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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Improved large-scale hydrological modelling through the assimilation of streamflow and downscaled satellite soil moisture observations" *by* P. Lopez Lopez et al.

**Anonymous Referee #2**

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## OVERVIEW

The manuscript investigates the potential of large scale hydrological modelling (PCR-GLOBWB) for discharge simulation in the Murrumbidgee basin in Australia. Specifically, the assimilation of downscaled satellite soil moisture from AMSR-E and discharge observations is tested for improving flood simulation. Meteorological forcings are obtained from 1) downscaled global data (WFDEI), and 2) local high resolution gauging stations. A locally calibrated hydrological model (OSWS) is used as benchmark to test



if the assimilation of soil moisture and discharge observation is able to improve the performance of the global model as close as possible to the local one.

## GENERAL COMMENTS

The manuscript is well written and clear. Different analyses with two different hydrological models are performed for a well-equipped basin, thus providing robust results (but to be clarified, see below). The topic is surely of interest for HESS readership as both global-scale hydrological modelling and data assimilation of satellite soil moisture and discharge are relevant and timely research activities. Therefore, the paper has large potential to be published on HESS, but only after addressing the major comments I raised below.

1) MAJOR: Two different models (global and local) are compared in the paper. However, the differences between the two models (e.g., infiltration equation, lateral flow component ...) are not well specified and, hence, not clear. Theoretically, global models might be more complex and with an improved structure with respect to local models that frequently are quite simple (in the hydrological community). The real difference shown in the paper is not between a global and a local model, but between an uncalibrated (global) and a calibrated (local) model. The differences are in the parameter calibration, and NOT in the models. Therefore, the interpretation of the results is for me misleading.

Why not performing calibration of the global model similarly to the local model? Why not performing the assimilation of soil moisture and discharge into the local model? These points should be discussed and clarified. I guess that it needs additional work, but results will be more relevant (at least for me).

2) MAJOR: Linked to the comment 1, both models (also the local model) perform very

badly in figures 8 and 9. Looking in details at Figure 8, with local meteorological forcings, the open loop (and the assimilation of discharge) simulation does not produce discharge, much lower than the observations. The local model (OSWS) significantly overestimates discharge. Similar considerations can be made for the simulation with global forcings (even worse). I do not like to comment on model results, as it is expected that model fails to reproduce observations. However, in the example of Figure 8 the discrepancies are significant. In figure 9, for several cases the open loop simulations provide negative NS values. In these conditions, I expect that even the assimilation of perfect observations will be not able to improve the performance. More important, the assimilation in a model with a strong bias with respect to observations might have some issues in the specifications of the different comments (see comment 3).

3) MAJOR: As the authors know very well, the assimilation of any observation in a model has some issues. In data assimilation, the specification of modelling and observation errors, of the model structure (e.g. soil layer in which soil moisture data are assimilated), of the bias correction technique, of the spatial-temporal correlation of errors, etc., has a significant impact on final results (see *Massari et al., 2015* for a recent example). The authors mention these issues shortly in the discussion, but no analysis is made to address this issue. Can the authors add some more explanations on the choices made in the data assimilation experiment? Better, a sensitivity analysis on the selected choices is required. Otherwise, the obtained results might be only a random realization of an ensemble of results that might be very different depending on the subjective choices made in the assimilation experiment.

4) MODERATE: For me it is obvious that assimilating satellite soil moisture from AMSR-E will improve the agreement with AMSR-E observations, as shown in section 3.1 (it happens for any variable, model ...). In the basin, independent in situ observations should be available. Why not using these observations for a more robust assessment of the assimilation results in terms of soil moisture simulation? Can the authors add this analysis?



5) MODERATE: At page 10578 it reads that the use of finer spatial resolution satellite soil moisture products might be responsible of the good results obtained in the paper. However, the comparison with coarse resolution data is not made. Therefore, it can't be stated that finer resolution data provides improvements. It should be checked with specific analysis.

6) MODERATE: In the discussion and in the conclusions I believe that the authors are over optimistic in the evaluation of the obtained results. With respect to the open loop, an improvement is obtained, and it is good. However, the performance in the open loop is too poor and it seems to me quite easy to obtain a better agreement after the assimilation. I suggest reformulating the text.

In the specific comments, I added some corrections and suggestions that should be implemented.

On this basis, I believe the paper deserves to be published only after a major revision.

**SPECIFIC COMMENTS (P: page, L: line or lines)**

P10562, L12: I believe that neglecting lateral fluxes in global hydrological models is the major issue that needs further investigations. Can the author comment on that?

P10563, L6: Several recent studies have been published on the assimilation of soil moisture data in hydrological model (*Massari et al., 2015*), even in Australia (*Lievens et al., 2015*; *Alvarez-Garreton et al., 2015*). I believe that these studies should be mentioned and commented here.

P10563, L9: The reference to *Dorigo et al., 2010* for the AMSR-E soil moisture product is not appropriate. I suggest using *Owe et al. (2001)*.

P10563, L28: Also *Aubert et al. (2003)* assimilated both discharge and soil moisture

observations, I suggest mentioning here.

P10567, L20-21: Why both air temperature and potential evapotranspiration are used? I believe that only one variable is required. Please specify.

P10569, L17: Here I see two issues. First, the soil layer depth of OSWS model is surely much larger than that of AMSR-E observations. Therefore, their direct comparison might be not appropriate. Second, the depth of the first layer of PCR-GLOBWB model (5 cm) is also larger than that of AMSR-E data (2 cm). More important, it should be clarified how the surface information is propagated with depth through the assimilation procedure. Indeed, it is found that this aspect has a significant impact on the results (*Brocca et al., 2012*; *Chen et al., 2011*).

P10571, L18: After linear rescaling, the H matrix is equal to identity matrix. I suggest mentioning here.

P10572, L10-17: It is not fully clear from the captions and the legend of figures 5-10 the different scenarios. The figures captions should be self-describing. It should be evident that the assimilation is only performed in the global model, and that the symbol "w" is referred to the local model (it is not specified in the captions). I suggest using symbols more close to the meaning, e.g. GLOBWB for the global model and OSWS for the local model. The captions should specify all the symbols. The labels should be larger to be read easily.

P10573, L25: What is 410057 gauging station? Is it in situ observed soil moisture? Please clarify.

P10575, L1-3: The performance scores were already defined in section 2.5, please avoid repetitions (also at P10576, L18-20).

P10577, L16: I would suggest showing the results at station 41001. I believe results will be more meaningful than those given in Figure 8. Please provide also the NS-values.

P10577, L28: "reasonably good streamflow predictions". As mentioned before, results



are quite poor looking at figures 8 and 9.

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