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Discussions

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.

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Response to Reviewer N°1

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Interactive comment

Response to Reviewer N°1

November 3, 2020

We wish to thank the referee for his careful evaluation of the manuscript. Please find below the details responses to the comments (in bold). Some modifications of the manuscript are mentioned in italic.

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.

The model set-up used in the publication is based on the calibration of the base model. As the SSF model doesn't involved additional parameters, the same calibration is used for it. The SSF-DWF model involves another parameter, the deep layer depth

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therefore a new calibration is needed which was carried out from the calibration of the base model. Therefore the most tested and probably the most robust calibration is that of the base model. Another argument in favor of this assertion is that the base model has been thoroughly tested over the last ten years or so, and therefore carefully calibrated, including on the catchments studied in this article (Roux et al., 2011 ; Garambois et al., 2013 ; Garambois et al., 2015a ;Garambois et al., 2015b ;Douinot et al., 2018), whereas the SSF-DWF model has just been developed. Thus, if one parameterization is more questionable, it is more that of the SSF-DWF model than that of the base model. Moreover, Douinot et al. (2017) already showed that the base model is not able to reproduce correctly parts of the hydrological response, whatever the parametrization, that's why we argue that the better results are related to the new model formulation.

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.

Additional flood events would indeed by valuable for the study. However, the selection of the flood events is limited by the period of availability of the LDAS-Monde product at fine scale (2.5 kilometer resolution and 3 hours time step), i.e. since July 2017, as quoted in section 2.3.2 (Bonan et al., 2020). During the July 2017- April 2019 period, only 3 flood events occurred on each of the catchments. No additional flood events can be added for this period over the studied catchments as we couldn't make any comparising for them. Nevertheless, we think that the results concerning 6 events

on 2 different catchments can constitute a first interesting analysis because they all go in the same direction: the interest of the SSF-DWF functioning hypothesis for the representation of the saturation.
The following sentence is added in section 2.2.2: 'This period is chosen because it correponds to the period of availability of the LDAS-Monde product'

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.

The authors agree with this point. The use of the piezometric measurements had been attempted in order to add more validation data source. The reference to the ADES data, as well as the values in the tables and graphics are removed. The ADES network is only quoted in the last paragraph 4.2.3 Water content of the deep layer, as a possible opening for further works.

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.

Some performances are indeed poor but when the performance is poor for the base model, it is also poor for the SSF and SSF-DWF models, that's why we argue that it doesn't affect the results of the model inter-comparison. Indeed, several other calibration tests could have been carried out so as to improve the results of the hydrological models. However, the main purpose of this study focuses on a model inter-comparison to test several functioning hypothesis and assessed their respective potential for the simulation of soil moisture dynamic in the same context.

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.

The absolute values of the different soil moisture data are not directly compared. A special attention is paid not to do so when comparing different data source. When comparing the MARINE soil moisture outputs from the different models (BM, SSF or SSF-DWF), the soil moisture values are compared in terms of absolute values. In this case, the absolute values are comparables because they represent the same physical variable. Yet, the soil depth considered in each model is specified.

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

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moisture patterns is largely unexplored in the scientific literature even though it is a highly relevant topic.

We definitely agree that the comparison of spatial patterns is of high interest for the improvement of the understanding of hydrological phenomena. As the direct comparison of maps with different spatial resolution is not straighforward, we choose to consider instead the spatial moment values as a proxy for this analysis.

The paragraph 4.2 Comparison at the catchment scale is splitted into 2 paragraphs : 4.2.1 Catchment average behavior and 4.2.2 Spatial variability.

In section 4.2.2, the spatial variability of soil moisture fields, as well as the conclusion drawn from the spatial moments values are values are detailled.

