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Interactive comment on “Combination satellite and in-situ data for the determination of evapotranspiration over heterogeneous landscape of the Tibetan Plateau” by Y. Ma et al.

Anonymous Referee #4

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The Tibetan Plateau is a very unique place. Understanding the surface energy partition for the area, especially differences between the area and the other area with the similar land surface type, is important. The subject of the manuscript fits ACP well. However, the manuscript is very similar to their early paper published in the same journal in 2011, i.e., the same methodology (even though “a new parameterization method” was claimed in the abstract), the same Fig.1, the almost same Fig. 2, and the similar Figs 3, 4, 5, and 6. The title of the manuscript is on LE, but the authors did not even change their symbols for the definition of APD in this manuscript and copied the one used in their 2011 paper, where the focus is on H. The only difference is that the authors

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introduced another satellite dataset, AVHRR to compare it with the satellite dataset from MODIS. Even for this difference, the authors failed to explain why the MODIS dataset is better than the AVHRR one. I do not see significant scientific values in this manuscript compared to their 2011 paper. Therefore, I suggest rejection of the manuscript at the current form.

Here are some suggestions if the authors want to resubmit the manuscript. Some of the issues are relevant to their 2011 paper too.

1) If the difference between the two satellite datasets is the focus of the paper, please say so from the title and describe how they are different in the text. The authors already demonstrated their strategies to derive H and LE in their 2011 paper.

2) In terms of the methodology, the authors need to clearly state the limitation of the method. As I understand the approximately constant EF is only valid during daytime under clear sky conditions with no serious weather conditions and soil moisture limitations. Theoretically EF has to depend on land surface types. To me, it is much valuable to describe how good the invariant EF method works for different types of land surfaces. Can the authors describe just briefly how well the calculated downward solar radiation compared to the observed one if there are any?

3) To compare the H and LE measured in the fields with the satellite derived ones, the authors need to explain the necessary details, such as what kinds of measurements are available at all the towers? How are the sensible and latent heat fluxes were derived? From Fig.3, it looks like the bulk formula is used to derive those quantities and no direct eddy correlation measurements are available. If so, how are all the relevant parameters, such as z_{oh} and d_0 , derived at each tower? What does each dot represent in Fig. 6 for the important comparison? What is the averaging time? There is a significant number of points in Figs. 6c-d that EF is larger than 0.5. They seem to be high for the NDVI map demonstrated in Zhong et al. (2010).

4) Please use symbols consistently. If EF is the one that the authors would like to focus

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on why introduce Δ ?

5) For clarity, it is better to list all the stations and their relevant information in a table.

6) Please define all the symbols used in the manuscript. I assume LT in Fig. 3 is local time? The units should be hours? Also, how were all the data derived in Fig.3? Are they averaged diurnal variations over a period of time or for a particular day?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8435, 2013.

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13, C5907–C5909, 2013

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