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Discussions

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

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We thank Anonymous Referee #4 for constructive and valuable 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 is a well written paper. Yes, I also felt that a model is presented without highlighting the novelty towards further understanding biosphere-atmosphere interac-

tions. But I see several new aspects (phenology, irrigation, landuse change), which, the authors can highlight in the revised work rather than portraying it as a model development paper.

We thank the referee for the kind words and agree that the manuscript would benefit from highlighting certain aspects more while toning down the model description part.

In the revised manuscript, we will have a clearly formulated research goal beside model evaluation. The aim of the paper will become clearly two-fold: to present and evaluate STEAM, and to analyse the characteristics of partitioned evaporation fluxes on land. We are considering referee #5's suggestion to elaborate more on the time scales (which also referee #2 found to be most novel and interesting). The time aspect of evaporation fluxes is also useful for interpreting results of Part 2. We will use STEAM to characterise the evaporation fluxes globally by 1) providing information on the terrestrial residence time scales of evaporation fluxes, and 2) quantifying the role of precipitation for evaporation partitioning. We believe these two issues are relevant for understanding the importance of partitioning evaporation and also connect well with the subsequent analyses in Part 2. We will further condense the method descriptions (Sect 2-4), and highlight the novel aspects of the model.

I support the authors for using the Jarvis model approach, which is pragmatic for a spatially-explicit hydrological model at the global scale. There are several uncertainties in the Ball-Berry model too. The B-B also equally suffers from uncertainties that are related to photosynthesis and soil moisture simulations under future climates. The behavior of A-gs relationship in C3 and C4 plants also gets complicated as climate changes.
I think, the Jarvis model is pertinent here because STEAM is a global scale hydrologi-

cal model that does not have a serious C cycle component.

We agree and thank the referee for this comment.

Specific equations of runoff are not shown. Please show them.

We described the runoff calculation in text:

1. p.210, L22-24: "Runoff is the sum of excess water Q_{uz} (exceeding $S_{uz,max}$) from the unsaturated zone and Q_w from the water stock (exceeding $S_{w,max}$)".
2. p. 220, L 9-10: "runoff fields from STEAM have been derived from subtracting mean evaporation and mean snow storage changes from mean precipitation over the years 1999–2008."

We will reformulate it in equation format for increased readability.

I am interested to know how you parameterized the soil depth globally. This constitutes the soil storage size, which determines the depth of water table. The position of water table determines surface runoff-generation (ie runoff initiates when WTD reaches 0). So I would like to see how runoff is calculated. Is it Hortonian runoff? How is the river routing done?

We suppose the referee by soil depth refers to the storage capacity of the unsaturated zone $S_{uz,max}$. It is calculated as rooting depth multiplied by the volumetric soil moisture at field capacity (see Eq. 24). Lateral flows are disregarded (see p.210, L6-7). We consider the runoff simplifications acceptable as trade-off for model simplicity and will discuss the limitations of our model setup in light of previous

model-intercomparison studies (as suggested by referee #2).

The soil moisture scalar of the Jarvis equation should decline beyond saturation, especially in high-latitude biomes. In this way you can have stress due to water excess also.

The currently employed Jarvis water stress function (Eq. 19) follows the study of Matsumoto et al., (2008), who used it to examine field sites up to $62°$N latitude. In addition, implementation of stress due to water excess is impractical in our current model setup because soil moisture never exceeds saturation. We are grateful for the referee's suggestion and will have it in mind for future model improvement. However, we think the current treatment of moisture stress is acceptable for our current needs.

After providing the references, you may delete some equations that are already well documented in the literature.

Referee #5 also suggested a briefer Sect. 2-4. In the revision, we will condense the method sections (Sect 2-4). We will move Sect. 3.3.1. on land-use parameter to Appendices and remove "B3 Daylength" from the manuscript.

References

Matsumoto, K., Ohta, T., Nakai, T., Kuwada, T., Daikoku, K., Iida, S., Yabuki, H., Kononov, A. V., van der Molen, M. K., Kodama, Y., Maximov, T. C., Dolman, A. J. and Hattori, S.: Responses of surface conductance to forest environments in the Far East, Agric. For. Meteorol., 148(12), 1926–1940, doi:10.1016/j.agrformet.2008.09.009, 2008.

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