The paragraph detailing the delta1 dynamics is reformulated as :

" *The general behavior of the $\delta 1$ spatial moment when computed on the SSD is that the $\delta 1$ increases when precipitation happens and then decreases at a variable rate. Indeed, as precipitation necessarily flows towards the outlet, $\delta 1$ values are bound to increase (i.e. the SSD fields get closer from the oultet after a precipitation event. The $\delta 1$ time series obtained with both the SSF and the SSF-DWF models are sig nificantly closer to 1 than the $\delta 1$ values obtained with the base model. This means that the SSD fields simulated with the base model are globally closer from the outlet than with the SSF and the SSF-DWF models, that is to say that the propagation of the water throught the drainage network in the upper soil layer is faster for the base model than for the SSF and the SSF-DWF models. The analysis of the $\delta 1$ time series allows to quantify the impact of the calibration of lateral transfers on the SSD distribution*. "

The paragraph detailing the $\delta 2$ dynamics is reformulated as :

" The $\delta 2$ values for the SSF and SSF-DWF models are globally closer to 1 than for the base model, that is to say that the SSD fields simulated with the SSF and SSF-DWF models are globally more uniform than for with the base model. This can be explained by the fact that the SSD is globally higher for the SSF and SSF-DWF models than for the base model (see figure ??), the difference between the SSD and saturation in the drainage network (i.e. 100 $\%$) is stronger for the base model than for the other two models. This leads to SSD fields more uniform for the SSF and SSF-DWF models than for the base model. This result is particularly observed for the Orbieu catchment. The analysis of the $\delta 2$ time series allows to quantify the differences between one the one side, base model, and on the other side the SSF and the SSF-DWF models. ”

The following synthetic sentence is added at the end of 4.2.2:
” *The analysis of the delta1 and delta2 spatial moments provides an inovative way to assess the spatial variability of the SSD fields. The reaction of the SSD fields to precipitation are quantified. The difference between the spatial repartition of the ouputs of the base model on the one side and the SSF and SSF-DWF models on the other side, is highlighted.* ”

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.

A particular care has been paid to improve the clarity of the paper. The technical changes pointed below, as well as spelling errors have been corrected. The clarity of the paper have been improved by selecting essential information. A table has been added to present the different data source for soil moisture. A scheme has been added to present the three different versions of the MARINE model (BM, SF and SSF-DWF). The litterature review of the various satellite data source, the detail of the different soil layers in the LDAS-Monde product have been moved to an appendix section. The introduction is completed and reformuled. The conclusion is shortened and reformuled. As explained above, the spatial moments have been chosen to perform the analysis of spatial patterns which is crucial for distributed hydrological modelling. That is why we keep this analysis in the article reformulating it to make it more relevant.

SPECIFIC COMMENT :

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.

A paragraph describing the processes is inserted (see below) and the paragraph on representation of the subsurface in the models is reformulated.

" Several mechanisms generate the partition between infiltration and surface runoff. Surface runoff can happen when rainfall intensity excess the maximum infiltration rate of the soil (infiltration excess), or when the precipitation volumes exceed the storage capacity of the soil (saturation excess). Then, the generation of surface runoff directly rely on the water content of the subsurface. Within the subsurface, both vertical infiltration flows and lateral transfers take place. These flow are controlled by the

physical characteristics of the porous media, such as its hydraulic conductivity or its capacity at saturation. In addition, preferential flows happen through macropores or fractured aquifers. "

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

The sentence is replaced by :

" Several studies have demonstrated that local soil moisture measurements are representative of relatively larger areas and hence they can be compared to spatially distributed simulation outputs around the point of measurement (Brocca et al., 2009; Tramblay et al., 2010) "

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

The edit has been done.

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.

The following sentence is added :

"Satellite imagery provides valuable spatially distributed data. 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.
The edit has been done.

L64: I would change "tested" with "used".
The edit has been done.

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

L82: Change with "and the flood events considered for this study".
The edit has been done.

L103: Change "volumic" with "volumetric" throughout the text.
The edit has been done.

L108: Change with ". . .are defined in the so-called deep water . . .".
The edit has been done.

