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Interactive comment on "Calibration of a parsimonious distributed ecohydrological daily model in a data scarce basin using exclusively the spatio-temporal variation of NDVI" *by* Guiomar Ruiz-Pérez et al.

Guiomar Ruiz-Pérez et al.

guiomar.ruiz.perez@slu.se

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First of all, we would want to thank the referees for the suggestions and comments. They will be useful to improve the manuscript. To better address their concerns, our reply has been divided into two sections, concerning: (1) research comments and (2) writing style comments. We are aware that some changes are required. However, both referees highlighted the good research quality of the manuscript and they found it relevant. It is always a pleasure to hear that your work is receiving attention and interest and is providing new insights. The revised version of the manuscript will be

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significantly improved incorporating reviewer's suggestions. In the following, we have addressed each question formulated by the referees.

Research comments

1- Are the model parameters different from cell to cell? If yes, which parameters are identical and which parameters are different?

To answer this question is important to better understand the concept of split-structure for the effective parameter value at each cell. This calibration strategy consists on an application of a scalar multiplier to each prior parameter field (specified from data describing watershed characteristics: soils, vegetation, topography, land use, etc.) and to estimate a "best" value for this multiplier via calibration. This so-called "multiplier" approach makes the assumption that the prior parameter field properly describes the spatial pattern of a specific parameter (the pattern of relative magnitudes from cell to cell), but that the magnitudes of all the parameter values must be adjusted to achieve a better simulation of the model response. Hence, the effective parameter at each cell (i.e. the parameter value used when running the model) is compounded by two parts: (1) a common correction factor for each type of parameter that takes into account the model, information and input errors and the temporal and spatial scale effects; and (2) the a priori estimated parameter value at each cell. Hence, for a given parameter, the a priori and effective values are different from cell to cell while the correction factor is common for all cells (and different from map to map). The estimated parameter values were extracted from the field work done and presented in the doctoral thesis by Franz (2007) and following the recommendations provided by the TETIS model's support team. Two of the authors are actually active members of this team and we also used our own experience.

2- I did not understand how the model calculate the LAI which then is used to calculate the transpiration?

The LAI is calculated by the dynamic vegetation sub-model called LUE-Model. The

LUE-Model computes the leaf biomass (Bl) according to the following equation: (dB_l)/dt=(LUE*ε*PAR*fPAR-Re)*φ_l (B_l)-k_l*B_l

where LUE is the Ligth Use Efficiency, ε takes into account the reduction in LUE due to stress sources, Re is the respiration, ÏŢl(Bl) is the fractional leaf allocation and kl is the leaf natural decay factor to reproduce the senescence. Once Bl is computed it can be transformed into LAI by using the specific leaf area (SLA) and the vegetation fractional cover (fc) according to the next equation: LAI=B_l*SLA*f_c In the current version of the manuscript only the references about this model were mentioned and they should be specified. In this way, readers will only have to check the references if they are interested in specific details. These two equations together with the explanation will be provided in the next version of the manuscript. More detailed description can be found in Pasquato et al. (2015) and Ruiz-Pérez et al. (2016) (references embedded in the manuscript).

3- Maybe I missed, but what is the resolution of the implemented model?

You did not miss, we forgot to give that information. The temporal resolution is already specified and it is daily while the spatial resolution was 90X90 meters. It will be included in following versions of the manuscript.

4- How did the manual calibration help to ïňĄnd the best parameters? How the parameters' ranges have been constrained? In table 1, LUE tree and shrub is out of speciïňĄed range (Shrub is misspelled).

In this case, the manual calibration was considered mandatory as long as the model had never been used at catchment scale and, therefore, we had not clues about its suitability. Although non-statistical indicators were reported, the manual calibration helped to find the best parameters and constrain the searching boundaries in this following three senses: The best set of parameters obtained after the manual calibration was used as seed for the automatic calibration. We think this fact reduced the computational time required by the automatic calibration as long as this starting point or seed

is supposed to be closer to the best global solution than a random starting point. We were allowed to double-ckeck the values of the parameters after the manual calibration with those ones recommended in literature. In this way, we assured that the searching boundaries to be used during the automatic calibration process were consistent and wide enough. The manual calibration pointed out that wider ranges were not required and, in this sense, it constrained this searching boundaries. A manual calibration always gives clues about the potential inter-relationships between parameters. These clues can be used to guide the automatic calibration process (this research was not the case) and to be critic with the results obtained after the automatic calibration (it was the case here) since a sense of relative values was provided by the manual calibration. In that sense, the manual calibration can be extremely helpful to find the best and with physical consistency parameters. Finally, thanks for the observation about Table three. The boundary for all three cases was 1.12 instead of 1.2 and 'Srhub' will be corrected in the whole table.

5- A clearer explanation regarding EOFi would be appreciated. What does different i exactly mean?

