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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 #1

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This paper deals with the improvement of an existing SVAT by application of a multi-layer vegetation approach. In general it is a good idea to test models with higher complexity for application in "reality". So the manuscript has a good approach for publication. But, apart from the discussion about the performance of the new approach in relation to the former approach, some points must be taken into account for the acceptance of the paper:

Equation 7 uses the threshold of 298.15 K. What is the physical basis for this threshold - or is is an empirical value?

Equation 11 describes the calculation of stomata resistance dependent on photosynthesis activity of the plant (Farquhar model). This leaf photosynthesis model does not consider interaction between stomata resistance and soil water availability (stomata regulation by trees in case of disturbed water supply from soil). The authors must check the literature to include this effect in eq. 11 (e.g., literature about stomata control and decoupling coefficient).

The authors should explain how they want to tackle the mismatch between rough resolution of driving data (reanalysis 0.5 degree) and high vertically resolved vegetation layer. Is it necessary in this case to leave the bigleaf concept? Apart from that, it is doubtful whether reanalysis data with a resolution of 0.5x0.5 degree give a realistic information for soil water pool.

The performance of the model strongly depends on model tuning. There are a couple of tuning parameters without plausible natural background. This fact makes a transferability of the results to other sites difficult. Could the authors discuss this problem?

The multi-layer approach shows an improvement especially in soil heat flux. Is it relevant for climate? Apart from that, for inter-annual cycle soil heat flux must be about zero (not fulfilled in Fig. 4)!

I recommend a major revision of the paper and I would review the paper again.

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