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Hydrology and Earth System Sciences Discussions
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Interactive comment on "A multi-sourced assessment of the spatio-temporal dynamic of soil saturation in the MARINE flash flood model" *by* Judith Eeckman et al.

Anonymous Referee #1

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OVERVIEW

The study investigates different formulations of the MARINE flash flood model to assess their performance in reproducing spatial and temporal dynamic of soil moisture during flash flood events. Specifically, three different formulations are compared for 6 flash flood events in two basins. As benchmark, in situ, satellite, and modelled soil moisture observations (plus piezometric head) are used.

GENERAL COMMENTS

The paper is fairly well written and clear, but in my opinion several parts need to be improved and other parts corrected. The topic of the paper is interesting for the read-

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ership of HESS as the assessment of flash flood modelling tools through soil moisture observations is relevant to understand their reliability and robustness. Therefore, I believe the paper might deserve to be published after addressing the comments I have listed below, with the indication of their relevance.

1) MAJOR: The main result of the paper is that the new formulation of MARINE model (SSF-DWF) is performing better than the base model in terms of reproducing soil moisture dynamic (and river discharge). However, I am not sure that the paper clearly demonstrate this point. The main question is: are the better results related to the new model formulation or to its parameterization? I mean, if the base model is recalibrated I guess it will be able to reproduce soil moisture dynamic as well as the SSF-DWF model. Is that true? This point should be assessed carefully in the paper.

2) MAJOR: I am fully aware of the difficulties in obtaining river discharge observations during flash flood events. However, I believe that 3 flood events per catchment is not enough for a robust assessment. A larger number of events should be assessed, also by selecting smaller events (at least 10-15 events are needed). Otherwise the obtained statistics are too weak to provide robust results.

3) MODERATE: The assessment of deep layer soil moisture through groundwater observations is misleading. Due to the short time periods considered, and the long-term characteristic of groundwater response, the assessment does not provide meaningful results. If the authors do not extend the time period of the analysis, I would suggest to remove this part.

4) MAJOR: Model performance in reproducing river discharge is not good for several events (NSE<0). I am aware that the main objective is the model assessment through soil moisture observations, but if the model is not good in reproducing river discharge I would expect the same with soil moisture. Is it possible to recalibrate the model for such events (and better for a larger number of events) to assess if improving discharge simulations also a benefit in soil moisture reproduction is observed? Otherwise I am

not sure if the model is a suitable tool for simulating soil moisture and river discharge in the selected catchments.

5) MODERATE: The assessment in terms of soil moisture should be carried out only in terms of temporal dynamics. The assessment in terms of absolute values or in terms of range of values is meaningless as the different soil moisture observations have different representativeness in terms of spatial scale and soil depth. Sometimes in the paper it reads this kind of assessment that should be removed.

6) MODERATE: Related to the point above, I would strongly suggest to extend the analysis of spatial patterns. The model capability in reproducing spatial soil moisture patterns is largely unexplored in the scientific literature even though it is a highly relevant topic.

7) MODERATE: I have found the paper too long and difficult to follow in some parts. I would suggest reducing some parts and/or moving them to the appendix. For instance, the analysis of the spatial moments (Figures 9 and 10) does not add important findings to the paper and can be moved to the appendix (or removed). As always in scientific papers, it is better to show a more limited number of figures and tables but more focused to the main message the authors want to convey to the readership.

In the specific comments I have added several suggestions to improve the manuscript (in my opinion). Please address the comments carefully as several parts need to be corrected.

SPECIFIC COMMENT (L: line or lines)

L29-35: Several mechanisms of runoff generation do exist, such as infiltration excess, saturation excess, subsurface and deep groundwater flow, flow through macropores and preferential flow. The description in this paragraph is too simplistic and it should be improved.

L45-47: Several studies have demonstrated that local soil moisture measurements are

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representative of larger areas and hence they can be useful for initializing flood models (e.g., Brocca et al., 2009 JHE; Tramblay et al., 2010 JoH). Therefore, this part should be partly changed.

L49: I would change “continuous models” with “land surface and distributed hydrological models”.

L53-54: The sentence “However, remote sensors . . . of surfaces” is not clear and it should be revised. Note that different remote sensing techniques have been developed for obtaining soil moisture from satellite measurements.

L59: Note that also simplified approaches, e.g., Soil Water Index (used also in the paper), have been developed for obtaining root zone soil moisture. They should be mentioned here.

L64: I would change “tested” with “used”.

L70-80: Different models and products are mentioned here without references, they should be added.

L82: Change with “and the flood events considered for this study”.

L103: Change “volumic” with “volumetric” throughout the text.

L108: Change with “. . .are defined in the so-called deep water . . .”.

L116: A figure showing the three different schemes of the MARINE model would help the reader to understand the differences in the model representation.

L119: What does it mean that “the flows in deep layer remains a function of the water height”? Which water height? Is it the water depth in the soil layer? Please clarify.

L126: Change with “particularly prone to flash flood events”.

L143-144: An average soil depth of 27 cm and 37 cm for the two catchments seem very thin. Is that correct? What does this parameter represent? I believe that the actual soil depth is much larger.

L151: Change “pluviometers” with “raingauges”, krigging with one “g”.

L152: Change with “are available at hourly time step and 1 km resolution”.

L152: What are “critized observed discharges”?

L166-167” What are “meteorological antecedents”?

L167: Six events are not enough to guarantee robust results.

Table 2: The uncertainty values are quite strange, I would suggest removing them. It is very hard to provide good numbers as the uncertainty of different products is dependent on many factors.

CGLS SWI should be referred to Bauer. . . et al., 2018b).

ESA CCI is obtained from a number of active and passive sensors, please revise.

L218: ESA CCI should be referenced by Dorigo et al. (2017 RSE doi:10.1016/j.rse.2017.07.001).

L223-224: Figure 2 is not showing the fraction of missing values, please check and revise.

Table 4: Acronyms (BM, SSF, DWF) should be defined in the captions, or a list of acronyms should be provided.

Figure 5: For some events it is evident that poor model performances are due to wrong initialization. How is the model initialized? If the initial soil moisture condition is calibrated, does the model work correctly? This kind of assessment should be carried out. Again, otherwise the model is not a good tool for flash flood prediction (e.g., event March 2017, Orbieu).

Figure 6: Crowded figure, difficult to distinguish the different lines.

L393: Should be “March 2018”?

L405: “to BE consistent”

L441: Kendal correlations of 6.4 and 8.7? Maximum value should be 1.

L442-444: The sentence is not clear and it should be revised.

L478-504: There’s no need to repeat in the conclusions the analyses made, remove this part.

RECOMMENDATION

Based on the above comments, I suggest a major revision before the possible publication on Hydrology and Earth System Sciences.

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