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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assimilation of SMOS brightness temperature into a large-scale distributed conceptual hydrological model" *by* Renaud Hostache et al.

Renaud Hostache et al.

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We would like first to thank Referee 2 for the careful reading of the paper and the relevant remarks and comments. In the remainder, Referee 2's remarks are written in normal font while **our answers are written in bold**.

The main objectives of this study is to compare a large-scale conceptual hydrometeorological model (SUPERFLEX) and a physically-based land surface models (CLM) in their ability to simulate SMOS-like brightness temperature (Tb) and soil moisture, and (ii) to evaluate the improvement in model predictions when assimilating SMOS Tb observations. It is well written and the abstract reflects the objectives and results well.

Results are supported by appropriate figures and tables, references (some could be updated). This study is very interesting and promising. However, the paper does not do it justice. I feel like it was written quickly and that the authors skimmed over some key explanations. A more in-depth explanation of the methodology and analysis of the results are necessary. They are some caveats and I am particularly concerned about the title that does not reflect the real purpose of this study (comparison the ability of SUPERFLEX and CLM to simulate Tb and soil moisture). To me this kind of comparison is a bit unfair from the beginning as you do not necessarily want a large-scale distributed conceptual hydrological model and a large scale physically based land surface model (CLM) for the same purpose (?) Not to mention the fact that SUPERFLEX is calibrated, what about CLM? If authors wish to pursue in this way, then I am missing a proper description of the CLM set up (not only referring to a previous study and mentioning a 'quasi-identical' set up). My recommendation is major review, please find below an attempt to help.

We thank Referee 2 for this analysis. A more detailed description of CLM will be provided in the updated version of the paper. SUPERFLEX is calibrated whereas CLM is not. Indeed, while a conceptual model such as SUPEFLEX requires a substantial calibration effort because its parameter values cannot be set a priori, CLM is not supposed to be calibrated as it is physically based and its parameters can be determined based on the basin's known characteristics. Moreover, one can argue that calibrating CLM using SMOS data would not be an easy task, especially because of a high number of parameters and a significant computational effort needed to achieve a robust calibration for a large basin such as the Murray Darling. We will clarify this in the article.

General major comments (additionally to what is mentioned above) -From the abstract I see scores and headlines but I have no clue where the study takes place, please introduce south eastern Australia from the beginning (maybe from the title, it has to change anyway to reflect the content of the work).

We thank Referee 2 for this relevant remark. We will edit the abstract and the title accordingly

-Also from the abstract it is surprising that ERA-Interim is still used rather than ERA5. The recent literature (2018, 2019) is already full of studies demonstrating the added value of ERA5 with respect to ERA-Interim. I assume that in the previous CLM study (Rains et al., 2017) ERA-Interim was used and that is the rational for keeping it. This should be stated somewhere and ERA5 mentioned, if not tested as I believe it will prove useful.

We agree that ERA 5 is a better dataset than ERA interim and is of great interest. However, as acknowledged by Referee 2, using ERA5 for this study would render any comparison with the study by Rains et al. (2017) rather meaningless. We consider this comparison to be one of the main objectives of this study. We will better explain why we used ERA-interim and introduce Era5 in the paper.

- work must be done on statistical scores to provide an indication of how significant they are, I suggest to add at least p-values to assess the significance of each datasets and a 95

We will carry out and present significance tests for the experiment based on SUPERFLEX before resubmitting the revised version of the manuscript.

-Figures and tables should be self explanatory (?) please expand captions, add units when necessary (Kelvin: : :), label each panels for sake of clarity and refer to the labelling in the captions (some figure are hardly visible).

We will clarify and improve some of the figures and captions accordingly.

Other comments - scores from the abstract should more detailed, are you talking about surface soil moisture? Root zone soil moisture?

The scores given in the abstract have been computed based on observed soil moisture for both the surface layer and the 30cm root zone soil layer. This will

be clarified in the revised version of the article.

