

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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Received and published: 11 June 2014

We thank D.G. Miralles for commenting on our manuscript. Below, comments by D.G. Miralles are in italic and our responses are in upright font. Unless otherwise stated, sections and equations referred to are those of the manuscript.

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).

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The full sentence (p. 206, L23) was "Perhaps as a result of the uncertainties and the scarcity of a global dataset on evaporative partitioning, no research on moisture recycling has considered the separate effects of physical and biophysical evaporation fluxes." The 'scarcity' was used to speculate on why no moisture recycling research has studied different evaporation fluxes separately. In the revised version, we will specify that the 'scarcity' refers to evaporation partitioning datasets at the high temporal resolution required for moisture recycling studies.

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, the PT-JPL product, or the GLEAM model. Global inter-comparison of these models, and their partitioning of evaporation, is the subject of projects like the LandFlux initiative of GEWEX (2010–present) or the European Space Agency (ESA) WACMOS-ET (2012–present).

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.

We referred to a range of studies on global evaporation partitioning (see e.g., Table 4), but agree that the mentioned studies and projects are also relevant to

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include. In the revision, we will add them to the literature review section.

We would like to clarify that the main justification of STEAM is **not** the 'scarcity of global data' — it is our research needs and interests in later moisture tracking analyses (see also our first response to Referee #1) that necessitate the combination of model simplicity and land-use change flexibility in simulating evaporation partitioning at a specific spatial and time resolution. In the revised manuscript, we will clearly state the rationale of STEAM to avoid misunderstanding.

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 guarantees 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). 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 —

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which could be assessed by using eddy-covariance measurements (like in all previous global evaporation models).

Comparison with GRDC runoff data does not constitute the only validation. We also checked STEAM total evaporation by land use type to independent studies (see Sect 5.3.1. and Table 6), river basin evaporation to WaterMIP (Sect 5.4. and Table 8), and LandFlux-EVAL (see Supplementary materials). The LandFlux-EVAL product (Mueller et al., 2013) also contains a FLUXNET-based global dataset (Jung et al., 2009), thus, allowing us to implicitly validate against eddy-covariance measurements.

Evaporation partitioning is challenging to validate. Uncertainties are large and the actual partitioning ratios debated (e.g., Coenders-Gerrits et al., 2014; Jasechko et al., 2013, 2014; Schlaepfer et al., 2014; Schlesinger and Jasechko, 2014; Sutanto et al., 2014). In our study, we compared our evaporation partitioning for different land-uses and regions to reported values from literature (Sect. 5.3.1.) and found that the results from STEAM fall within the range of previous estimates.

In the validation of GLEAM, the assumption was made that eddy-covariance field measurements do not capture wet canopy evaporation, and comparison was made between eddy covariance fluxes and the modeled evaporation excluding interception (Miralles et al., 2013). This approach can also be questioned as interception occurs also on ground, litter and low vegetation surfaces which eddy covariance measurements are able to capture. Thus, in areas with no or short vegetation, eddy covariance method measures the total evaporation (i.e. including interception from the ground and low vegetation). In forests, eddy-covariance measurements above canopy captures interception, while those below canopy will have difficulties capturing canopy transpiration (Wilson et al., 2001). In fact, eddy covariance measurements from FLUXNET is sometimes used specifically for determining interception (e.g., Czikowsky and Fitzjarrald, 2009).

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In the revised manuscript, we will include an expansion of the supplementary materials to show more in detail how results from STEAM compare with the range of total evaporation estimated by the models and datasets participating in LandFlux-EVAL.

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.

Manual calibration and expert judgment is not *per se* inferior to other methods. Look up tables for land-use parametrization is commonly used by non-satellite based global hydrological and land surface models alike. Because we have chosen to work with land-use parametrization (for ease of future land-use change experimentations), we are limited to the use of literature values. However, as we also discussed in the manuscript, 'the range of parameters in the literature can sometimes be significant and contradictory, due to discrepancies in scale, parameter definitions, and methods of parameter estimation. The choice of land-use parameters is therefore not simply taken as a mean from the literature values investigated, based on the preservation of the internal consistency of STEAM, manual calibration and priority for literature values with higher relevance.' (p. 215, L6-11). To illustrate why expert judgment can be superior to 'objective' averaging or other statistical treatment, we take the example of leaf area index. While we included both the study of Federer et al. (1996) and Van den Hurk (2003) in our literature review, it was clear that the values of Federer et al. (1996)

