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Interactive comment on "Comparison of satellite based evapotranspiration estimates over the Tibetan Plateau" *by* J. Peng et al.

Anonymous Referee #1

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The authors present a short comparison study that compares evapotranspiration (ET) products over the topographically complex Tibetan Plateau. While accurate ET products on regions of TP are a challenge and thus demand attention, I am left a bit confused about the purpose of this paper and the value it presents to the scientific community. I feel that merely plotting a comparison of the data products without much further discussion does not warrant a publication on its own. The manuscript is well written, and the topic is very suitable for HESS. I think that major revisions could greatly improve the paper and make it a good contribution to this important topic.

General Comments

The paper is very short and there is a limited amount of in-depth analysis that is being done to compare 5 different data-sets/ approaches to a reference synthesis product.

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Discussion paper

The main conclusion seems to be that different algorithms that use different input data produce different results and that HOLAPS is somewhat better in comparison to the reference data set of which we don't know, how good it is on the Tibetan Plateau.

One major problem I see in the analysis is that the datasets and reference products have very different spatial and temporal resolutions, which makes a meaningful comparison problematic. This is exacerbated by the fact that ET on the semi-arid to arid TP is strongly dependent on influences of the Asian summer monsoon, which propagates northward during the summer and reaches different areas at different times (or not at all on northern TP), where dry westerlies dominate. I would suggest that the analysis should be included.

I think it would be beneficial if the authors could add a discussion that 1) tries to assess why the products produce the results that are presented here. 2) discuss the reference product and its validity on TP or at least point to such a discussion if already published by other authors.

The reference dataset is monthly only, which obviously does not allow for assessment of the products below the seasonal scale. I would expect that a lot of uncertainty in ET products on TP is on diurnal and short timescales due to monsoon dynamics and complex topography.

Due to the short nature of the paper, which almost seems to be more of a technical note, I think that such a discussion could easily be warranted without making the paper too long.

Please find below specific comments, where I think some of these general challenges could be addresses.

Specific comments:

L 49: HOLAPS (Loew et al.): At this point, the cited reference is still in review and reviewers seem to be criticizing gaps in the paper. I feel that a bit more introduction to

HOLAPS, which the authors of this paper are associated with, seems needed.

L73: "However, accurate estimation of ET over TP is still a challenge due to the limited in situ observations" - This is correct, however there is a network of flux and energy balance stations on the Tibetan Plateau, of which one of the co-authors certainly has access to the data. As HOLAPS resolutions are high, I think some comparison to this data is needed.

L118: "Since are no reliable in-situ measurements available over the TP for the 118 current study period" - Ma et al 2008 (who is a coauthor) introduce a network of flux stations that was established in the 2000s. So there should be at least some estimates of LE. If these are not available for 2003, why was this period chosen? Ma et al (2009) also estimates ET for the Plateau from satellite, which could potentially be used to compare?

Section 2: It is not entirely clear to me, whether the authors use the datasets from Vinkullu and Chen or whether they calculate these themselves. Maybe this could be added in a sentence.

L 142: "over the whole TP and four sub-regions (see Figure 1)." - Why were these subregions chose? It would feel more natural to divide these subregions to reflect climate/ monsoon influence rather that just lat/ lon.

L 150-152: "The differences between SEBSSRB-PU, PTSRB-PU and PMSRB-PU are attributed to the differences of the models. But also for the same model, different forcing data lead to different results (SEBSSRB-PU and SEBSChen). These results suggest that model and forcing are equally critical for the estimation of ET (Vinukollu et al., 2011)." - This is a trivial result, but the why is important. Maybe this would be the area to add some discussion.

Figure 2 and L153: "Overall, the HOLAPS dataset is found to have good agreement with the benchmark product (LandFlux-EVAL) with similar spatial pattern of LE" - I find

this difficult to discern based on the resolution difference. I suggest to add a figure, in which HOLAPS and SEBS_Chen are spatially averaged to the same resolution to allow for a better comparison.

Figure 4: Based on the figure, I would say that there is little difference between the products with exception of SEBS, which does not seem to work well on TP. Do the authors have a comment on why that is.

L 164-166: " In general, all products capture well the seasonal variability with minimum LE in the summer and maximum LE in the winter. However, the mean values of different LE products differ substantially" - I think that as stated above summer and winter may not be the most meaningful category as ET is mainly driven by water availability from the monsoon. So it would make sense to have at least winter (cold), dry and wet (monsoon) for discussion.

Figure 5: HOLAPS seems to do considerably worse in region 4, which is the region in which the there is most moisture available, monsoon influence is strongest and which probably has the densest sensor network. While the other 3 regions are much drier and more "remote." I feel that this potential bias for wet areas/ wet season should be explored. Region 4 is the smallest region though, so that bias may be hidden in the overall comparison (due to area averaging effects).

Conclusion: The conclusions reflect the paper, but as stated above, I feel that the results at this stage should be supplemented with a more in depth discussion of processes.

Technical comments:

L18: "Land-atmosphere interactions are largely influenced by surface latent heat fluxes ..." - In my opinion LE is an important part of land-atmosphere interactions and does not influence Land atmosphere interactions.

L19 "... due to its unique and special geographical position and physical environment"

- This sentence does not convey any meaning, if not followed up with specifics.

L29: " with ET decreases " - with decreasing ET

L93: While PT and PM are standard, I feel SEBS warrants a citation.

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