Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-16-RC2, 2017

Hydrology and Earth System Sciences
Discussions
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Interactive comment

Interactive comment on "Calibration of a large-scale hydrological model using satellite-based soil moisture and evapotranspiration products" *by* Patricia Lopez Lopez et al.

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Received and published: 25 February 2017

This paper deals with a classical calibration study of the large-scale hydrological model PCR-GLOBWB using satellite-based products of evapotranspiration and soil moisture by taking the Moroccan Oum Er Rbia basin as an example. This topic fits very well to the scope of this journal. However, at times the paper is difficult to comprehend and dull to read. Especially the presentation of the calibration results gave me a hard time. Also, the structuring of the results section needs to be improved (e.g. introduction of subchapters). The revised version should also be checked by a native speaker.

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Discussion paper

Notwithstanding its formal deficiencies, this paper gives some valuable information on the calibration of distributed models in data poor regions and the usage of satellite-based data products. Therefore, I believe the paper deserves to be published after addressing the issues with regards to contents and presentation as listed in detail in the following.

General comments:

The motivation for choosing the study area is too weak. Basically the research presented in this study could be accomplished in any catchment. For instance, you could mention the specific challenges for the calibration of hydrological models in such environments.

The introduction is repetitive and too long. Please rewrite in a more focussed way and describe more clearly the structure of the paper.

A justification for using 6 performance metrics for the precipitation evaluation is missing. Since only the performance metrics NSE and KGE are used for the model validation analysis, I suggest to limit the precipitation data evaluation also to these metrics.

The presentation of the results needs to be improved. It is very difficult to keep the attention to the text, because the text is difficult to comprehend and the results are merely listed. Also a critical in-depth discussion of the results is largely missing.

Specific comments:

P3L18: GLEAM is a comprehensive model for the estimation of terrestrial evaporation and root-zone soil moisture from satellite data. Please clarify.

P7L3-20: This section is copious and repetitive. Please rewrite in a more clear and concise way.

P7L32-P8L6: This section should be placed in the results section. Please indicate the temporal resolution of the data from the rainfall gauging stations. In addition it is not clear to me, why you need to six performance metrics for the precipitation validation. Also, just listing the values of the metrics is not sufficient.

P9L3: Why did you use the first three layers? Given the extremely low penetration depth of the C-band data used for the ESA CCI SM product, you should only compare to the first layer. The depth of this layer needs to match the penetration depth of the C-band data, i.e. 2 cm.

P9L5-8: In my opinion this procedure leads to an untrustworthy and unsound comparison of simulated and observed soil moisture. A direct comparison of model results and observed data is a prerequisite for an unbiased and unadorned evaluation of the simulation results.

P9L15-34: This section is copious and repetitive. Please rewrite in a more clear and concise way.

P10L2: Why did you choose KGE for this analysis?

P10L4: In my view, it does not make sense to recalibrate for each precipitation data set. I would be more sensible to use the data that corresponds best with the rainfall gauging station data.

P10L5: I guess the different Ksat-values correspond to the soil layers S1-3. What about the Ksat-value for S4?

P10L6: Reference potential ET is not a parameter. Since it varies in time, it is variable.

P12L8: Delete “nearly”

P12L8: Due its complexity, Figure 5 is difficult to comprehend. In particular, the meaning of the dots in the scatterplots stays unclear with regard to what are actually representing (i.e. why is there more than one dot per variant). Since only three different values of the prefactors are considered no continuous scale should be used for the x-axis (otherwise the reader gets puzzled why the dots are not spreading).

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Discussion paper

P12L10: Please explain in more detail, why these prefactors should be well defined.

P12L16: “are considered”

P12L17: Please explain in more detail, why these prefactors should be well defined.

P12L23-31: This section is incomprehensible. Please rewrite.

P12L34: “scatterplots”

P13L3: The scatterplots of Figures 6 and 7 are quite repetitive. I would be enough to present only the NSE and KGE values of all variants and some selected scatterplots in case it is helpful for the discussion of the results.

P13L6-7: This is very obvious and provokes the question why you are using the poorer precipitation data sets for the model calibration analysis at all.

P13L26: You should first introduce the motivation for presenting these figures.

P13L35: I would rather like to see the unscaled results, because this procedure embellishes the model results.

P14L9-10: You could simply check using the measured precipitation data.

P14L30: See comment P13L6-7

P15L8-9: So wouldn’t it be more sensible to use multi-objective function calibration procedure?

P15L10: The discussion chapter is largely a summary of the results (first half) and an outlook, which should be placed in the conclusion chapter. In order to reduce redundancy, I suggest skipping this chapter and moving parts to the results and conclusion chapters.

P16L12: Another promising way towards a more in-depth validation of distributed models are the empirical orthogonal functions analysis and the wavelet coherence analysis (e.g. Fang et al., 2015).

Figures:

Figure 3: The precipitation field seems to be shifted (the highest precipitation amounts are expected in the Atlas mountain ranges, see e.g. Chehbouni et al., 2008). You should add dots in the lower graphic.

Figure 5: The scatterplots are too crowed and difficult to read.

Figures 6-10: Always indicate that you are showing monthly values, e.g. "Monthly observed discharge".

Additional literature

Chehbouni, A., Escadafal, R., Boulet, G., Duchemin, B., Simonneaux, V., Dedieu, G., Mougenot, B., Khabba, S., Kharrou, M.H., Merlin, O., Chaponnière, A., Ezzahar, J., Erraki, S., Hoedjes, J., Hadria, R., Abourida, H., Cheggour, A., Raibi, F., Boudhar, A., Hanich, L.,Guemouria, N., chehbouni, Ah., Olioso, A., Jacob, F. and Sobrino, J. (2008): An integrated modelling and remote sensing approach for hydrological study in semi-arid regions: the SUDMED Program. International Journal of Remote Sensing, 29: 5161-5181.

Fang, Z., H.R. Bogena, S. Kollet, J. Koch and H. Vereecken (2015): Spatio-temporal validation of long-term 3D hydrological simulations of a forested catchment using empirical orthogonal functions and wavelet coherence analysis. J. Hydrol. 529: 1754-1767, doi:10.1016/j.jhydrol.2015.08.011.

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