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were more "best guesses" while Van den Hurk et al., (2003) were better supported by derivation from satellite data. Thus, we chose to align our leaf area indices much more closely to those of Van den Hurk et al. (2003) rather than Federer et al. (1996). There is to our knowledge no complete IGBP land-use wise look-up parameter table based on satellite data available, and it was beyond our scope to create such a table. Thus, although we agree that there is room for future improvement, we find our approach justifiable at present.

There is an obvious resemblance between STEAM and GLEAM, 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 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

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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, Nature Geoscience or Nature Climate Change. 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.

It is incorrect that we have 'omitted any reference'. The manuscript included several references to '(Miralles et al., 2010)'. However, as D.G. Miralles points out, this reference in Table 4 should be corrected to '(Miralles et al., 2011)'.

We disagree that the resemblance between STEAM and GLEAM is obvious. Below we respond to each of the examples of similarity (a-f) that D.G. Miralles raises:

- a) *'evaporation model dedicated to the partitioning of terrestrial evaporation at the global scale'*. STEAM is dedicated to simulating evaporation and evaporation partitioning **based on land-use types for coupled use with the moisture recycling model WAM-2layers**.
- b) *'based on the offline forcing of a radiation-driven formula of potential evaporation'*. Offline forcing by radiation-driven evaporation forcing is a feature widely shared by global hydrological models (see e.g., Wang and Dickinson, 2012) and not at all uniquely GLEAM. Note also that the potential evaporation formula is not the same. GLEAM uses the Priestly Taylor formulation (Miralles et al., 2011), whereas STEAM uses the Penman-Monteith formulation.
- 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'*. Again, the principle to use a running water balance to constrain potential evaporation is widely employed by hydrological models and used

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at least since the early Budyko Bucket models from the 40s (see e.g., Shuttleworth (2012), Beven, (2012)).

- d) *'uses a water balance model to estimate interception'*. Again, using a water balance for interception modeling is not at all uncommon. Looking in detail, we find the interception simulation procedure to differ considerably. GLEAM uses a revised version of Gash's analytical model. Their interception is calculated daily, involves assumptions of rain event phases, and precludes short vegetation interception (Miralles et al., 2010). STEAM uses a bucket formulation, operates on 3 hour basis, and allows interception for all land-use types.
- e) *'is run with ERA-Interim inputs'*. The data input is not a unique feature of the model, and ERA-Interim is a widely used product. Our reason for using ERA-Interim is for spatial and temporal consistency with WAM-2layers, which has used ERA-Interim since the first version (van der Ent et al., 2010).
- f) *'has been developed by a hydrology group from a Dutch university (just a few years later)'*. True, but we do not see the relevance of this similarity.

As for the model name, we think the names are sufficiently different for the scientific community to easily recognize them as two different models. First, GLEAM stands for Global Land-surface Evaporation: the Amsterdam Methodology, while STEAM stands for Simple Terrestrial Evaporation to Atmosphere Model. Second, many models have had much more similar acronyms and yet successfully co-exist: e.g., SEBS and SEBAL, MOSAIC and MOSES, SWAP and SWAT.

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).

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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.

Also described in the manuscript (see Sect. 5.1), Schlesinger and Jasechko, (2014) published a new article after the Nature commentary-reply chain. In this new article, they discussed that isotope studies tend to overestimate transpiration and lowered the global transpiration ratio estimate to 61%, which is almost the same as the 59% estimated by us. In the work of Sutanto et al. (2014) they conclude that isotopic measurements and global models all have their own (dis)advantages and it remains difficult to upscale point measurements to a global value. Just recently, the work of Jasechko et al., (2013) (i.e., the ref. 11 that D.G. Miralles refers to) was also challenged by Schlaepfer et al., (2014). However, we do not intend to be perceived as biased, and will add the Jasechko et al. response as well the other recent critics of Jasechko et al., (2013) to our discussion in Sect. 5.1.

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