If we apply the EOF decomposition (also called Principal Component Analysis) to a simple matrix, the EOFi is the i eigenvector. We always assume that the eigenvectors are ordered according to their corresponding eigenvalues (i.e. the amount of variance explained by them). Hence, EOF1 is the first eigenvector associated with the first eigenvalue and, therefore, which explained more amount of variance. Therefore, i means the position of the eigenvalue when is sorted according the explained variance. In our research, however, we wanted to apply this methodology to analyse spatio-temporal data. That's why the first step was to transform this data into a matrix. Basically, we construct a matrix (F) in which each column is the temporal variation of the data in a particular cell while each row represents the cells values during a particular time step. Once the matrix was constructed, we applied then the EOF analysis as usual. Therefore, we obtained the eigenvectors as usual. However, these eigenvectors

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can be regarded as maps by considering the same ordering criterion as used in F construction. In this way, the i-eigenvector becomes to the i-main/principal pattern/map. Hence, EOFi is the principal pattern associated with the i eigenvalue. Having reviewed the current manuscript, we found inconsistencies in line 5 and equation 5. We should have kept the same sub-index i instead of j. Otherwise, it might be confusing. We will improve this section and we will check the mathematical consistency within the equations. This concepts will be clarified in the revised version of the manuscript.

6- How would be the model performance with and without calibration on observed satellite data? Any gains or losses there? This would be great to be addressed.

We completely agree with your suggestions. In fact, we are working on it in new ongoing projects. In this new applications, we want to use different sources of information (field observations, remote sensing data, etc.) with different resolutions (point measurements, spatio-temporal data, etc.) in order to determine whether models performance improve. However, the study area of this manuscript was discarded for this analysis because this Kenyan catchment can be considered as scarce-data catchment. In fact, the available data is really poor and for this reason, it was precisely selected for this experiment. We wanted to face the issue of no having available observations. The calibration was completely 'blind' in terms of observed discharge, i.e. observed discharge was not even known at the beginning of this research. In this way, we assured that the calibration relied only on the satellite data. The main reason to do so was because we did not want to analyse the potential performance improvement by including satellite data, but how well we can calibrate a model by using only satellite data when this data is used properly. This latter goal builds the main theme of this research. Anyway, as mentioned, we also are interested in your suggestion but we would recommend to achieve this goal in study areas with good quality of field data. Hopefully, we can discuss in-depth this topic in following applications.

7- I am not convinced that what the authors are showing is only taking into account the remote sensing data. Did the authors look into the seasonality or the recession of

the hydrograph and adjust the range accordingly based on some expert guess? If yes, what is the effect of those assumptions or limitations? In a nutshell I would like to see "how exclusive the model result is regarding NDVI".

As said in the previous question, the calibration of the model was blind in terms of observed discharge. We did not use the observed discharge in any way. Neither the observed discharge per se nor its seasonality and/or other statistical metric. Therefore, we did not look into the seasonality of the recession of the hydrograph, neither adjust the range accordingly based on some expert guess. However, the TETIS-VEG model is process based. It is not a black box and it was driven by precipitation and temperature records measured at field. The estimated value at each cell was done by using data describing watershed characteristics (soils, vegetation, topography, land use, etc.). Moreover, the proposed calibration process relied on the satellite NDVI main patterns. As said in page 15 lines 1 to 3, the temporal variation of the EOF1 (which explained more than 60% and which dominated the calibration process) was related to the two usual rainy seasons of the study area. The NDVI contains information about seasonality by itself. By using the proposed conceptual model, such information is transferred to all hydrological processes involved in the water cycle (incl. discharge at the outlet point). Of course, some characteristics as runoff propagation parameters cannot be assessed using satellite NDVI. However, they are not influent in this case study since the model was run at daily time step.

Writing style comments

Since these comments are very similar, we consider more fruitful to address them with a common response. These are the comments regarding to language issues and style: This draft paper has major language problem. It is recommended that the paper should be edited by professional language editor before the last edition. I highly recommend the authors to make sure that the sentences are accurate, quantitative and fluent. As an example, in the abstract I can see that the authors wrote "extraordinary amount of information". What does it mean? They also mentioned "scarce data dry region"; do

they mean data-scarce dry regions? For example on page 4 line 2 the authors stated that “but it was complete enough for our purpose”. What is complete enough and what is the purpose? Is it really necessary to write this sentence? There are many similar cases across the manuscript. I encourage the authors to show the added value of the manuscript clearly and in precise manner. At this moment the manuscript is a mix of methods, literature review and theories. The clarification on model structure, model inputs, model outputs, and the ranges of the parameters would be highly appreciated. As suggested by the reviewers, the manuscript will be improved either by a professional language editor or by taking advantage of one of the co-authors on board who is native English speaker Additionally, we will give a thorough editorial check in order to meet the requested requirements. We will avoid ‘empty’ sentences as those ones mentioned in the second bullet. We will ensure that the sentences are accurate, quantitative and fluent. To accomplish the last bullet, we will improve some sections and some changes in the document organization are likely to happen.

Note: A supplementary file is attached with all this content and proper equations

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-573/hess-2016-573-AC1-supplement.pdf

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