Hydrol. Earth Syst. Sci. Discuss., 5, S1308–S1316, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S1308/2008/

Hydrology and Earth System Sciences Discussions

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

H. C. Winsemius et al.

Received and published: 1 October 2008

We are very glad with the reviewer's comments. Some of the important issues brought forward are that we haven't discussed the effect of possible bias in the SEBAL estimates on our posterior parameter distribution, and that we could do a better job in model, parameter and choice of priors description. Below we will carefully address all the points brought forward and hope this discussion will proof fruitful.

Major comments: Referee: 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.

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Interactive Discussion

Discussion Paper

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

Authors: we agree that some additional information can be given on model input and parameters. We propose to add a table where we describe model parameters, their units, physical meaning and chosen prior range. Furthermore, the input data used was a remotely sensed rainfall product, largely based on measurements collected by the Tropical Rainfall Measuring Mission (TRMM, Huffman et al., 2007). Potential evaporation estimates were actually quite complicated. We obtained local measurements from the meteorological institute for minimum and maximum temperatures, relative humidity and wind speed. To ensure that the spatial variability in potential evaporation was met, we blended these in-situ measurements with monthly climatology fields of relative humidity, wind and temperature of the Climate Research Unit (New et al., 2002), following an approach described by Reynolds (1988), who applied it on buoy measurements of sea surface temperatures. We will add all this information in the revised manuscript.

Referee: I have the impression that SEBAL estimates are poor over certain classes (overestimation of ET giving way to increases of Smax? 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.

Authors: The assumption that (p.,2304, l. 12-13) “the quality of the SEBAL data is reasonable and at least unbiased” may indeed be a critical, especially over areas with a high degree of cloud cover. We propose to add a paragraph in the final manuscript, where we show what the effect of an overestimation of evaporation would be on posteriors of S_{max} and l_p for one of the model units in the highlands, where indeed cloud-cover could be a large problem for this technique. Sometimes clouds are simply extremely difficult to detect. A logical expectation is that indeed S_{max} will prove to be lower.

Referee: 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.

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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.

Authors: Expectation is that more information will yield less uncertain parameter ranges. Reason for not making a longer time series, is simply that it is extremely time consuming to generate these estimates, especially under ungauged circumstances. This is another reason why we do not recommend to use SEBAL purely as calibration data in natural areas. There is a too great deal of uncertainty involved to justify this. As for the tables, presented by Allen et al. (1998). The problem here is that these tables typically represent crops for agricultural purposes, and not for the unique Miombo vegetation in the Luangwa basin. In fact, we want to show that the SEBAL evaporation estimates in combination with a conceptual hydrological model can teach us what these parameters should be in ungauged basins. Therefore we selected very large prior ranges instead of making any assumption on them. Model application actually taught us that our model concept is not likely to be the right one for all the land cover classes.

Referee: They mentioned that they used Google Earth for a survey of the area. It would be nice if they could provide some kml maps (e.g., basin boundary, model units, land cover classes), thus allowing the readers to easily “Google Earth” the area as well. However, I managed to reach it by typing in “Muchinga”.

If possible and easily accessible in HESS, we can provide a kml map as supplementary material. We will inform the HESS publication office if this is regularly done. If not, we hope that the provided maps contain enough detail about the geographical location to help a reader to find the area in Google Earth.

Referee: 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

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model and the SEBAL estimates. Is there any chance to make use of the (poor) local data for further analyses?

This is an interesting question, which has been posed to the first author many times. First of all, the Predictions in Ungauged Basins (PUB) initiative resulted in a great number of studies that were all performed in a sort of jack-knifing context. This may prove useful to develop methodologies, but we think that after 5 years in the PUB-decade, efforts should move to truly ungauged basins, where the need for improved understanding of hydrological behaviour is necessary for prediction of future change. Secondly, for evaporation data to be truly informative, it is necessary to investigate semi-arid catchments. In temperate climates, evaporation is generally energy constrained, which makes the constraining power of evaporation estimates much lower (i.e. evaporation is often near the potential rate). Unfortunately, most semi-arid areas where our group works are ungauged. We are now in the process of combining all (weak) local information with the remotely sensed information to constrain the parameter space of the whole model structure.

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

We will re-check the whole manuscript before re-submission.

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

Ref: 2294-8: Evaporation. What about transpiration? see also 2297-29. Auth: Evaporation is the process, where water transfers from the liquid to the gas phase. Transpiration is one of these evaporation processes. In our terminology, 'evaporation' therefore covers all evaporation process, (i.e. from intercepted rainfall, open water, soil, transpiration). We follow the same terminology as for instance Brutsaert (1982).

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

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Auth: Maybe indeed not a clear term: what about ‘dominant land cover characteristics’?

Ref: 2294-15: “behavioural parameter sets”: what does it mean? Auth: These are parameter sets that yield ‘behavioural’ model results given the SEBAL information. In other words: well performing model parameter sets. This term is often used in statistical hydrology (e.g. Beven Binley 1992)

Ref: 2294-22: “field capacity”: not used anymore in the text. See the comment on model description. Auth: We will add this term in the model description (see reply on first comment).

