal cycle so that the reader can clearly see the magnitude of the flux biases.

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