- P.2, L.13, '[...] as uncertain forcing [...]' OK so justify the use of ERA-Interim over ERA5

We agree that ERA 5 is a better dataset than ERA interim and is of great interest. However, as acknowledged by Referee 2, using ERA5 for this study would render any comparison with the study by Rains et al. (2017 rather meaningless. We will better explain why we used Era-interim and introduce Era5 in the paper.

- P.2, L.27, surface soil moisture (SSM)

We will add Âń surface Âż before soil moisture accordingly.

- P.3, L.5, November 2019 ? Do you mean 2009 ?

This is right . Thanks for highlighting this mistake

- P.14, L.15, "[: : :] is impacting soil moisture variations more significantly [...]" what is the meaning of "significantly"?

Here we meant to say that evapotranspiration has a higher impact on soil moisture variations.

- Figure 2 must be improved, ground based measurement stations are barely visible, also is the main river represented the only one in Australia (this is not a paper quality figure).

Our assumption is that Referee 2 is referring to figure 1 rather than figure 2. The right panel of this figure will be enlarged to render the measurement stations more visible. We will improve this figure in general.

- section 2.2.1, if this work has been published elsewhere, maybe it can be put in an annexe / supplementary ?

To our knowledge this work has not been published elsewhere

- P.7, L.2, "[: : :] $0.25_matchingtheoneusedinERA - Interimdataset.''misleadingatyouputisat0.25_whileitsnativespatialresolutioniscloserto80km$

We thank Referee 2 for highlighting this. We will clarify this point in the revised version of the paper.

- I am missing somewhere a clear description of the 2 models set up

A description of CLM will be provided in the updated version of the paper.

- Section 2.2.3, please discuss further the possible impact on the 2 models comparison.

We will clarify in the paper that SUPERFLEX is calibrated and CLM not. We will also emphasise the fact that the calibration of Superflex using SMOS data gives it a higher chance to reach better performance levels. Moreover, we will argue that calibrating CLM using SMOS data would not be an easy task, especially due to the required computational demand for calibrating a very high number of parameters over a large basin such as the Murray Darling.

- Figure 3, units, significance, labels

This will be done

- Table 1, as it stands it is not very useful, expand the caption so readers may know what is it about, statistics between what and what? What are the units? Cal stands for calibration, Val stands for validation...(general major comment), use same number of digit..

We will improve the caption of this table.

- Figure 4, is left panel useful? Significance of the differences in figure 5?

Yes as the left panel proposes average values that are more integrative, we believe it is useful.

- P.15, L.6, "[: : :] time series [...]" a figure would prove useful

A figure with a time series would only show results for one station. We think that showing a more general analysis using Taylor diagrams is more relevant.

- Figure 6, same min/max for axis of left and right panels

Since the considered soil depths differ, the performance value ranges in different intervals. We do not see the benefit of "same min/max for axis of left and right panels" as this would render the figure less readable (i.e. the points will be closer to each other).

- P.17, L.9-12, please discuss the use of SMOS anomalies as it could be an explanation (?)

The fact that we assimilate SMOS anomalies helps reducing bias between simulations and observations during the assimilation. However, at that moment, we do not see why the assimilation of anomalies would explain the fact that ubRMSE and RMSE are not reduced.

- Figure 8, I do not understand bottom left panel, rainfall and obs? Also why the number of stations differs from a panel to another?

In the bottom left panel, the same colour scale is used to indicate rainfall amounts (map) and number of SMOS observations assimilated at each measurement station (colour of each points). This will be clarified in the new version of the paper. The number of stations differs from one panel to another as all stations do not measure soil moisture for all soil depths. The bottom panel indicates all measurement stations. The top panels show stations measuring SM in the 8 and 30 cm top soil layer. This will be clarified as well.

- Figure 9 is interesting!

We thank Referee 2 for this positive remark.

- Figure 10, not clear enough that 5 pairs of data are represented on the Taylor Dia-

gram, pleas improve the quality.

We will improve this figure

- Title presents 1 objective, the abstract 2 and the beginning of the conclusion 3, please be consistent.

We will improve this

- I am personally not a big fan of bullet points in a conclusion but I may be a personal statement.

We thank Referee 2 for this remark.

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