

Interactive comment on “Constraining model parameters on remotely sensed evaporation: justification for distribution in ungauged basins?” by H. C. Winsemius et al.

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The paper sets up a framework for exploiting remotely sensed information, namely evapotranspirations estimates obtained by means of SEBAL, in the calibration of a semi-distributed model. The Authors observe that such information is effective in reducing the range of variability of the model parameters controlling such process. Once delineated a limited number of hydrological units based on land cover classification, the model is run on a given prior parameter distribution; the modelled ET is compared to the SEBAL estimates for obtaining the posterior parameter distribution. For cer-

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tain land cover classes the parameters appear clustered together, giving the chance to make their range much narrower for further calibration efforts. For other land cover classes the parameters posterior distribution is quite spread, giving the chance to further subdivide the area, or show that the initial choice of the parameters' range is not suitable for that class and further model runs are needed.

The articles is an interesting application of a combined approach for hydrological studies in ungauged basins.

However, in the current version the articles leaves several issues open or not clearly discussed.

In particular, I would ask the Authors to revise the model description, with a special attention in defining in a hydrologically sound way the model parameters, state which are the data requirements and how did they handle them.

Ancillary data requirement specifications are needed for SEBAL as well, with a proper discussion of their (ECMWF fields) representativeness.

I have the impression that SEBAL estimates are poor over certain classes (overestimation of ET giving way to increases of S_{max} ? Is it more likely than water abstraction from the groundwater reserve in highlands?) and I would like to know the opinion of the Authors on this.

They should discuss the effect of using only one season and whether they do expect that the parameters posterior distribution changes in long term applications.

Moreover, I would the Authors to try to give a prior estimate of the parameters' ranges for the different classes before carrying out the analysis, e.g., looking at Allen et al. (1998) tables on rooting depth and soil water depletion fraction.

They mentioned that they used Google Earth for a survey of the area. It would be nice if they could provide some klm maps (e.g., basin boundary, model units, land cover classes), thus allowing the readers to easily "Google Earth" the area as well. However,

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I managed to reach it by typing in "Muchinga".

I am wondering why the Authors did not choose to perform such analysis in a gauged basin, where the additional information could have helped clarifying the hydrological behaviour of the area and the eventual source of uncertainty within the model and the SEBAL estimates. Is there any chance to make use of the (poor) local data for further analyses?

The article needs a careful language check. The commas are often misused, sometimes the verbs are not correctly coupled to the subject.

Further comments, corrections and requests are listed in the following, labelled with PAGE-LINE(/LINE).

2294-8: Evaporation. What about transpiration? see also 2297-29.

2294-12: "similar land cover": what about something like "land cover aggregates"?

2294-15: "behavioural parameter sets": what does it mean?

2294-22: "field capacity": not used anymore in the text. See the comment on model description.

2295-25: "measurement networks are collapsing". Provide an evidence of that, or just use a softer "un/under-gauged" concept.

2298-2: "The thermal-infrared character of SEBAL". what does it mean? the use of TIR and VIS information ...

2298-13: the data availability (and the meaning of fig.1) is not discussed properly. what is GHCN? and local?

2298-19/22: This sounds reasonable, but highly suspicious: it is clear that the SM conditions are not so sensitive to the precipitation fall in a certain day, but what about strong under- or over-estimates? And what about long term runs?

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2299-2/3: "assuming that errors are uncorrelated in time and space". This is certainly not the case for remote sensing products: if you make mistakes on the estimation of forest ET, it will be systematic in space and time! Do you need that condition in any step of the method?

2299-6/7: "with only a small amount of old hydrometric station data (at the time of writing only one was fully operational, installed in November 2007)". Not relevant there.

2299-7/19: Try to rephrase for presenting in a systematic way morphology, soils, vegetation, and climate for each subdivision.

2299-17: What about the seasonal distribution of the annual rainfall?

2300-12/14: "Unfortunately, evaporation cannot be assessed for a complete hydrological year, because during the wet season, no cloud-free images can be found for this region". Did you explore the possibility of making use of geostationary satellites and a space-time compositing?

2301-4/6: "Two known anchor points need to be selected where $H=0$ and $H=R_n-G$ are fulfilled (i.e. the "dry" and "wet" extremes in the satellite image).". It is just the opposite. In wet conditions $H=0$; in dry conditions $H=R_n-G$. Moreover, it is not clear how to select the anchor points: two pixels in a single date image, hence to be repeated for each image? Is it meaningful to apply this over large areas / different vegetation conditions?

2302-24/25: What about defining in a hydrologically sound way (e.g., soil water content at saturation) the variables instead than redirecting to HBV? And why to change their symbols with respect to that?

2303: It is not mentioned where do the meteo input (P and T_p) come from and how are they adjusted over the different model units.

2306-7/9 and figure 4: Can you label the two riverine units with different symbols? It seems that there are two clusters in both the parameters graphs.

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2306-10/25: In my opinion the evaluation of the results for highlands is questionable. What happens in case of a relevant SEBAL overestimation of ET? The posterior parameter distribution will mirror the efforts of the model to compensate ET with higher initial storage and lower I_p (it will keep ET close to its potential rate for longer periods).

2306-26 to 2307-12: As for riverine, it could be useful to represent the units with different labels. Perhaps the further subdivision needed is already there. What do you mean for "The reason for their dry-season dormancy may well be temperature related"? Is it cold or warm during the dry season? This drives back to a previous question on T_p .

Figures.

1. There is almost no description within the text. The acronyms are not spelled out anywhere. The availability is not described as well.
2. Try to enlarge a bit. Is it worth to add also a DEM?
3. What about merging the maps on the left? On the right part the recharge flux (r_c) is not labelled. In the soil storage, the curve starting from S_{max} is not described (if it is needed). Enlarge both of them.
4. Enlarge. I_p is not labelled correctly. Try to use different labels for each hydrological unit.

REFERENCES

Allen, R.G., Pereira, L.S., Raes D., Smith, M. Crop evapotranspiration - Guidelines for computing crop water requirements, FAO Irrigation and drainage paper 56, Rome, 1998.

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