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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Spatial pattern evaluation of a calibrated national hydrological model – a remote sensing based diagnostic approach" *by* Gorka Mendiguren et al.

**Anonymous Referee #1**

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## 1 General comments

The manuscript is dealing with the topic of spatial patterns in distributed hydrological modeling. The authors present a study in which they derived a remote-sensing based ET dataset which they use to improve the spatial patterns of the MIKE-SHE national model of Denmark (DK model). These improvements are achieved by adjusting the parameterizations and input data of the existing DK model.They conclude, that spatial patterns of remote sensing data are a valuable information to inform hydrologic models about spatial patterns, whereas these models are usually calibrated on integral signals, such as streamflow.



The topic fits the scope of HESS and is of scientific interest. The chosen methods seems to be appropriate but some clarification in the methods section is still missing. The authors introduce novelty to the field of applying remotely sensed data for hydrological modeling by consideration of bias insensitive pattern matching techniques. The manuscript lacks, here and there, the soundness of the applied evaluations using scientific methods. A lot of evaluation of the spatial patterns in figures 5 to 7 is done on visual basis without proper numerical/scientific quantification. Some features, e.g., the often mentioned "clear" distinction between model region 5 and 6 are hard to observe for the reader if even existing. Further, the adopted DK model is never evaluated regarding streamflow or groundwater levels which is the main purpose/application of this model. Another criticism is the absence of a proper discussion of the findings of this study, there is little referencing to any other study such as Mu et al. (2007, 2011) for remotely sensing based ET estimates.

With exception of section 2.2 the manuscript is well written and good to understand. It could improve by better organization of the sections. I would swap sections 2.3 and 2.4 because sections 2.2 and 2.4 belong together in my opinion. Further, I suggest to fully reorganize and rewrite section 2.2 since it is hard to follow and the storyline is missing in there.

Concluding, I suggest to accept the manuscript for publication in HESS after major revision.

## 2 Specific comments

Introduction:

The introduction is well written and gives a appropriate overview on the topic and shows the novelty of this study compared to existing research.

Methods:

In general section 2.2 should be reorganized and rewritten because it is difficult to follow (I am missing the storyline here) and hard to understand what all the variables and equations are needed for. I think a major thing missing here is the presentation of the TSEB equation to assess which variables are needed in order to estimate ET. This will make clearer why you estimate LAI and vegetation height among others.

- please include TSEB equations
- P4L24: Is the LAI estimate sensitive to its source satellite? So is there any difference in LAI data originating from TERRA compared to AQUA?
- P4L30: What does BRDF mean?
- Eq. 1: Please state the wavelengths for $B_1$ and $B_2$
- clarify for what purpose LAI, albedo, VH and others are needed, I think the TSEB equation will help a lot for that
- Eq. 6: please explain $LAI_{MaxClass}$
- Fig. 2: probably add the growing phase as a gray box, I think the red line should be dotted outside the growing phase as it was estimated with Eq. 5, merge both legends to one, caption: probably show pixel in map (Fig. 1) - row 100 and column 84 definitely means nothing to anybody, add: LAI corresponds to the left ordinate and Fg to the right one.
- Are the data interpolated around the braking point or is a jump appearing in Fg? How reasonable is that?



- P6L5-9: How is that approach justified? Do you have any evidence with observations or references in literature?
- Eq. 8: I think you mean $EF = \frac{ET}{R_n}$
- P6L30-32: I do not understand this sentence
- Eq. 9 and 10: Why was the original RD approach based on LAI adopted to NDVI. LAI is available as seen on previous page. Could you please elaborate a bit on that?
- Eq. 9: Please explain $NDVI_i$ the same as $NDVI_{max}$
- P7L7: LAI in meters?
- How is $RD_{max}$ estimated?
- I don't understand what "matching the original DK model" for RD and KC means. I thought the aim is to make them variable. How did you achieve to make them matching, by parameter calibration? Please elaborate a bit more on that.
- P8L3-6: At P6L14-16: you state an actual value comparison is not anticipated. Here you are calibrating your TSEB model with eddy covariance data. Why? Please elaborate more on that.
- P8L1-2: Wouldn't a variance based sensitivity method better fit the purpose of identifying the parameters which have to be used for model calibration instead of the derivative based approach applied herein? Probably provide some details about the chosen sensitivity approach.

