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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 #1

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General comments

This paper is a model description of the 'STEAM' evapotranspiration model with some evaluation of its performance compared to other land surface models and measured values. The paper shows that the model does an adequate job at hindcasting evapotranspiration ratios. The model appears to be rational, though strongly reliant on some unsubstantiated assertions which should be addressed. However, there is not a compelling argument given for why a new model is needed and what original scientific contribution it makes, compared to existing offline land-surface models. The conclusions are not well justified by the results and analysis, as the relationship between the results and the conclusions are not directly related. Specifically, the abstract refers

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to the originality of the model lying in the analysis of the pathways of water in the atmosphere and the links between land use and water resources. However, there is no further reference to either in the paper itself, meaning that it's difficult to identify what the original contribution of this paper is.

The analysis of the model focuses on the evapotranspiration over different land cover types, making it within the 'land use' remit of ESD. However, there are three questions I have about its current placement in ESD-D:

1) This paper is primarily a model description. I agree that model descriptions are important to publish and I applaud the authors for doing so. However, with such a large element of model description, I'm a little surprised that the authors didn't submit to a model description journal. E.g. Geoscientific Model Development.

2) If the intention for the paper is that it should be a model evaluation with some research results, then the hydrological focus of the model might make it a better fit for a journal that was more focused on atmospheric/hydrological processes. E.g. Hydrology and Earth System Sciences.

3) It is not clear what the rationale is for this paper being part of a pair. The other paper uses a different model and there isn't a justification given for why and how they are linked, further than a cursory mention that it is part 1 of 2 and that the second paper also looks at evapotranspiration.

Specific comments

1) Section 3.2 contains a critical assumption of the model with no justification beyond that it is 'logical'. I suspect that this assumption (of the order that water is evaporated from the different surfaces) determines a large part of the partitioning of water. This must be clarified and justified. Ideally, sensitivity experiments should be performed to show to what extent these assumptions affect the overall results.

2) There is a lot of awkward phrasing, with mixing up of tenses and pronouns and some

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typos. While I fully appreciate that writing in a second language is difficult, the paper needs to be fully proof read by a fluent/native speaker to make sure that points are being made clearly and as the authors intended. Similarly, some of the expression is rather casual and imprecise for a journal article. Some examples:

a. Section 4.3. Page 220. Line 17: 'considered to be on the high side'. Rephrasing the expression with referenced evidence within the same sentence would aid comprehension.

b. When reference is made to Supplementary material, (not appendices as is used here throughout the text) it is useful for the reader if the reference is as specific as possible: point readers to a particular section of SM, a table, a figure etc. Such directions are usually put in brackets. E.g. The blah is blah (see supplementary information figure x).

c. Page 222, line6. This comes across as a normative and vague statement, especially in the phrasing that "criticism shows" it to be wrong and "unlikely". Please specify why you disagree. It would also be considerate to note Jasechko's reply, as well as other recent critiques of isotope estimates. There are several other instances where the authors might want to consider whether it absolutely necessary to dismiss the work of others when presenting their own model.

3) There are numerous instances (mainly in the results section) of a figure being only very briefly referred to, without any clear attempt to explain why it is being shown or link it to the conclusions or the overall argument. This makes the paper feel very disjointed. Information is given, but without the guidance through the information from the authors, pointing out why it is interesting and important, that the reader would like. There are also many more tables, figures and appendices than are really needed or justified. To give one just one example, tables 1 and 3 could easily be combined.

4) Many of the sections would benefit from being revised and reordered to make them shorter and clearer. Some examples:

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a. The results and discussion is quite disjointed and not focused on the results of the model, with lengthy diversions criticizing data. This section doesn't give a clear indication of what the original results are. Many of the results are given with little analysis or discussion. The introduction is a rather long stream of information that is presented without a clear reason for why it is absolutely pertinent to the question in hand. This needs to be revised, clarified and shortened.

b. Section 2 and 3 would be more logical to a reader if they were the other way around. I.e. The model first, then the data used in this particular simulation (as presumably the model could be used with other datasets).

c. The beginning of section 3 (p.210) could be substantially reduced in length if it were presented in a table with only the salient points clarified.

d. Section 4 gives three analysis methods which were used. However, it is then very difficult to find what the results of these analyses were. It would probably be easier to read if the results of the analyses directly proceeded the description of the analysis technique, given that they are quite brief.

5) Some of the terminology used is not as clear as it could be. For instance:

a. The terminology used to describe the separate parts of evapotranspiration is not particularly helpful. Using biophysical/physical for transpiration and all evaporation is not (so far as I know) a common terminology. Moreover, for ESD, it is potentially confusing for readers from a land surface or earth system modeling background who are familiar with the terms biophysical/biogeophysical in reference to all the surface changes (albedo, evapotranspiration, surface roughness etc.). Neither is this terminology consistent with the 'part 2' paper, which simply uses evaporation and transpiration. I would recommend that the authors follow this example.

b. The words 'floor' and 'ground' seem to be used interchangeably. It would be easier for the reader if one was chosen, clearly defined, and then used consistently thereon.

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6) The color schemes used in the maps are not very accessible to those who are color blind, etc. Please consider using a color blind friendly color scheme (see for instance Light and Bartlein (2004), for further information on this subject).

Light and Bartlein (2004) The End of the Rainbow? Color Schemes for Improved Data Graphics. Eos, Vol. 85, No. 40. Available from: http://geography.uoregon.edu/datagraphics/EOS/Light-and-Bartlein_EOS2004.pdf

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