

Interactive comment on “Coupling the high complexity land surface model ACASA to the mesoscale model WRF” by L. Xu et al.

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Authors' Response to Anonymous Referee #1

We appreciate the comments by the referee #1. We respond to his/her comments below:

This manuscripts presents the coupling of a more complex land surface model to WRF and thoroughly evaluated the performance of the new coupled modeling framework against the observations and original WRF-NOAH framework. I appreciate the amount of efforts and the compelling motivation in the introduction and can see high chance of this manuscript eventually to be published. The demonstration of the scientific value of such a new modeling framework, however, de-

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serves more attention and extra efforts. Given the significantly increased model complexity, it is not exciting to see that "Overall, when compared to the simple single layer WRF-NOAH model, the WRF-ACASA model has greater model complexity without decreasing the quality of the output". What's more exciting is to see the model simulated carbon dioxide fluxes, and if feasible, some evaluation on that.

We agree with the reviewer that we need to better identify the novelty of the WRF-ACASA model. In the revised manuscript we will make it clearer that this paper is the first of several evaluation papers. This paper focuses on the fundamental representation of surface meteorology, which is a necessary evaluation of a land surface model.

We will also add a discussion in the conclusion section on the comparison between lower and higher complexity model. The high complexity ACASA model properly accounts for important biological and physical processes between the ecosystem and the atmosphere, and the model performs well when compare to an extensive set of observation. It is true that ACASA did not outperform the NOAH scheme at this point. However, without tuning the ACASA model to any region, the model performs comparable to that highly tuned and lower complexity NOAH model. This should be considered as a good sign of the ACASA scheme.

Finally, we will extend the discussion of model capability that makes it novel and “exciting”: simulation of carbon dioxide fluxes and water fluxes. While these are not evaluated in this paper, we are currently preparing a study on that exact topic. However, we feel that a more meteorologically focus evaluation study is necessary before looking into carbon fluxes (and water fluxes, which we are also considering).

Also, most of the model comparison essentially focuses on the local scale simulations. I am wondering whether extra spatial complexity of the atmosphere and land processes and their interactions can be revealed by the more physically based representation of the ecophysiological schemes, which is not extensively

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discussed in this manuscript.

The reviewer raises an interesting point about the complexity and spatial issues of the study. The sophisticated ecophysiological schemes of ACASA are not discussed in detail in this manuscript, because that work has already been fairly extensively published and is referenced in the manuscript. However, we understand the need to extend the discussion of this topic, which will be done in the revised manuscript.

Lastly, the figure quality can be improved. For example, the fonts in Fig. 5-13 are too small to read.

The quality of figures will be improved in the revised manuscript. We will pay particular attention to the visibility of figures including the fonts.

Figure 3 seems not necessary and can be easily combined with Fig. 2.

This is a great idea, and we will combine Figure 2 and 3 in the revised manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 7, 2829, 2014.