Authors' Response to Reviewer 1

Authors' response (A, black) to the specific comments of the Reviewer (R, blue).

Specific comments

R: Novelty: I think you should state more clearly that your study is a novel contribution in respect to both the ways of introducing uncertainty on soil properties (if I understand correctly, this is done more simple in other studies?) and that you take the temporal resolution into account in your analysis (which is not considered in Refsgaard et al. (2016), Hansen et al. (2014), He et al. (2015))

A: We thank the Reviewer for underling the novelty of the study. We agree that both aspects, i.e., characterization of the uncertainty in soil properties and temporal resolution, represent a novel contribution and further improvements of current methodologies. For these reasons, we will better emphasize these novelties in the new version of the manuscript. However, we want also to clarify here that there are other perturbation methods that can be considered even more sophisticated (see discussion at Page 4, Line 16-28). But exactly for this reason, these methods are more difficult to be directly applied in e.g., hydrological modelling studies and more simple approaches are usually adopted. By that, we think that the approaches presented in the present studies should be not considered as more complex, but rather as a way to fill a gap among the available methods towards a flexible representation of different uncertainties in soil properties while maintaining a relatively easy integration in hydrological modelling studies. These considerations will be also added in the new version of the manuscript.

R: Title: I suggest changing the title to "Effects of uncertainty in soil properties on simulated hydrological state and fluxes at different spatio-temporal scales"

A: Thank you for the suggestion, we will change the title accordingly.

R: Figure 1 and Page 3, line 22: When first seeing figure 1 and reading the text, I was a bit confused about the transect depicted in the figure. After reading the rest of the article I now understand that it is a horizontal transect through a catchment and not a vertical transect (showing how the sand% change with depth). Could you maybe make this more clear in the text and also in figure1?

A: We will clarify that the depicted line is a horizontal transect to avoid misunderstanding in the new version of the manuscript.

R: Page 3, line 4: I suggest adding some extra text to this sentence, which tells the reader that you are using more sophisticated methods to describe the uncertainty, compared to the studies you mention on page 2 that use more simple assumptions. In this way you clearly indicate that your work is novel.

A: We thank the Reviewer for emphasizing the novelty of the method. As reported in the comment above, we will better clarify in the new version of the manuscript the new aspects of the methods in comparison to the approaches available in literature.

R: Page 5, line 11: I do not understand what you mean by "the vertical soil horizons are aggregated to the total soil depth of 2 m"?

A: The original soil map provides the information for different soil layers to the depth of 2 m. For the present study, this vertical distribution is not accounted for and the vertical average soil property is calculated for each soil unit. We will rephrase the text to avoid misunderstanding.

R: Page 5, line 12: How do you define the 29 soil units? Could you show the units on the maps of figure 3?

A: We use the term soil units to indicate the presence of 29 polygons within the catchment. The term is used to avoid misunderstanding with the term soil class e.g., two soil units could be belonging to the same soil class (e.g., clay loam). But if the Reviewer suggests another terminology, we are open for further suggestions.

R: Page 6, line 21: How is the upscaling done? Is it just taking an area-weighted average of the parameters?

A: The upscaling rules are different for each parameter. The rules were selected based on different specific studies and they are reported and described in Kumar et al. (2013) and Samaniego et al. (2010). However, it has to be noted that in the present study we are actually not upscaling the soil parameters because the generated soil maps are at the same resolution as the model grid i.e., 500 x 500 m^2. For this reason, the upscaling rule does not affect the result of the present study. In the new version of the manuscript we will clarify these aspects.

R: Page 8, line 8 + Figure 2: So are the gauging stations shown on figure 2 "artificial stations" you put in to define the subcatchments you use in analysis #3? If so, could you call them something ells on figure 2 that indicates that these are not real gauging stations with actual measurements

A: The locations represent real gauging stations. We preferred to use these positions instead of arbitrary choices. But the Reviewer is right in saying that we did not use the actual streamflow measurements for comparison but we rather use the locations only to define the drainage areas. We will clarify this in the new version of the manuscript.

R: Page 10, line 32 + page 11, line 1: Are these average CV values across the catchment (15% for Q, 11% for GWR, 3% for SM and 1% for AET) for all the perturbation methods all together (that is how I read the first part of the text) or for the RE method only i.e. the results in figure 6 left (this is how I understand the parenthesis on line 1, p.11)? Please make this more clear in the text.

A: Yes, they are and additional information will be added in the new version of the manuscript to avoid misunderstanding.

Page 11, line 14-15 + figure 7: So you calculated correlations coefficients for each of the 3 perturbation methods and then afterwards the average and standard deviation of these R2 (which is plotted on figure 7)? Please specify this in the text and in the figure text.

