Authors' Response to Reviewer 2

We thank the Reviewer for the positive feedback and for the additional suggestions provided to improve the manuscript. We will integrate all of them in the new version of the manuscript as discussed below.

Thank you and best regards,
The Authors

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

Specific comments:

R: Parameter maps. Currently the mentioned link between soil properties, model parameters and model fluxes is never explicitly stated within the paper. The reader is left a alone with a set of references (see: p. 6, l. 25f) that lead to a detailed description of the model with all its pedotransfer functions. I believe that many readers would appreciate if the (most) relevant pedotransfer functions are explicitly mentioned (e.g. within a table). Following that reasoning, one would also like to see some maps (they should be readily available) of the change of parameters with respect to a change in soil properties. Such maps could be made in a similar fashion as Fig. 4, where a perturbation example for percentage of clay is mapped. This would make it easier for readers to connect the information shown in Fig. 3, Fig. 4 and Fig. 7. The paper does already provide ample amounts of supplementary material, so it could at least be added there.

A: we agree with the Reviewer that additional information regarding the pedotransfer functions (PTFs) will help the readers to better connect the perturbation of the soil properties (texture and bulk density) to the simulated state and fluxes. For this reason, (1) we will provide a list of most relevant pedotransfer functions (PTFs) integrated in the model, and (2) we will provide some maps of the soil hydraulic parameters estimated based on these PTFs in the supplementary material.

R: P 5. l. 24. This comment might be a little nit-picky. It would be nice if the authors would elaborate a little about the mentioned bounds. One or two sentences would already suffice to make the picture clearer: Besides the 100% upper bound in the sum, the soil properties are (obviously) also bound at 0%. Furthermore there sum always results in 100%. That is (of course) the reason why the silt does not need to be perturbed directly, as it is fully defined by the other two textural classes. Now, this is all clear to readers but I think it would be good to be mentioned explicitly. Furthermore, one might expect that these bounds mess with Gaussian noise in some minor way. Intuitively one would expect that this lowers the uncertainty in some areas of the basin. This notion is dispelled in Fig. 5. It shows clearly that these theoretic influences are as good as non-existent. They are not are not visible at all! Nevertheless, there are areas in Fig. 3 (and 4) where the soil properties are exactly at these bounds – the sand is at 100% and the clay is at 0%. I think one should at least be clarify these aspects, even if they seem to be irrelevant for the resulting analysis (as shown in Fig. 6). On the other hand, it could be that I am missing something.

A: we agree that forcing the texture perturbation to bounds (i.e., 0 < texture < 100) (i) could mess with Gaussian noise and (ii) could lower the uncertainty in areas of the basin where the actual values are close to the bounds. As also pointed by the Reviewer, these characteristics are not relevant in our study. This is because the noise used (variance) is relatively low and the areas within the catchment where the actual values are close to these bounds (0 or 100%) are relatively limited. Still, these characteristics of the perturbation methods could be relevant for other studies i.e., when the noise introduced is higher or extreme texture values are more frequently presented within the catchment. For these reasons, we think that the comment of the Reviewer is relevant for a more general understanding of the perturbation methods and we will add this information in the revised manuscript.

R: P. 5, l. 21-22 and Table 1. The noise is defined by its variance. I would propose to use the standard deviation instead. This would (a) make it easier for readers to interpret (clearer units, simpler dispersion summary); (b) bring closer to the units used for the analysis (Fig. 5 shows the standard deviation (!) of the clay-ensemble); and (c) sync in a rather abstract way with the uncertainty quantification (the coefficient of variation is defined through the standard deviation).

A: we agree that the use of the standard deviation will simplify the interpretation and it will be consistent with Figure 5. For this reason we will modify these values in the revised version of the manuscript. It has to be noted, however, that the variograms (e.g., exponential model used for the spatial correlated method) are usually defined by variance and correlation length. To our knowledge, it would be rather uncommon to present the variogram model in terms of standard deviation. And for this reason, the variograms depicted in the supplementary material (Figure S2 and Figure s3) will be not modified.

P. 13, l. 18-22. These sentences need to be reformulated somehow. The intention or setup is not clear to me. In concrete: The phrasing “streamflow at the catchment outlet” is used twice, which makes it difficult to understand.

A: streamflow at the catchment outlet is sensitive to the perturbation of long spatial structure while it is not sensitive to small scale soil perturbation. This result is shown in Figure 9 comparing the CV of SF obtained for the bigger catchment (64x64 km^2) based on the RE and CP methods, respectively. A similar lack of clarity in the discussion of these results was underlined also by the other Reviewer. Overall, we realized that it is more convenient for a better understanding 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). We will reorganize this section in the revised manuscript, accordingly.

P. 17, l.5-20. The section is understandable as such, but could be rephrased to make it easier to read. As I understand it, the gist is that fine-grained soil information is important for local states and fluxes but not for integrated ones. As it stands now, one reads at first that the fine soil resolution is not important for model performance (p.17, l. 9), only to read a view later that the fine soil resolution is important for model performance (p.17, l. 14-15). Readers will infer the meaning from the context, but the phrasing seems to be needlessly difficult.

A: the gist is very well summarized by the Reviewer. We will phrase the text accordingly to avoid possible misunderstanding.