Reviewer #2

This paper by Lutternauer et al. performed parameter sensitivity analysis of a land surface model at two watersheds in France. The authors employ various sensitivity analysis methods to assess an extensive set of model parameters, which is quite interesting.

We thank the Reviewer for the encouraging assessment of our work. We provide in the following our answers to the comments emerged.

However, I do have some concerns that I feel need to be addressed:

1. My major concern is on the water balance at those two catchments. The average evapotranspiration is only 38.6% and 11.6% of total precipitation in those two catchments, which means runoff (surface and subsurface) must be large. Does that align with the observations at those catchments? Are there observations available to evaluate the model performance? I am concerned because I feel that the sensitivity analysis is most useful when the parameter values are sampled around their optimal values in the multi-dimensional parameter space. Otherwise, the analysis may not reflect the real parameter sensitivity. For example, in an extreme case, if the model simulates predominantly surface runoff with minimal evapotranspiration, parameters linked to evapotranspiration would exhibit weak sensitivity, which does not reflect reality. It would be helpful if the authors can show the observations of discharge, or evapotranspiration, if available.

With reference to the evaluation of model performance, we stress that our purpose is not to perform model calibration. Rather, we are placing our study in the context of model diagnosis. Hence, we place our study in the context of *ab initio* global sensitivity analysis. As we state in the original manuscript, this is a critical step that needs to be performed prior to model calibration. It provides insights on model functioning and on the way its response is affected by parameter uncertainty. In this sense, our global sensitivity analysis is performed prior to model calibration. One can then perform a global sensitivity analysis after model calibration and assess the way residual parameter uncertainty (i.e., conditional on available data) influences the residual uncertainty associated with model outputs. This second step has a different purpose than the one we consider in our study, as recognized in a variety of studies.
Prompted by the Reviewer's comment, we will place further emphasis on this point in our revised Introduction.

2. Related to the first point, I feel the paper can be strengthened if all components of the water balance can be included.

As we state in our answer to the first comment of Reviewer #1, we did not take surface runoff under consideration. This is tied to the fact that it was not observed at either of the two sites. The soil is quite permeable in both catchments. It is mostly covered by vegetation and rain intensity is not high enough to generate significant runoff under those conditions. As such, our conclusions hold for sites or situations where runoff can be considered as negligible when compared to the other water flux component (evaporation, transpiration, and groundwater recharge). This aspect will be clarified in the revised manuscript.

3. The authors mentioned that the model was run in a distributed way, but only selected one grid for each catchment for analysis. I am wondering if the model is three-dimensional. From what I read, the model seems to be a one-dimensional grid model. If it is one-dimensional, running the other grids should not affect the results. Some clarification would be very helpful.

The hydrological model NIHM is a two-dimensional (in the horizontal plane) physically based coupled surface/subsurface model. For each element/cell of the 2D mesh, the recharge is computed by the land surface model NIHM-MLSM (the detailed description of the model is provided in the Supplementary Material). The Global Sensitivity Analysis concerns NIHM-MLSM. As we clarify in the Supplementary Material and in the body of the manuscript, the latter is a one-dimensional model which has to be run for each element/cell of the mesh associated with NIHM.

Prompted by the Reviewer's comment, we will add some additional details about this point in the revised manuscript.

4. The paper does not have a "discussion" section, which limits the paper's impact. I feel some discussion would strengthen the paper substantially. For example, how does the sensitivity analysis results compare with other studies? How do the four sensitivity analysis methods differ? Do the sensitivity analysis results reveal some important insights into the model mechanisms, or the hydrological conditions at those catchments?

Prompted by the Reviewer's comment, we will: (*a*) expand on the meaning of the sensitivity indices employed; and (*b*) reorganize the session on results.

As we state in our reply to Reviewer #1, the results of a (global or local) sensitivity analysis are model dependent. Even considering diverse LSMs sharing some parameters, the relative weight of some parameters may change. Otherwise, we think that, since water availability is a key variable for evaporation, transpiration and groundwater recharge, soil related parameters will play an important role. We will explicitly address this issue in the revised manuscript.

5. I feel it could be helpful to show the values of LAI and albedo in the manuscript, either using a figure or a table.

The following table will be provided in the revised manuscript. It lists monthly averaged values for Albedo and LAI.

	Month											
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**
Bruche												
Albedo	0.208	0.155	0.137	0.136	0.168	0.190	0.177	0.158	0.164	0.179	0.202	0.543
LAI	1.101	0.869	1.316	2.120	3.959	4.392	4.633	4.401	4.333	3.540	1.212	0.850
Doller												
Albedo	0.187	0.152	0.143	0.149	0.169	0.195	0.179	0.167	0.173	0.176	0.190	0.268
LAI	1.091	0.809	1.229	2.147	4.264	4.79	4.906	4.757	3.730	3.730	1.043	0.867

Table XX: Monthly averaged values for Albedo and LAI for the two locations considered in the Global Sensitivity Analysis.

Specific comments:

1. Units of field capacity and porosity in Table 2 are missing.

They were not included because these are dimensionless quantities. We will make this clear in the revised manuscript.

2. *Line 540: " For example, the value of B for the evaporation at Bruche during the month of July associated with the LAI of January must be zero. However, inspection of Fig. 6a does not reflect this anticipated outcome. This apparent anomaly is attributed to a random noise…"*. I don't quite agree with the authors. The results from previous steps might affect future steps. I don't think the B value for July evaporation associated with January LAI is necessarily zero.

We agree that this assumption can be questioned. In the revised manuscript, we will address this point considering the impact of the root zone related parameters on evaporation because these parameters are not involved in the computation of the evaporation (evaporation is assumed to occur in the litter zone only).

3. Figures 6 to 11 are difficult to read. Can parameter symbols be used instead of parameter index in those figures? Or at least, put the parameter identification codes in the manuscript, instead of the supplemental materials?

We will address this point in the revised manuscript.

4. What are θ_L and θ_p in Equation 25 in the supplementary material?

We apologize for the typos in the equation, that is now being rectified.

5. Can authors add how surface runoff is determined? I think that could help the readers understand some of the results.

As we state in our answer to the first comment of Reviewer #1 and comment #2 of this Reviewer, we did not take surface runoff under consideration, given the conditions of the two catchments considered.