A: Yes, we did. We will better specify this information in text and legend.

Page 11, line 20: It looks to me as the pattern in soil moisture uncertainty is very similar to the patterns in clay%? When I visual compare the CV SM map in figure 6 (left) and clay% maps in figure 4.

A: We checked again the correlation coefficients calculated between CVs and clay. The values are correct. From visual comparison, we see, on the one hand, that one soil unit is remarkable visible with high values in the CV SM map, i.e., one long soil unit crossing the entire catchment from south-west to north-east. On the other hand, however, other locations are not highly correlated. For these reasons, we believe that the visual comparison could be misleading for the average correlation over the entire catchment as it is quantify instead by the calculated correlation coefficient.

R: Page 11, last section: When reading this I was wondering why the AET is not correlation to soil moisture. But you give the explanation on page 12 line 17-18, that AET is close to PET most of the time, and I guess that is why they are not correlated? Maybe you could also mention this explanation on page 11?

A: In this section we discuss the correlation and we still do not provide the actual values of SM and AET to justify the behavior. For this reason, we rather prefer to stick to the results presented i.e., to discuss the correlations here and to leave the interpretation for later in the text where the actual values are depicted. However, we will refer at page 12 to page 11 for giving reasoning for the observed correlations.

R: Page 12 line 29 + Page 16 line 6: I do not understand what you mean by threshold behaviour/condition?

A: The relation between soil moisture and fluxes is non-linear and based on threshold conditions. For instance, the fast interflow runoff component is activated if a certain soil water content is available. For this reason, if we have uncertain SM but these values are below this soil water content, we will not have uncertainty on simulated runoff. It can now happen to have the same uncertainty in SM but in a range where SM is above this water content. In this case, runoff is also affected. A similar consideration holds for the relation between SM and AET but for soil drying conditions i.e., in case SM is below a certain soil water content, AET is affected. For these reasons, the uncertainty in SM does not always reflect the uncertainty in the other fluxes. We will extend this discussion in the text to better clarify the hydrological behavior. It has also to be noted that these considerations are not specific to the hydrological model used in the present study but they hold for most of the hydrological models.

R: Page 13, line 18-26 + point 5 in conclusions: You conclude that stream flow, which is an integrated flux, is only sensitive to large spatial structures, whereas the local states and fluxes (i.e. soil moisture, AET, GWR) are sensitive to small scale variations. This makes sense to me. But I would like some more explanation (on page 13) on how you see this from the graphs in figure 9, since that is not clear to me.

A: We agree with the Reviewer that we did not clarify enough how it is possible to infer these conclusions as they are actually given by comparing both Figure 5 and Figure 9. Figure 5 presents the uncertainty introduced in the soil properties by each method. Here we show how RE method perturb long spatial structure while CP method only small scale features. Figure 9 represent the uncertainty in the model output. In this case, we can look at the streamflow of the catchment (e.g., SF CV for catchment > 60 x 60 km^2) and see that this model output is strongly perturbed by the RE method, and for that, only by the long spatial structures. In the same figure we can look at the uncertainty in SM or AET at the model resolution or, eventually, as they could be measured in the field e.g., catchment < 1 x 1 km^2. These model outputs are affected also by the uncertainty introduced by the CP method. For this reason, these local state and fluxes are sensitive to small scale variations.

This explanation will be extended and integrated in the new version of the manuscript. To this end, we rather realized that it is more convenient to first discuss the characteristic correlation lengths introduced by each perturbation method (Page 13 line 30 – Page 14 line 26) and later to extend the discussion to the implications for the specific model applications (Page 13, line 18-26). For this reason we will reorganize this section.

Page 14, line 7-26: I found this sections difficult to understand, please consider to rephrase so it is easier to read. Since you are talking about "representative scale" in the section, I suggest that you present the RES concept already here (you only mention it in the conclusion).

A: We agree with the Reviewer that the discussion requires further extension. We tried to combine relevant results obtained in different disciplines (i.e., the REA scale, the RES concept, the ergodicity concept) but we rather provide only limited information about that while referring to additional references for further details. In the new version of the manuscript we will elaborate the discussion to better clarify the different concepts also within this manuscript.

R: Page 16, line 24-25: I think you should make it more clear, that you have done something new compared to the other studies using the RES approach. I suggest starting the sentence with something like "This study proposes two extensions to the RES approach…"

A: Yes, we will emphasize more the novelty of the present study. Thanks.

Technical comments (not listed)

A: Please note that all the technical comments provided by the Reviewer will be integrated in the new version of the manuscript and are not reported in this response document.