Results and discussion

- P9L5,L8: Please make a distinction between the terms parameter and variable, the reader gets confused otherwise.
- P9L8/Fig 3: better: TempA = $T_a$
- Fig. 3: $LAI_{max}^{Agri}$, $LAI_{max}^{Forest}$, $LAI_{max}^{Meadow}$ do not appear.
- P9L14 & P8L5-6: Why did you select only those 4 parameters out of 10. For PT the others seems to be more sensitive then the forest PT, for example.
- Please justify the assumption to add the residual energy to LE. I only know approaches using corrections based on the Bowen ratio or adding the residual energy to SH.
- Fig. 4: Thanks for including error bars to the plot. I think it is misleading showing only the error bars of the observation. Could you also show error bars on the simulation, e.g., emerging from different parameter sets?
- P9L20: Could you please mentioned the spatial resolutions of EC and RS data?
- The results section is missing in general a discussion with other studies. E.g., estimating ET from MODIS data comparing to Mu et al. (2007, 2011).
- P10L14-15: Is it reasonable to observe lower ET for forest areas? Wouldn't canopy interception increase ET only after precipitation events?
- P10L17: are causing differences in area 2 in the model domain
- P10L19, Fig 6, P11L5, and others: I am very sorry but I cannot observe the pronounced difference between zone 5 and 6. Could you provide some more information on that, e.g., zoomed plot numerical analysis? At the provided plots I do not see this features.



- P10L10: reformulate: extracted
- P10L24: ... does not necessarily lead to reasonable ET ...
- Fig. 9 and P11L9-15: Could you provide numerical evidence to the explanatory variables of the spatial patterns of ET. I can see the E-W gradient in clay content and ET but the others are not observable. Consider rewriting or deleting some of your conclusions since they are not supported by your data. Possibilities to get evidence: scatterplots or SPEARMAN rank correlations.
- I miss the comparison of the model performance in streamflow and groundwater table between the original and modified DK model. I understand that the spatial representativeness of the modified DK has improved compared to the original one. But shouldn't be made sure that the water balance is still sufficiently represented by assessing the streamflow and groundwater tables since that is the major purpose of the model? Therefore, the model performance shouldn't deteriorate significantly if evaluated with those variables.

## 3 Technical corrections

- Fig. 1: excluded in figure) - parentheses missing, consider using different symbols for Agri and Meadow because they are hard to distinguish.
- P2L10: rational behind developing
- P2L21-25: because you do not provide exhaustive list of references for each application example i suggest to use 'e.g.,' in front of the references
- P3L7: Figure 1 presents the herein used study domain.

- P4L27: I would put the LAI sentence to the previous paragraph and start the new paragraph with: “The study focuses”
- P5L6: delete successfully after Boegh et al.
- P5L7: this study instead of the study - you should check that in the entire manuscript
- P5L7: similar approach was applied where ... - please delete “was applied” later in the sentence
- P5LL25:please do not introduce abbreviation like 10U which are never used in the manuscript
- P6L5: To identify the different periods, first, the dates ...
- Is $LAI_{i.max}$ the same as $LAI_{Max}$ in Eq. 4 and Eq. 9? check consistency
- P6L6: breakpoint Fig. 2 not figure 3
- P6L6: better: breakpoint Fig. 2, i.e., the onset of the growing season
- P6L8: Eq. 6 instead of 5
- you are switching from Eq. to equation and Fig. to figure in the entire manuscript check consistency
- probably check for figure and equation referencing in the entire manuscript
- Eq. 7 & 8: netRad and net radiation - consistency
- I would suggest to use formula symbols like $R_n$ instead of words like netRad
- P6L20: The resulting maps ...



- P6L21: in just climatological maps
- P6L26: latent heat (LE) or **evapotranspiration** measurements **are**
- P6L29: which is usual instead of not unusual
- P7L7: RDi = $RD_i$
- P10L11: Fig. 5 instead of Fig. 56
- P10L11: pattern identifie**d** the **T**SEB
- P12L22: the meso.. instead of The meso..

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