Ref: 2295-25: “measurement networks are collapsing”. Provide an evidence of that, or just use a softer “un/under-gauged” concept. Auth: We will at least refer to Figure 1 for this and look for a reference for this.

Ref: 2298-2: “The thermal-infrared character of SEBAL”. what does it mean? the use of TIR and VIS information. Auth: Indeed, the use of thermal long-wave information. The sensors needed for SEBAL are not cloud penetrating. We will revise this sentence to make this more clear.

Ref: 2298-13: the data availability (and the meaning of fig.1) is not discussed properly. What is GHCN? and local? Auth: We agree that this figure needs some more explanation. We will add more information after p. 2298, l. 13. For the referee: GHCN is the Global Historical Climatology Network, which provides monthly in-situ records of rainfall, temperature, relative humidity and wind speed.

Ref: 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?

Strong over or underestimations will of course reflect on the model states. However, the non-linearity of the model helps to reduce this effect: semi-arid river basins need a certain degree of saturation before significant flow is generated. If there is a bias in the

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rainfall, then most (not all of course) of this bias will be reflected in the discharge. We agree that there is an assumption: the rainfall should be reasonably timed within the rainy season and should not have a too high degree of bias.

Ref: 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? Auth: This is not necessarily the case: if errors are due to a wrong estimation of for instance surface roughness (i.e. a parameter that remains constant in all SEBAL derivations), then this may be true, but most errors will be caused by random uncertainties, such as the atmospheric properties at the moment of satellite overpass and the hot and cold pixel selection. Therefore we think that this assumption is valid, except perhaps over areas where we may have consistent undetected thin cloud coverage. The authors do not fully understand why this would be different for forest evaporation than for other vegetation types. We are happy to further discuss this issue with the referee of course.

Ref: 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. It may be relevant to prove the ungauged character of the river basin. Therefore we would like to keep this statement.

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

Ref: 2299-17: What about the seasonal distribution of the annual rainfall? Auth: We will add a comment about the rainy season here.

Ref: 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? Auth: This is a very interesting research venue

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and we have been considering to research this. However this is not straightforward and should be done as a separate study. We focus of this study was to apply an established technique in a modelling framework.

Ref: 2301-4/6: “Two known anchor points need to be selected where H=0 and H=Rn-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=Rn-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? Auth: The two should indeed be opposite, this will be corrected. Indeed the selection of these pixels is a source of uncertainty and is repeated for each image. It can be done over large areas as long as land surface temperature is corrected for elevation differences (i.e. the assumption is that $\mathrm{d}T = at_s + b$) and as long as the atmospheric conditions are spatially more or less constant, which can be assumed during cloud-free days.

Ref: 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? Auth: This will be done in table form, where the physical meaning of parameters will be added as a description. In the HBV convention, symbols are written as in computer language and not as mathematical symbols (i.e. mathematically, FC would mean $F \times C$). In our opinion it can lead to misunderstanding, especially when the paper is read by somebody who is not aware of the typical HBV symbols for parameters.

Ref:2303: It is not mentioned where do the meteo input (P and Tp) come from and how are they adjusted over the different model units. Auth: We will further explain this (see previous comment on TRMM and potential evaporation)

Ref: 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. Auth: This is true,

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the mode and distribution of the different units will never be entirely the same. If you look closely, you'll see different modes for each model unit in each class. Because of this, we use the combination of distributions to construct the (possibilistic) fuzzy measures for each land cover, instead of choosing a (probabilistic) distribution function. It is just very clearly visible in the riverine case.

Ref: 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 lp (it will keep ET close to its potential rate for longer periods). Auth: As mentioned earlier, a good comment which we will investigate as described.

Ref: 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 Tp. Auth: We will improve on this: in short, Chidumayo (2005) suspected that the reason for leaf shed with Miombo forest is because of low temperatures. It is simply an example of what we could learn in terms of model improvement from this exercise: if we model Miombo covered lands as moisture constrained, the model will predict poorly in future (climate) change scenarios in case the land cover is actually temperature constrained. I.e. in a moisture constrained model, transpiration would occur with a higher rate after the wet season but would stop earlier than in the recent case, because the moisture store depletes. In a model where surface resistance would be temperature constrained, the Miombo surface would perhaps keep on transpiring because it can cope with the milder winter temperatures in the climate change scenario at the cost of depletion of deeper groundwater.

Figures. 1. There is almost no description within the text. The acronyms are not spelled out anywhere. The availability is not described as well. Auth: we already commented on this and it will be enhanced.

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2. Try to enlarge a bit. Is it worth to add also a DEM? Auth: the second reviewer suggested the same. We will make the Africa figure much smaller and include a DEM in the background.

3. What about merging the maps on the left? On the right part the recharge flux (rc) is not labelled. In the soil storage, the curve starting from Smax is not described (if it is needed). Enlarge both of them. Auth: We will include r_c and will split the figure in two figures.

4. Enlarge. lp is not labelled correctly. Try to use different labels for each hydrological unit. Auth: We will correct this.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 2293, 2008.