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Interactive Comment

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.

D. G. Miralles

diego.miralles@bristol.ac.uk

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Dear authors and editor,

I read with interest the manuscript by *Wang-Erlandsson et al.*, and I am left with several concerns, three of them are quite general. I would really appreciate it if the authors could take the time to address these three points:

1 – The missing background and justification

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A recursive claim that there is a *'scarcity of global data on evaporative partitioning'* is used to justify the development of the STEAM model (see e.g. pg. 206, L23). However, over the last decade, there have been numerous efforts to derive the separate components of the terrestrial evaporative flux at the global scale, not just from land surface models and climate reanalyses, but also by combining satellite data. Different operational datasets of global transpiration, interception loss, soil evaporation, open-water evaporation or snow sublimation are currently used within the global evaporation community; amongst others: the MODIS evaporation product[1,2], the PT-JPL product[3], or the GLEAM model[4,5]. Global inter-comparison of these models, and their partitioning of evaporation, is the subject of projects like the LandFlux initiative of GEWEX (2010–present)[6] or the European Space Agency (ESA) WACMOS-ET (2012–present)[7].

Of course, the long-term existence of this line of work does not preclude the surge of new models like STEAM, the more the merrier! But the authors should make a better effort to acknowledge this previous work, and hence find a better justification for the need of STEAM than simply 'there is nothing else'. The current justification is not only brief (as pointed by Referee #1), but it is also inaccurate: global models dedicated explicitly to partitioning evaporation already exist, they have been validated thoroughly and are widely used.

2 – Lack of validation of the evaporation partitioning

All the above-mentioned models estimate the partitioning of land evaporation differently from each other, with their own uncertainties and assumptions. However, in all cases there has been an attempt to quantify the skill of their estimates over different land covers using eddy-covariance measurements, which at least guaranties a first assessment of the quality of separate evaporation fluxes – like forest transpiration, grassland transpiration or snow sublimation (see e.g. GLEAM validation with 163 eddy-covariance stations and 701 soil moisture sensors from different land covers[7]). These models (i.e. GLEAM, PT-JPL, or MODIS) have also been compared to the water balance from GRDC and literature values (see e.g. refs. 4,8), but these exercises can only aim at providing a measure of the accuracy of total terrestrial evaporation over long time scales and large areas. At no point do these comparisons assess the accuracy, or the time-scales, of the separate components of evaporation. For this reason, it is surprising that in the case of STEAM — for which the main rationale seems to be the estimation of the partitioning of evaporation into different components and their time-scales (not simply the estimation of total long-term evaporation) — the validation has been limited to comparison to GRDC runoff data. At the very least, I suggest the authors to rephrase sentences like *'validation shows that STEAM produces realistic evaporative partitioning'*, where is it shown?

Nonetheless, a better solution would be to include some form of validation of the modeled land-use differences in evaporation — which could be assessed by using eddy-covariance measurements (like in all previous global evaporation models). Such a reality check is of particular importance considering the worryingly vague explanations on how land type parameters have been retrieved, e.g. *'The choice of land-use parameters is* (. . .) *based on the preservation of the internal consistency of STEAM, manual calibration and priority for literature values with higher relevance'* (pag. 215, L8-11). To what extent is the 59% contribution from transpiration a product of this *manual calibration and subjective priority*? If this issue is left unanswered, it will truly question the validity of some of the main conclusions.

3 – The uniqueness of the work

There is an obvious resemblance between STEAM and GLEAM[4,5], not just in the name! Given that some of the authors have had interactions with GLEAM in the past, I am surprised that they have not noticed this resemblance. Moreover, I

ESDD
5, C107–C112, 2014

am surprised that they have omitted any reference to this previous work. STEAM, like GLEAM: **(a)** is an evaporation model dedicated to the partitioning of terrestrial evaporation at the global scale, **(b)** is based on the offline forcing of a radiation-driven formula of potential evaporation, **(c)** constrains this potential evaporation based on estimates of evaporative stress that are computed with a multilayer running water balance dedicated to derive root-zone soil moisture, **(d)** uses a water balance model to estimate interception, **(e)** is run with ERA-Interim inputs (see GLEAM in refs. 7,9), **(f)** has been developed by a hydrology group from a Dutch university (just a few years later). There are certainly many other similarities between STEAM and the original GLEAM, but it is probably unnecessary to continue enumerating.

Nevertheless, since I foresee that the authors will focus on their differences, and not their similarities, when addressing this comment (like e.g. the fact that GLEAM does not consider a controversial process like litter interception explicitly, or that irrigation is accounted via assimilation of soil moisture observations in GLEAM), I note that those differences seem minor compared to the resemblance, and that by no means do they make STEAM a novel and unique methodology that requires no reference to antecedent work. Note also that the GLEAM products are already widely used by the community of readers that *Wang-Erlandsson et al.* is targeting, and that many of these readers have used GLEAM data for a variety of studies over the past four years (see e.g. refs. 1-3,5,7-9), including papers in Nature[10], Nature Geoscience[9] or Nature Climate Change[6]. It will not help these users if the references to GLEAM are omitted; the development of the science should be clear, progress should be documented.

Finally, let me please add one more note. Despite the algorithm similarities between STEAM and GLEAM, the latter estimates around 80% of land evaporation as transpiration (see ref. 4, that should be the one cited instead of ref. 10 in Table 4).

ESDD
5, C107–C112, 2014

This 80% is in line with other studies like ref. 11, that I personally feel that has not received an impartial treatment in the manuscript, given that all the criticism raised by ref. 12 was successfully addressed (at least in my opinion) by the *Jasechko et al.* reply presented below the commentary. This dissimilarity in the percentage of transpiration using parallel formulations (GLEAM 80%, STEAM 59%) underlines again the critical importance of model parameterizations, and the necessity to validate the separate evaporation fluxes in some way, if the authors aim to make any strong claim about their model's evaporation partitioning.

Looking forward to the authors' response.

Best Regards,

Diego G. Miralles
(personal correspondence at Diego.Miralles@Bristol.ac.uk)

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

ESDD
5, C107–C112, 2014

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