Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-414-RC1, 2019

Hydrology and Earth System Sciences
Open Access
Discussions

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

Anonymous Referee #1

Received and published: 1 September 2019

OVERVIEW
The study investigates the use of a distributed conceptual hydrological model for simulating soil moisture (and evaporation) over large areas. Specifically, the model has been applied over the Murray-Darling basin in Australia and calibrated by using brightness temperature observations from SMOS. For that, the hydrological model (SUPERFLEX) is coupled with a radiative transfer model (CMEM). The model validation with in situ soil moisture (and evaporation) data has been carried out and compared with the

CLM-CMEM simulation performed in Rains et al. (2017).

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 the clarification of some important points.

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.

b. Is SUPERFLEX faster than CLM? Some information on the running time for the two modelling approaches should be given.

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.

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?

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.

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

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.

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.

P2, L32: Acronyms should be defined, and references to modelling approaches should

be given. Throughout the text, the acronyms should be defined.

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

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".

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

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

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.

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

P7, L9: not bold for "URI".

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

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

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

P16, L14: Why $25K^2$? Please add a reference or an explanation.

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

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

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

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

P23, L6-17: There is no need to summarize the study in the conclusions; I suggest shortening this part.

RECOMMENDATION

On this basis, I found the topic of the paper relevant, and I suggest a major revision before the paper can be published on Hydrology and Earth System Sciences.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-414, 2019.