Interactive comment on "Numerical modeling and sensitivity analysis of seawater intrusion in a dual-permeability coastal karst aquifer with conduit networks" *by* Zexuan Xu et al.

Anonymous Referee #1

In the submitted paper Xu et al. apply local and global sensitivity analysis on a density driven distributed model (SEAWAT) to simulate a coastal aquifer I Florida (US). They use the knowledge of previous studies to define the boundary conditions and initial parameter sets of the model. Then they apply a local sensitivity analysis on the 11 model parameters in respect to various output variables of the simulated matrix and conduit systems. The same analysis is repeated with a global sensitivity analysis method (Morris) to account for interactions among the model parameters. The parameter describing the salinity at the submarine spring outlet was found to be most sensitive but also the parameters describing the conduit properties were found to influence both the conduit and the matrix behaviour. The results of the more elaborate global sensitivity analysis

scheme differ for several model parameters indicating that parameter interactions have to be considered. Finally, the authors use the simulations obtained by the sensitivity analysis to conclude about the sensitivity of the karst system to external changes and about the most gaining observations concerning model parameter identification.

The paper tackles a very interesting field of research, which is the evaluation of distributed models via sensitivity analysis. The authors clearly show that such analysis provides valuable understanding of the model and system and they also highlight that the choice of the sensitivity analysis method has strong impact on the results and conclusions. For those reasons I definitely recommend this paper for publication in Hydrology and Earth System Sciences. However, some weaknesses have to be removed first:

1. The paper is much too long. In their last paragraph of the conclusions (and also in the abstract) the authors clearly state the main outcomes of their research. However, within the body of the manuscript, they lose themselves in details to often.

2. The usage of two sensitivity analysis schemes provides a lot of insights into their differences. However, the authors do not explain why they actually compare them. For many modellers the interaction among parameters is an accepted fact. So for the sake of focus and length of the manuscript: Is it really necessary including the local sensitivity analysis? If not, delete. If yes, provide more explanation why.

3. Some more links between the model setup and field observations/previous work is necessary. It is clear that a lot of previous work was done at the study site. But sometimes it would be helpful providing some summarizing information in addition to the reference to the previous studies.

4. The elaboration of Morris's method has to be improved.

5. A clear discussion relating these results to the result of other is missing.

6. No state of the art of sensitivity analysis is missing (and no comparison to other

sensitivity analysis studies with lumped of distributed approaches in karst).

I think these corrections can all be done within the frame of moderate revisions. Please find some more specific comments in the attached and commented pdf.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2017-85/hess-2017-85-RC1-supplement.pdf

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