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

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We would like first to thank Referee 1 for the careful reading of the paper and the relevant remarks and comments. In the remainder, Referee 1's remarks are written in normal font while **our answers are written in bold**

GENERAL COMMENTS The paper is fairly well written and clear. The topic is of interest for the readership of Hydrology and Earth System Sciences journal. The use of satellite measurements for calibrating hydrological modelling is an important topic and the development of new approaches for addressing the task is surely of interest. Therefore, I believe the paper might deserve to be published but, in my opinion, after

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Discussion paper

the clarification of some important points.

We thank Referee 1 for this assessment.

I listed here the main comments also including their relevance: 1) MAJOR: The main assumption of the paper is that a distributed and conceptual hydrological model is more flexible, easier to use, less complex and faster than a land surface model. Therefore, if with a calibrated hydrological model we obtain similar performance as compared with a land surface model, we can build better modelling approaches. However, the assumptions above are not tested. Several questions come to my mind.

a. Is the hydrological model SUPERFLEX less complex than CLM? The structure of the two models should be shown and compared. I have the feeling that the model complexity is nearly the same.

A better description of CLM will be provided in the updated version of the paper. We argue that the SUPERFLEX model is less complex than CLM for the following reasons: 1. Whereas CLM solves dedicated equations for energy balance taking into account vegetation status, SUPERFLEX lumps energy balance contribution to water balance via a simpler potential evapotranspiration formula. 2. Superflex has a limited number of parameters compared to CLM 3. In this study, SUPERFLEX is structured with two soil layers (respectively 0-7cm and 7-21cm) whereas CLM considers five layers for the same soil depth range. Moreover, Superflex has a limited number of parameters compared to CLM. The Set up of CLM therefore requires much more input data (e.g. soil types and land use IIRC). These are the principal differences (but not the only ones) between the CLM and the SUPERFLEX set up that, in our opinion, enable us to argue that SUPERFLEX is less complex than CLM. This will be better explained in the revised version of our manuscript.

b. Is SUPERFLEX faster than CLM? Some information on the running time for the two modelling approaches should be given. **Yes, it is much faster. More information on**

this will be added in the revised version of the paper.

c. Why do we need a faster and less complex model? Which applications are addressed? For climate applications, we don't need faster simulations, right? I believe these questions should be addressed before the publication.

We argue that faster models are a pre-requisite for carrying simulations at large scale without implying a high computational demand. Moreover, we argue that faster models are required for near real-time forecasting applications and for long-term simulations. This will be highlighted in the new version of the article.

2) MAJOR: Is CLM calibrated on SMOS observations? As I believe it is not the case (from Rains et al., 2017), it is unexpected that CLM and SUPERFLEX perform similarly for the reproduction of SMOS brightness temperature (SUPERFLEX is calibrated on SMOS). Do the authors have an explanation? Similarly, results against in situ soil moisture observations are similar suggesting that CLM is performing good also without calibration. SUPERFLEX is calibrated with SMOS brightness temperature that in Australia is well correlated with in situ soil moisture (from previous studies), therefore I expect it works good against in situ soil moisture. On this basis, I believe CLM should be considered more reliable than SUPERFLEX. Can the authors comment on that?

SUPERFLEX is indeed calibrated using SMOS observations. Referee 1 is right; CLM is not calibrated using SMOS data. Indeed, while a conceptual model such as SUPEFLEX requires a 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 are derived from various input data describing the characteristics of the catchment. Moreover, one can argue that, because of a large number of parameters, calibrating CLM using SMOS data would not be an easy task especially due to the computational demand over a large basin such as the Murray Darling. The calibration of many parameters would lead to a widely reported equifinality issue. We fully agree that CLM performs satisfyingly even without

any calibration. However, we think Referee 1 slightly misunderstood the objective of our study: we want to evaluate if a simplistic conceptual model when calibrated with a freely and globally available data product such as SMOS Tb can reach the performance level of a physically based model. We believe that the conclusions of our paper show that this is actually true. However, we do not agree with the statement that CLM should be considered more reliable than SUPERFLEX as the performance level are rather similar for both models during the calibration and the validation period. The advantage of CLM is that it does not require any calibration, while the strength of SUPERFLEX is that it can reach the same level of performance when calibrated with a freely and globally available SMOS data product. In addition, it can reach this performance level with a comparatively lower computational demand. We will clarify this as requested by Referee 1.

3) MODERATE: The differences between the open loop and the analysis are very small. Are they significant? Some tests to assess the significance of the obtained results should be performed.

