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Earth System Dynamics 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.

Anonymous Referee #2

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General comments:

A. The paper describes a new global hydrologic model (STEAM) that focuses on the representation of the partitioning of evapotranspiration into its components. To me the most novel contribution of the study is the analysis of the timescales associated with different ET components (Fig 4). This is the most interesting aspect; can this be locally validated somehow? That would be a tremendous addition to this work.

B. A shortcoming is that this paper needs to reflect clearly an awareness of what has been learned about model dependencies in partitioning of the water budget between ET and runoff, components of ET, etc., as learned from the many previous inter-

comparison studies, particularly PILPS. Koster and Milly (1997) is a great place to start. Furthermore, models that concentrate exclusively on one component of a linked and interdependent system, neglecting other components, will have a difficult time simulating any of the system well (cf. Koster and Mahanama 2012, and particularly Koster's recent Horton Lecture at the 2014 AMS annual meeting). The authors should defend their modeling philosophy in the context of the demonstrated pitfalls of such compartmentalization.

C. Throughout the paper the authors show a good awareness of issues and details that could affect the estimates by this model. However, in all the minutiae what becomes lost is the grand motivation for STEAM. There is one sentence (P207, L4) stating a goal and that is all. What are strengths and uniqueness of STEAM beyond what has been done before? Please describe why this is not just another tunable model that still suffers from having more parameters than data to calibrate it.

D. Along the same lines, if the model is supposed to be simpler yet better at producing "realistic partitioning between direct and delayed evaporation", then what is the purpose of all this comparing to other global products? Is this a model to be validated for some specific application, or are the authors setting out to do something that cannot be done with other models or data? This point is not stated in the paper. If the model is to be shown to be equally or more accurate than other (more complex) models, then validation against observations at field sites should be done. If the purpose of the comparison is to "run with the pack", to be within the range of all other models, then why do we need another model? Perhaps this becomes clear in Part 2, but all motivational aspects should be clearly stated in Part 1.

Specific/Technical Comments:

1. P205 L12-14: In fact, for most locations the large difference in net radiation between wet and dry seasons is the main factor - wet seasons are energy limited so total ET is restricted.

2. P206 L18: Those results have also been challenged by Sutanto et al. (2014) in HESS [doi: 10.5194/hessd-11-2583-2014] - this should be cited.

3. P208 L21: Why is such a low resolution used here (1.5°)? Please justify.

4. P208 L25: Split infinitive.

5. Sec 3: It appears there is no surface energy budget closure in STEAM. It is claimed that the diurnal ground heat flux is often near zero (P211 L18) and can be neglected, but by this argument most of the ET terms in most places could also be neglected. Without the ground heat storage / loss term, there are many locations (continental and strong thermally advective regimes, e.g. that have frequent onshore flow) and seasons (especially spring and fall in middle- and high-latitudes) where significant errors in available energy could be introduced. These may in fact average out on the annual time scale, but on monthly scales they could impact ET terms significantly, and potentially shift the simulated seasonal cycle of the long-memory terms.

6. Similarly, as there is no prognostic surface temperature, ERA-I mean, max and min temperatures are used. This precludes basic feedbacks in the system (e.g., from shortwave heating or evaporative cooling) that could drastically change the vapor pressure deficit used to estimate evaporation terms. Have you looked into the sensitivity of your approach to this simplification? Could you please discuss this?

7. Canopy interception: Driving with a low-resolution rainfall product that lacks point-scale variability in intensity can lead to over-interception (cf. Reichle et al. 2011) - great care must be taken to calibrate this properly.

8. P219 L3: What is the rationale behind directing irrigation water to the canopy interception?

9. P221 L4 & P229 L15: What is the point of showing 9 significant digits? Keep all quantities to 2-3 significant digits.

10. P221 L10: LandFlux-EVAL shows a range/envelope across many models and

products too. Where does STEAM fall in that range? If you are going to compare to other products, please be complete about it.

11. P222 L14: Even by your own estimates, irrigation makes less than a 2% increase in total ET. Compare to other irrigation studies, e.g., Guimberteau et al. (2012), Wei et al. (2013) - again there seem to be inconsistencies in scale analysis, or at least a lack of clear justification, for what is/isn't included in this model.

12. P228 L10: The result that total ET remains constant under most land use changes just reaffirms that the model is overconstrained by fixed meteorological inputs and lack of a surface energy budget. Without a full surface energy budget including prognostic temperatures, what other result could there be?

13. P228 L22: Regarding rooting depths, the formulation errors of land surface models was clearly exposed by Jackson et al. (1996) and Canadell et al. (1996).

14. P230 L29: This is true regardless of climate regime; runoff errors are inversely related to precipitation gauge density (Oki et al. 1999).

15. P231 L11: Use present tense here.

16. Appendix D: Refer to Materia et al. (2010) for a recent relevant study of this aspect.

17. Table 2: Rice is only flooded early in its life cycle, not for the whole growing season. This is probably not an accurate representation of this crop in the model.

18. Fig 1: Label the diagram with words in addition to variables - otherwise it is cumbersome to interpret.

19. Fig 5: This analysis should be done in more bands - not just the two hemispheres. At least add a separate band for the tropics. Or even better: plot as a Hovmöller diagram (zonal means - latitude vs month).

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