Geoscientific Model Development Discussions

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Comment on gmd-2022-131

Anonymous Referee #1

Referee comment on "Global Sensitivity Analysis of the distributed hydrologic model ParFlow-CLM (V3.6.0)" by Wei Qu et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-131-RC1, 2022

In the manuscript by Wei et al, the authors apply global sensitivity analysis (GSA) using Latin-Hypercube One-factor-at-A-Time method to the distributed coupled model ParFlow-CLM. In general, I think that applications of GSA to complex and spatially distributed model are welcome contributions, given the challenges related to the long model run time and the high dimension of the parameter space.

However, I would recommend to reject the manuscript, as 1) I believe that it does not align with the aim and scope of GMD, 2) the objectives and novelty of the manuscript need clarification, 3) the implementation of the GSA has flaws and the interpretation of the sensitivity indices (based on provided equation) is erroneous, 4) the model calibration need clarification 5) the manuscript lacks a discussion of the results in light of the very large existing literature on sensitivity analysis and 6) the manuscript is unprecise at many locations. Please find below detailed comments.

1) The manuscript does not match the aim and scope of GMD, in that it does not describe a new model or new model developments, and it does not introduce a new method for assessment of models. Specifically, the GSA method used is published elsewhere (van Griensven et al., 2006) and the manuscript does not describe a novel framework for application of GSA to a complex and spatially distributed model. It actually does not describe the strategy used to apply the GSA method. The text p10 L236 suggests that the parameters were considered as being spatially homogeneous, but this would need to be better explained in the manuscript as this seems to be a very strong assumption. Previous works have examined the application of GSA to spatially distributed model and contributed to address this challenge (a few examples are: Herman et al, 2013; Rouzies et al., 2021; Smith et al., 2022; van Werkhoven et al., 2008). It is not clear how the manuscript may relate to such previous works.

2) The objective and the novelty of the manuscript need clarification.

- Sensitivity analysis was previously applied to the ParFlow-CLM model, and in this respect, the authors cite the studies of Jefferson (2015, 2017) and Srivastava et al., (2014). It is not clear how the manuscript relates to these previous studies.

- In addition, the authors mention as an objective "to test the transferability of the results to regions with other topographies and climates" (p3 P80-81). It is well known from many past studies that sensitivity indices can vary tremendously across places (an extensive review is provided in Wagener and Pianosi, 2019). Therefore, we know a priori that it is very risky to extrapolate sensitivity analysis results beyond the study location. I also think that the analysis performed by the authors in this respect lacks breadth, because it is only based on three study sites.

3) The implementation of the GSA has flaws and the interpretation of the sensitivity indices is erroneous.

- The sensitivity index is defined in Eq. 6-7. The index is actually different to the one introduced in the original method (Eq. 7 in van Griensven et al., 2006). In van Griensven et al., the index corresponds to the average of the absolute value of the partial effect, while in the manuscript it is directly the average of the partial effects. The problem of not using absolute values is that compensations between the partial effects can occur when the model response is non-monotonic. This has been discussed for instance in Campolongo et al. (2007) with respect to the Morris method and it is common practice to use the absolute value of the partial (elementary) ieffect to avoid this compensation problem (e.g. Eq. 1.49 in Saltelli et al., 2008).

- The interpretation of the sensitivity index (e.g. p5 L128, p11 L247, p11 L254-255) is not correct. The sign of the sensitivity index of Eq. 7 does not provide reliable information on the direction of change (as I mentioned earlier, the relationship may be non-monotonic) and a sensitivity index equal to zero does not mean that the parameter is not influential (as there can be compensation effects when absolute values of the partial effects are not computed).

- The authors only use a subset of parameters for GSA (12 parameters), while the number of parameters is much larger in such a complex model. In particular, it has been shown

that land surface models can include a large number of hard coded parameters with an empirical basis (that are therefore largely uncertain) and many of which can have a large impact on the model output. This is documented for instance for the NOAH-MP land surface model in Cuntz et al. (2016) and Mendoza et al. (2015). Therefore, the authors would need at least to discuss this issue in the manuscript.

- Information on the setup of the GSA is missing. In particular, it would be crucial to know the sample size used and whether this sample size is large enough to obtain robust sensitivity indices estimates (as explained in Sarrazin et al., 2016). An indication on the sample size is also important from a methodological point view to understand which resources are required to run the analyses.

- The GSA only covers about two years of simulations, which may be too short to obtain reliable and period-independent sensitivity indices estimates. I refer for instance to Shin et al. (2013) when the impact of the length of the simulation period is discussed). The sensitivity indices may take different values over another simulation period. I also did not understand what the time period selected is. In Sect. 2.5.2 the period is "from 2016-07-01 to 2018-31-12" is reported, while in Figures 4-5 "from 01.01.2021 to 31.01.2022" is reported.

4) The model calibration criteria are unclear and I think that the interpretation of the calibration results are not complete.

- Based on which criteria was the "best simulation" (p9 L223) identified?

- "This indicates that there are still inaccuracies in the model parameters" (p10 L235): Mismatch in the simulated water balance could come from both issues in the parameter values (due for instance in part to the fact that the bias may not be considered as a criterion to select the best simulation), but also the model structures.

- The authors would need to better explain the objectives of calibrating the model before performing GSA.

5) The manuscript lacks a discuss section that would discuss the results in light of the very

large existing literature on sensitivity analysis.

6) At numerous locations, the text is unclear to me and I provide examples below:

- p1 L25-26 "help us to reduce the computational demands of completing multiple simulations of expensive domains": this needs clarification

- p2 L47 "Thus": I did not get this logical link between the two sentences.

- p2 L51-52 "In addition, the identification of sensitive parameters should also help to reduce the danger of non-unique solutions, i.e. equifinality": to me the issue of equifinality arises because there are limited data/information available to constrain the model structures and parameters.

- p4 L122 "This is more efficient than LH": I did not get why and what is meant by LH. The GSA method could directly be applied using LH without building a tailored sample?

- p16 L329-332: The two last sentences of the manuscript are fuzzy. Therefore, the conclusions of the manuscript are not clear.

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