To answer this question, we will carry out and present significance tests before resubmitting the revised version of the manuscript.

4) MODERATE: Does the SUPERFLEX model include lateral flow? If not (as I believe), it should be clarified.

This is right; In the simplified version used in this experiments SUPERFLEX does not explicitly simulate lateral flows within the root zone soil layers. This will be clarified in the revised version of the paper.

I listed in the specific comments a number of corrections and changes that are needed. SPECIFIC COMMENTS (P: page, L: line or lines) P1, Abstract: In the abstract, I have found too many details on the methodology and just few lines for the results. E.g., the results for simulating actual evapotranspiration are not mentioned. Please revise the abstract.

We thank Referee 1 for this relevant remark. We will edit the abstract accordingly

P2, L5-10: In the introduction, the prediction of flood is mentioned but the modelling approach here developed is tested only in terms of soil moisture and evapotranspiration. Indeed, I expected to see also results in terms of river discharge simulations by reading the title (hydrological model). Anyhow, less emphasis should be given to flood forecasting in the introduction.

We thank Referee 1 for this relevant remark. We will edit the introduction accordingly

P2, L32: Acronyms should be defined, and references to modelling approaches should be given. Throughout the text, the acronyms should be defined.

We will define the acronyms.

P3, L5: It should be "2009" instead of "2019".

Thanks for pinpointing us to this mistake

P3, L8: Please rename the "land surface modelling" for retrieval of soil moisture from SMOS brightness temperature. It makes confusion with land surface model. I would rename in "radiative transfer modelling".

This will be done

P4, L1: "tailoring the structure" of? Please clarify.

We will edit the text so that it becomes clearer that the structure of the model (i.e. reservoirs. . .) can be tailored.

P5, Figure 1: Map of Australia should be smaller, and that of Murray-Darling bigger.

This will be done as requested by Referee 1

P5, L1: I understand the use of ERA-Interim for performing the simulation as in Rains et al. (2017); however, it would be highly interesting to test ERA5, the new ECMWF reanalysis.

We agree that this would be interesting in general. However, this goes beyond the scope of the paper as the new results would not allow for a meaningful comparison with the study by Rains et al (2017) which is one of the main objectives of this study. We are happy to consider this remark for further studies.

P7, L5: It shouldn't be "surface" runoff, but total runoff, right?

This will be clarified: In the sentence, "surface runoff" was actually related to "routing function" (that simulates surface runoff)

P7, L9: not bold for "URI".

Thanks, this will be corrected.

P12, L14-15: Is the gradient of performance of SUPERFLEX similar to CLM? Please comment on that.

The gradient of performance of SUPERFLEX that is visible in Fig.3 is not observed in CLM's run. As argued in the paper: Considering that in our set up, the representation of the evapotranspiration is rather simplistic as it is based on the Hamon formula, this could explain the comparatively poor performance of the model in the western part of the basin.

P13, L1: Use "target" instead of "0" to avoid misunderstanding with the axis origin.

This will be modified

P14, L5-7: Why is there a strong underestimation of standard deviation? Do the authors have some explanations?

Our interpretation is that the two models are unable to reproduce the variance

of SMOS observations mainly due to some limitations of the radiative transfer model. Indeed, even with completely dry or wet soils, the simulated Tb do not reach the extreme values observed by SMOS.

P16, L14: Why 25K2? Please add a reference or an explanation.

Here, we used the same value as in Rains et al. (2017) to keep the experiments comparable.

P16, L14: "average performance metric" as compared with? Please clarify.

The reference used for this comparison are the in situ soil moisture measurements. This will be further clarified in the revised version of the paper.

P19, L1-: This part should be moved to the method section.

This will be modified in the revised version of the paper

P20, L13-14: Are we sure that ERA-Interim rainfall has larger errors for larger rainfall events?

We cannot be sure that errors are larger for larger rainfall for the Murray Darling basin, but this is something that was often reported in the literature as for example in the study by Xu et al (2019). Xiaoyong Xu, Steven K. Frey, Alaba Boluwade, Andre R. Erler, Omar Khader, David R. Lapen, Edward Sudicky, Evaluation of variability among different precipitation products in the Northern Great Plains, Journal of Hydrology: Regional Studies, Volume 24, 2019.

P21, L9: This part should be moved to the method section.

This will be modified in the revised version of the paper

P23, L6-17: There is no need to summarize the study in the conclusions; I suggest shortening this part. **The conclusion part will be shortened accordingly**

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