Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2015-551-RC2, 2016

Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Comparison of satellite based evapotranspiration estimates over the Tibetan Plateau" *by* J. Peng et al.

Anonymous Referee #2

Received and published: 16 February 2016

This very brief paper is aiming to perform a comparison of six existing global evapotranspiration (ET) products over the Tibetan Plateau (TP). Even though it is an important topic and within the scope of HESS, I cannot recommend this manuscript for publication and would recommend its withdrawal and resubmission after a thorough reworking. The details for this recommendation are presented below.

Tibetan Plateau

Even though the TP is prominently mentioned in the title and abstract, there is no TP specific discussion present in the paper except for its description in the introduction. The TP is chosen as an area for comparison of the six models, but it might just as well have been any other region. There is no discussion of TP specific features such as varied topography, ice/snow cover, vegetation characteristics, generally dry conditions,

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Discussion paper

low atmospheric pressure, etc. Similarly, there is no discussion of suitability (or not) of any of the ET products in such conditions based on the underlying assumptions of their models and the input datasets.

For example how valid are the 1 degree datasets in such heterogeneous terrain? Do they accurately reflect the changes in surface or air temperatures with changing elevation? Or, how do the different models handle snow/ice cover, which in a region like TP this might have very significant impact on the accuracy of the modelled ET? Is it treated as in as in Vinukollu et al., 2011? In reply to reviewer the authors of the Miralles et al. (2015) publication have stated that PM-MOD and PT-JPL do not have any special modification for treating snow covered areas and use the same parameterisation as for the underlying land cover. The Loew et al. (2015) manuscript also does not described how HOLAPS deals with snow cover and indeed the reviewers of that manuscript have asked for this information. However, the current manuscript does not even use the word snow once. The above two points are just examples and there are other TP specific issues which should be discussed.

Additionally, the four sub-regions of TP used for more detailed analysis appear to have been chosen arbitrarily. If there are indeed some specific reasons of why TP was split into those sub-regions, then this should be made clear and the characteristics of each sub-region should be described. Otherwise this split serves no useful purpose and no additional information is gained compared to evaluating the models over the whole TP. What's different about region 4 compared to other regions that the results are different? It would have been much more interesting to split the area based on land cover or climatic zone or any other important property.

Six ET products

Almost no description of the ET products is presented. It is OK to refer the reader to the original publications for the details, but at least a basic description of underlying principles of each model should be presented, together with the major differences between them. The same goes for the different meteorological and radiation forcings. For example, Vinukolly et al. (2011) on page 4007 states that SRB albedo was unrealistically low in snow conditions. How would this affect the outputs in TP? Was albedo varied seasonally in this study or kept constant? What about other inputs such a leaf area index or land cover?

There is also no clear justification of why those ET products were chosen for this study or the applicability of LandFlux-EVAL to be used as the "benchmark" ET. What is its internal variability and applicability to TP?

Additionally, the HOLAPS model is clearly favoured by the authors (who are also the authors of the manuscript describing HOLAPS) and it is presented already in the introduction as the best model. That could be fine if the manuscript is reframed as "evaluation of HOLAPS over TP". Otherwise it appears that the conclusions were reached before the study was conducted and that is not very scientific. Finally, the HOLAPS study has not been published yet in the final form so it might be a bit too early to submit this manuscript.

Comparison

There is severe lack of details and analysis of the results of the comparison. For example, why only spatial patterns of the ET averaged over the whole study period are shown. In an area such as PT, where presumably there are large annual variations in phenology and snow cover, a seasonal/monthly comparisons should also be presented. Also, to better understand the spatial patterns the maps of land cover, rainfall, snow cover, etc. should also be shown.

Other questions should also be explored. For example, what drives the differences between the different models. Are the differences larger in specific land covers, specific altitudes, specific time periods, etc? How could the model assumptions impact on the differences? What about other fluxes? Are the differences due to net radiation or partitioning into H/LE? This last question could be addressed by running the HOLAPS model with the same atmospheric and radiation forcings as other models. Or are the differences due to the spatial resolution of the input datasets. In such heterogeneous terrain the errors in the 1 degree forcings must be significant. Could HOLAPS be run with the inputs resampled to 1 degree?

Finally, there are TP focused studies presented in the introduction but there is no mention in the discussion how the magnitude and distribution of ET from the 6 products compares to the ET from those studies.

Specific comments

L47-54 There are other models (e.g. ALEXI) which can achieve a similar spatial and temporal resolution as HOLAPS. This was mentioned by two reviewers in the open discussion of the Loew et al. (2015) manuscript.

L81 The statement "especially HOAPS" should be justified or removed. This is the introduction and so far no results have been presented so this statement outlines the pre-conceived conclusion before the study is performed.

L101 What is the reference for the Surface Radiation Budget.

Section 3.1 There should be more discussion about the causes of the spatial patterns and the expected spatial patterns. Also it seems that there is a large seasonal difference in ET. Therefore spatial patterns split into seasons/months should be also shown and discussed.

L143 It appears that the sub-regions were chosen arbitrarily. Or do they have any differentiating characteristics? If not then it would be more informative if they were chosen based of a split in LC/climatic zones/altitude, etc. Each sub-region should be described.

L150 Patterns produced by PT and PM models are very similar, while SEBS outputs different fluxes. This should be elaborated on.

L151-152 What are the differences (apart from spatial resolution) between the forcings. Which one is more suitable for TP.

L159-161 PT and PM models are also within the range.

L164-165 Minimum ET is in winter, maximum is in summer.

L170-174 This seems to be important but is not elaborated on at all. Could this mean that SEBS estimates are potentially more accurate while LandFLux-EVAL is not really accurate enough to be used for benchmarking? LandFlux-EVAL also reports that its ET is towards the lower boundary of other studies (p3713 Mueller et al, 2013).

Figure 2 HOLAPS and SEBS-Chen maps should be resampled to 1 degree to allow simpler visual comparison.

L202-206 What is different about region 4 compared to other regions and the whole TP.

L207-208 This sentence does not bring anything useful to the discussion. The quantification and reduction of uncertainties might be a subject of future studies but at least those uncertainties should be described and discussed.

L209-211 There is no discussion in the results regarding the spatial resolution of any of the products or their applicability in TP environment, so this statement is unsubstantiated.

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