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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On the effect of the uncertainty in soil properties on the simulated hydrological state and fluxes at different spatio-temporal scales" *by* Gabriele Baroni et al.

Anonymous Referee #2

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The contribution analyses the uncertainty of model fluxes with regards the soil properties. The setup is as following: Different distributed soil properties – the percentage of sand and clay, and bulk density – are linked through a functional relationship with the parameters of the hydrological model mHM. Changing the soil properties has thus a direct impact on the model states and fluxes. The variability of the fluxes with respect to changes in the soil properties is investigated by perturbing the different properties with three different methods. The resulting ensemble of model states and fluxes is then analysed with regard to the uncertainties at different spatial and temporal scales. The contribution is novel, well written and logically structured. It is well suited for a publication in HESS. As is so often the case some small adaptions could be made. I therefore

recommend publishing it after some minor revisions.

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Specific comments:

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.

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.

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).

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.

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.

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