L116: A figure showing the three different schemes of the MARINE model would help the reader to understand the differences in the model representation.
A figure is added to summarize the main state variables and flux in the soil for the three models.

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.

Reformulated as :

" In the SSF-DWF, the flows in the deep layer is defined as a function of the water height in the deep layer "

L126: Change with "particularly prone to flash flood events".

The edit has been done.

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.

The soil depth values are taken from the INRA databases. These databases have been established for agronomic uses and do not document deepâĂŘweathered rock horizons (i.e. pedologic horizons of type C and deeper) (Vannier et al., 2014). The average soil for France is about 1 m. These values look correct, considering the physiography of the two catchments.

L151: Change "pluviometers" with "raingauges", krigging with one "g".

The edit has been done.

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

The edit has been done.

L152: What are "critized observed discharges"?

" critized discharges" are defined as discharge measurements that have been cor-

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rected from known biases. In particular, in the case of flood events, high water heights are difficult to extrapolate from the rating curve. Direct measurements have then to be criticized and corrected according to humain expertise. This analysis is carried out by the forecasters of the French national flood forecasting services.

L166-167" What are "meteorological antecedents"?

The expression "meteorological antecedents" refers to the paper of Berthet et al.,2009 : " How crucial is it to account for the antecedent moisture conditions in flood forecasting? ". This work shows the impact of the hydro-meteorological situtation during the days before the flood. For clarity purpose, this word is removed from the text.

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

Additional flood events would indeed by valuable for the study. However, the selection of the flood events is limited by the period of availability of the LDAS-Monde product at fine scale (2.5 kilometer resolution and 3 hours time step), i.e. since July 2017, as quoted in section 2.3.2 (Bonan et al., 2020). During the July 2017- April 2019 period, only 3 flood events occured on each of the catchments. No addictional flood events can be added for this period over the studied catchments as we couldn't make any comparison for them. Nevertheless, we think that the results concerning 6 events on 2 different catchments can constitute a first interesting analysis because they all go in the same direction: the interest of the SSF-DWF functioning hypothesis for the representation of the saturation.

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.

The uncertainties values are taken from the respective paper for each product. But indeed, the meaning of the uncertainty value might differ between the studies. The uncertainty column is then removed from the table.

CGLS SWI should be referred to Bauer. . . et al., 2018b). ESA CCI is obtained from a number of active and passive sensors, please revise.
The reference has been corrected.

L218: ESA CCI should be referenced by Dorigo et al. doi:10.1016/j.rse.2017.07.001).
The reference has been corrected.

L223-224: Figure 2 is not showing the fraction of missing values, please check and revise.
The graphical appearance of this figure is enhanced. The figure is sent to the appendix section .

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

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).

All the models (base, SSF, SSF-DWF) are initialized with the spatial soil saturation outputs from from Météo-France's SIM (Safran-Isba-Modcou, Habets et al., 2008) operational chain, the initial soil moisture is not calibrated. The first bump in the hydrograph is due to the hydrological functioning as with the same initialization the SSF model doesn't present the same behaviour. This is therefore an integral part of what this article seeks to analyze. Indeed, as stated above, the main purpose of this study is a model intercomparison to test several functioning hypothesis in the same context.

Figure 6: Crowded figure, difficult to distinguish the different lines.
Different shades of grey are used for the SMOSMANIA data lines.

L393: Should be "March 2018"?
Yes,corrected.

L405: "to BE consistent"
Corrected

L441: Kendal correlations of 6.4 and 8.7? Maximum value should be 1.
These are percent values. Corrected in the text

L442-444: The sentence is not clear and it should be revised.
This sentence is reformulated as :
" However, for this catchment, the response of the piezometric level to the precipitation is small for the studied events. This can be explained by the fact that the extend of the water table (1849 km^2) is large compared to the Ardeche catchment area (622km^2). Consequenlty, the variations of the piezometric level is not reliable to assess the simulated moisture of the deep layer for this catchment during flash flood events. "

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

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