

Interactive comment on “Contrasting roles of interception and transpiration in the hydrological cycle – Part 1: Simple Terrestrial Evaporation to Atmosphere Model” by L. Wang-Erlundsson et al.

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We thank Anonymous Referee #5 for constructive comments. He/she raises a number of concerns, which we will address here. The referee's comments are in italics, and our responses are in upright font. Unless otherwise stated, sections and equations referred to are those of the manuscript.

This paper is very well written and presented. The results presented are interesting. However, they are presented as if it was an evaluation of the model (e.g

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Section 5.1., 5.3.2., 5.4.). It is difficult to understand what the new scientific findings are. I suggest to put the results into a broader context, to highlight the importance of the results and the new scientific insights. This might need more literature research and will probably result in a longer version of the paper. I think that for readers of Earth System Dynamics, this longer discussion of the results and a significant reduction of the model description (Section 2.-4.) would be more interesting. Some of Sections 2-4 can go into the Supplementary information.

I think the paper would also benefit if one experiment was selected and elaborated on extensively (e.g. on residence time scales), while another experiment is chosen for an independent, shorter paper (e.g. on the effect of land-use changes on evaporation and evaporation partitioning). This is just a suggestion, and I leave it to the authors to decide how to improve the scientific value of this paper beyond model evaluation.

We thank the reviewer for the compliments and the valuable suggestions. We agree that the manuscript would benefit from putting the results in a broader context rather than simply as evaluation. Also referee #3 emphasized the scientific question beyond model evaluation, and referee #4 suggested us to highlight “new aspects (phenology, irrigation, landuse change)” rather than portraying the manuscript as “a model development paper”.

However, comparison with other studies is still relevant because we are introducing a new model. Thus, in the revised version, the main content of the manuscript will become clearly two-fold: 1) model evaluation and 2) analyses of the characteristics of the partitioned evaporation fluxes on land. We will further condense the method descriptions (Sect 2-4), move the land-use parameter section to the appendices, and highlight the novel aspects (e.g., evaporation partitioning sequence, phenology, irrigation) of the model. The land-use change experiment described in Sect. 5.3.3. will be removed.

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The comparison between STEAM and other models for evaporation should be elaborated on in the introduction.

In our responses to referee #2 and to D.G. Miralles, we also suggested an expansion of the literature review. In the revision, we will elaborate more on relevant learnings from previous model inter-comparison studies and previous evaporation partitioning studies.

Further, several very interesting applications of STEAM are mentioned in the text, but I think it would be desirable to explain these benefits a little more detailed (e.g. how can it be used for atmospheric moisture tracking?).

This is an excellent suggestion by the referee. We will explain the benefits of STEAM more detailed in the revised manuscript. Several other referees also requested improved explanation of the rationale of STEAM. Explaining the benefits more in detail would also help communicate about the potential usefulness of STEAM in moisture recycling studies, and strengthen the connection between Part 1 and Part 2.

Interactive comment on Earth Syst. Dynam. Discuss., 5, 203, 2014.