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Interactive comment on "Global assessment of how averaging over land-surface heterogeneity affects modeled evapotranspiration rates" *by* Elham Rouholahnejad Freund et al.

Anonymous Referee #1

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The manuscript presents a global-scale assessment of the effects (i.e., bias) of large-scale averaging of atmospheric forcing, namely P and PET, on evapotranspiration rates. The assessment relies on the application of a previously published approach based on the Budyko framework and using existing datasets of P and PET. Additional analyses are presented at the continental- (i.e., CONUS) and regional-scale (i.e., Switzerland) in order to explain the role of climate and the scaling of evapotranspiration bias with the grid resolution, respectively.

The work addresses an important subject of research (i.e., how to quantify averaging effects in large-scale models), which is certainly of relevance for HESS readership and

for a broad scientific community. However, in its current form the work appears too much a mere application without offering new insights on such a relevant subject. In my opinion, this makes the contribution (after substantial revisions) more suitable for a technical note. I provide below a list of points that support my evaluation.

1. Authors motivate the need of their work (i.e., quantification of averaging effects on evapotranspiration) and discuss their results in light of well-known ESMs simplifications that do not take into account the fine-scale spatial heterogeneity in the atmospheric forcing and land surface characteristics. However, the averaging effects are assessed under steady-state conditions and neglecting non-linear land surface processes, two assumptions that do not reflect the actual way ESMs (and large-scale integrated models) are implemented. While authors clearly disclose these limitations (see lines 225-226; lines 229-230; lines 271-274 lines 361-364, etc...), my impression is that at the end of the article the reader is left to wonder about the maturity of the work. For instance it is not clear how current results can be exploited to calculate bias correction factors for ESMs simulations. Sentences as those reported at lines 363-364 ("... we are now working to quantify aggregation bias in ET...) support somehow my impression that reported results are in a sort of "intermediate" stage.

2. In a similar vein to the previous point, one of the main conclusions is that from 'an atmospheric perspective' averaging is for sure an approximation in those regions characterized by strong topographic gradients. This is probably not new and authors could have made an effort to explain better the different degree of sensitivity between P and PET.

3. Another point of the work (summarized in Figure 4) is that using different datasets we end up with different averaging effects. Again, was this not expected? Could authors provide an explanation of the different degree of sensitivity between P and PET datasets? If you do not provide any insight on this how can you claim (in the abstract) that your work discusses the underlying mechanisms of such differences? I have the same concern for Figure 5. The ordering (see lines 275-276) is really not informative.

4. According to Figure 5 it seems that when estimating ET the spatial heterogeneity matters for a minor portion of CONUS domain and when using certain datasets. Is this realistic? These results seem in conflict with efforts (of several groups) that have emphasized the need of including lateral moisture distribution in ESM simulations. Including this lateral effects may completely change the "picture", am I wrong? This concern brings me to the introduction (lines 145-146) when authors highlight the need of a general framework for systematically quantifying biases in ET estimates due to spatial averaging. Is it the case at the current stage of the work?

5. The discussion around Figure 6 is not clear. If I compare at single grid points the color scale at 1/32°, 1/16°, 1/8° and so on with the ones at 1° and 2°, I see that the magnitude of the bias is not increasing, isn't? Further, the bias extends over larger areas because you're increasing the resolution, am I wrong? In any case, here it is important to implement some statistics that accurately quantify the scaling of the bias with the grid resolution.

Specific points: - The title is a bit misleading because you're accounting just for land surface heterogeneity related to P and PET.

- Key points cannot be defined as long and multiple sentences

- In the abstract you cannot make a long discussion of a previous work findings. Please revise.

- Line 70: What do you mean with "grid-averaged land surface parameterizations"?

- Paragraph between lines 99-108 appears disconnected from the main flow of the introduction.

- Lines 126-128: The issue of spatial averaging is also due to the fact that atmospheric and land surface components of ESMs "work" at different resolutions. In order words, it is not " just" a problem of data volume and computational limitations.

- Line 122: what do you mean with "likely" magnitude?

- In Eq. 1 "n" is not defined. How is it estimated?

- How PET is calculated in the two datasets? This point has to be discussed in order to provide additional information about possible discrepancy between the many existing approaches to calculate PET

- Figure 3e and Figure 4: How did you calculate the percentage averaging error?

- Lines 269-270: Saying that "perhaps due to the sharp gradient. . ." is a weak statement that conveys the impression that you're not really sure about the interpretation of the results.

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