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Comment on hess-2021-324

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Referee comment on "Guidance on evaluating parametric model uncertainty at decision-relevant scales" by Jared D. Smith et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-324-RC1>, 2021

I have read with interest the manuscript entitled 'Guidance on evaluating parametric model uncertainty at decision-relevant scales'. The study examines the sensitivity of the simulations of a spatially distributed ecohydrological model to the model parameters for calibration purposes. Sensitivity is considered with respect to different model output metrics that correspond not only to performance metrics assessed at the basin outlet, but also statistics of the model output calculated for the different hillslopes of the basin where no output observations are available.

The study is a welcome contribution to the field of sensitivity analysis of spatially distributed models, which is challenging due to the high dimensionality of the parameter space of these models and which requires further investigations. The study discusses the issue of calibration and uncertainty estimation in absence of output observations, in particular at internal locations of a river basin where model-based information is critically needed to support decision making.

Overall, the manuscript is well written, the analyses were performed with care and the experiments are well documented in the supplements. However, I have a number of suggestions and I think that a number of points need clarification, in particular regarding the choice of (output) sensitivity metrics and the analysis of the multipliers, as detailed below.

p1 L8 'parameter multipliers': I suggest adding 'for spatially distributed parameters' for clarity.

p1 L15-16 'for some parameter multipliers [...] reducing dimensionality.': This needs clarification.

p2 L31 'sensitivity metrics': I suggest specifying what this term refer to for clarity (e.g. performance measure or statistics of the simulated model output).

p2 L34-35: Could you provide some references/examples for this?

p2 L37-39 'Matching [...] with controlling extremes.': A link between this sentence and the rest of the paragraph is missing.

p3 L92-93 'will be evaluated [...] error model': The authors should clarify whether they refer here to future studies that may use the guidance presented in the manuscript.

p4 L107 'performance measure': Consider revising the terminology. The metric of Eq. (2) is not really a performance measure, as it does not use observed values.

p5 L130-131 'Therefore, [...] the TN estimation method': This sentence is not clear to me. Observed data are also used for the streamflow objectives (Eq. (1)). In addition, does the water quality objective only consider the basin outlet or also hillslopes?

p5 Section 2.3 A justification for the choice of the four calibration performance measures is missing (e.g. a justification could be that these metrics are typically used in previous studies, and in this case some references to some of these studies should be provided). In addition, I think that the metric of Eq. (1) could also be used for calibration purposes. Therefore the difference between the decision-relevant and calibration-relevant sensitivity metrics is fuzzy.

p6 L170 'EEs for each parameter [...] in the parameter domain.': Please add a reference for this (e.g. Pianosi et al. (2016)).

p6 L178 'Step changes': Does this refer to the quantity $\Delta_{\{s+1,s,p\}}$ of Eq. (7)? Please clarify.

p6 L178-179 'to allow for a uniform [...] within the specified bounds.': This needs to be better explained.

p8 L217 'mean EE value': Clarify whether this refers to the metric defined in Eq. (8).

p8 Sect. 2.6: I do not understand this point. The use of a multiplier for a certain parameter type is based on the assumptions that the value of the parameters of this type vary in the same proportions in different locations of the basin, and I do not understand why these parameters should have similar sensitivity (here EEs).

p11 L300-303: This is based on a rather strong assumptions that the C-Q relationship obtained using the WRTDS regression method can be extrapolated to other flow conditions. This may not be appropriate for in-depth nitrogen study, in particular in agricultural areas with highly varying nitrogen inputs. I understand that it is not the point of this study to have a sophisticated nitrogen routine. However, I think that some comments should be added to further highlight the simplifications/limitations of the data-driven nitrogen routine used. My comment also refers to the sentence p21 L503-505.

p13 L359 'healthy ecosystem': I suggesting replacing this expression by something like 'more humid ecosystem' for clarity.

p14 L373-374: 'This result demonstrates [...] to calibrate.': This sentence needs clarification.

p21 L511-514: The authors could refer to the study by Cuntz et al. (2016), which also demonstrates the importance of including in the calibration some parameters that are typically set to fixed values and in particular hard-coded parameters, using the NOAH-MP land surface model.

p22 L553-555: I think that it should also be emphasized that, because of the issue equifinality, calibration strategies that identify an ensemble of possible parameter sets (as compared to a unique 'best' solution) and that therefore consider parameter uncertainty are more appropriate.

Minor edits

p3 L61: I suggest replacing 'will' that 'are typically'.

p7 L192: replace 'smaller' by 'smallest'.

References:

Cuntz, M., Mai, J., Samaniego, L., Clark, M., Wulfmeyer, V., Branch, O., et al. (2016). The impact of standard and hard-coded parameters on the hydrologic fluxes in the Noah-MP land surface model. *Journal of Geophysical Research*, 121(18), 10,676-10,700.
<https://doi.org/10.1002/2016JD025097>

Pianosi, F., Beven, K., Freer, J., Hall, J. W., Rougier, J., Stephenson, D. B., & Wagener, T. (2016). Sensitivity analysis of environmental models: A systematic review with practical workflow. *Environmental Modelling & Software*, 79, 214–232.
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