Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-16-RC1, 2017

Hydrology and Earth System Sciences
Discussions

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

R. C. Nijzink (Referee)

r.c.nijzink@tudelft.nl

Received and published: 24 January 2017

The study of Lopez Lopez et al. deals with a modelling exercise carried out for the Oum Er Rbia basin. The hydrological model PCR-GLOBWB was applied for this basin and optimized with GLEAM evaporation and ESA CCI surface soil moisture, in different calibration scenarios. In addition, three different precipitation products were used as forcing data. The authors show that a step-wise calibration with GLEAM and ESA CCI and forcing data from EI and MSWEP provides improvements in model performances. The added value of this research is very clear to me. I fully agree with the authors that

applications of (remotely sensed) data in hydrological modelling are mostly limited to one extra model variable to calibrate on, whereas combinations of data products are explored to a much lesser extent. In addition, the paper is well written and concise. Nevertheless, I'd like to raise several remarks in order to help the authors to improve on their manuscript.

General comments

My most important point considers the calibration. It consists of 81 runs with three different values for the calibrated prefactors. I fully understand that running a distributed model has a high computation cost, but this number of runs seems rather low to me. There is a big risk of undersampling, leading to results that can just be a mere coincidence. For example, the most optimal value of f_w may actually be 0.889, whereas only 0.75, 1 and 1.25 are explored in the study. Once again, I understand the burden of computational efforts, but at least the authors may want to reflect on this limitation in their discussion. In addition, it is mentioned that, except for these four prefactors, the remaining parameters were kept fixed. How many parameters are kept fixed and to what extend is the model already directed towards a certain solution by the choice of fixing certain parameters? For example, according to Figure 2 PCRGLOB-WB uses an interception routine. If the maximum interception capacity is kept fixed, it will probably influence the results for GLEAM versus the modelled evaporation.

In addition to this, the step-wise calibration consists out of first calibrating on evaporation with GLEAM, and, in a second step, on soil moisture. I just wonder how much influence this order in calibration influences the results, especially as soil moisture strongly influences evaporation. Did you consider a step-wise calibration with first ESA CCI soil moisture and then GLEAM in a second step?

I also wonder what the reasoning is behind the choice to compare the ESA CCI surface soil moisture with the soil moisture of the first three soil layers of PCR-GLOBWB. As mentioned by the authors (page 8, line 30) the ESA CCI soil moisture only represents

the first 0.5-2cm, so wouldn't it make more sense to just compare with only the very first soil layer (first 5cm according to page 5, line 14) in PCR-GLOBWB? In this way, all parameters affecting the soil moisture in all the three layers will react, which can also be noted from the results for f_k , but one could wonder whether this is for the right reason.

Often, a comparison is made between the reference scenario S0 and the scenarios S2-S4. Nevertheless, S0 is merely an uncalibrated model and especially for Ait Ouchene (Figure 6), the model performances are rather poor. Therefore, not much is needed to achieve improvements in this case. Isn't it much more interesting to focus more on comparing S1 with S2-S4? In other words, how close can we get to a calibration on streamflow with help of GLEAM and ESA CCI? It would be interesting to see if differences occur in Figures 8-10 for S1 and S2-4. Ideally, there would be no difference, but I expect that this will not be the case.

Detailed comments

P8.L2-3. I don't know if these specific stations were used for MSWEP, but as MSWEP used station data as input (also remarked by the authors on P7.L19-20), isn't it logical that MSWEP provided a better fit to the station data?

P10.L15-17. Why model at a daily basis and only compare on monthly values? What is the temporal resolution of the data (discharge, GLEAM and ESA CCI)?

P12.L10. I can see that f_w shows a clear pattern, but I don't see this clearly for f_e.

P13.L16-17. These numbers refer to the WFDEI-case

P14.L9-11. It may as well be model structural deficiencies as wrong parameterizations. It is a bit easy to blame the input data directly, especially as it happens for two out of three input products. It must noted as well that even though EI has the peaks in 2002 right, it also underestimates the peaks in 1996 and 1997.

Throughout the manuscript, the terms KGE, NSE etc. are used and sometimes refer to

a case with evaporation and sometimes to cases with soil moisture or discharge. For clarity, it might be good to add a subscript (e.g. KGE_E, KGE_{SM} etc.).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-16, 2017.