Response to reviewers' comments

"Two-way coupling between the sub-grid land surface and river networks in Earth system models" by N. W. Chaney, L. Torres-Rojas, N. Vergopolan, C. K. Fisher

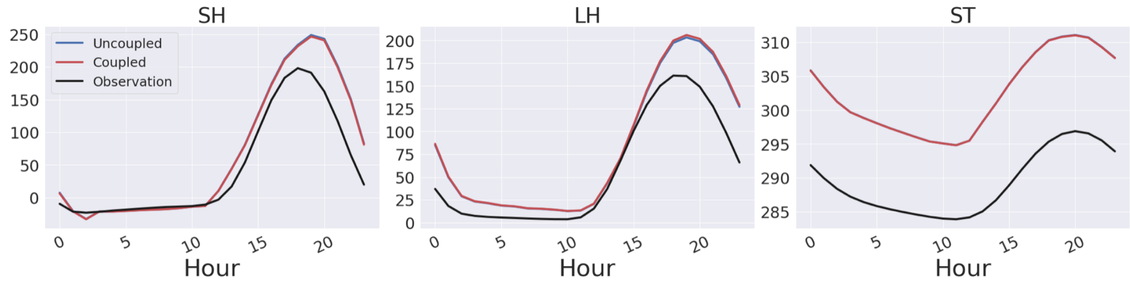
We thank the reviewers for their time and helpful comments. We have addressed each point below. Reviewer comments are shown in *blue italics*, while author responses are shown in unformatted text.

Reviewer #1: I appreciate the authors' responses, which addressed part of my comments. I still would like to challenge the authors to further elevate this manuscript. In particular, I have the following feedback on the authors' responses.

We thank the reviewer for the constructive feedback. We provide responses to the reviewer's comments below.

It is true that most ESMs don't represent two-way coupling between their land and river components. A complete representation of it involves both the suitable process description and, more importantly, a proper parameterization strategy. The former is relatively easy since the ESM modelers just need to borrow some governing equations from the well-established fields of hydrology or hydraulics. The latter, however, is much more challenging and, in my opinion, more critical in a model development paper like this. For a new process description in ESMs, a good parameterization usually means the parameter values are provided via some a priori estimation or extensive calibrations and ready to use for the potential users. As we are all aware, for ESMs it is usually not practical to expect the users to define/calibrate the parameter values by themselves because 1) it is computationally prohibitive to perform sufficient parameter calibration with ESMs like we used to do with much cheaper hydrological models and 2) very often the ESMs users have a very diversified background and many of them do not have adequate hydrology background to carry out such parameter estimation on their own. In a nutshell, when adding a new process representation in ESMs, it is most important to demonstrate that its corresponding parameterization strategy is compatible with the process description and hence effectively improves the model predictions in some aspects. In some sense, without a proper parameterization, adding a new process description will not necessarily bring better model predictions, particularly for sophisticated models like ESMs. Therefore, it is not obviously beyond the scope of this study to add sufficient model evaluations against the ARM SGP observations that the authors already have, and equally importantly, other observations at a regional or global scale where ESMs are typically applied at. In any case, it is my understanding that, when publishing a new process development in ESMs, it is more important to have a good parameterization strategy and adequately validate the new development against some observations. If the editors feel that it is not necessarily the standard for GMD, I am also fine with it.

We appreciate the honest feedback from the reviewer. We agree that a parameterization aimed for use within ESMs should show improvement when compared to observations. However, determining the observations one uses to evaluate a parameterization like this one is not as trivial. Let's take for example the spatial means of surface fluxes and land surface temperature over the entire domain and compare them to the VARANAL database which provides macroscale observations of surface fluxes derived from network of eddy covariance stations and energy balance/bowen ratio in-situ stations overvthe SGP domain. The figure below compares the modeled average summer of 2017 diurnal cycle of surface latent heat flux, sensible heat flux, and skin temperature of the coupled and uncoupled (baseline) simulations against the observations.



The coupling leads to a minor increase in macroscale latent heat flux and a minor decrease in sensible heat flux. When compared to the observations this leads to a slight (but almost negligible) improvement in SH and a slight deterioration in LH. Based on this one could conclude using the baseline model parameters that the implemented parameterization doesn't improve the macroscale spatial mean (which is already clear in Figure 8). A comparison with discharge would most likely show almost the exact same story. However, we argue that that conclusion would be misguided as there are large changes in the modeled spatial heterogeneity of the system which would play a key role in the land-atmopshere interactions over the system. Furthermore, the implemented parameterization enables the model to represent key land/river interface processes that are known to be important yet are almost completely lacking in ESMs. As such, we argue that simply evaluating and calibrating the parameterizations against the avalaible macroscale spatial mean observations at SGP is not appropriate (and strongly misleading).

Instead, it should be evaluated using observed remote sensing spatial fields of surface fluxes, inundation, and land surface temperature. One might imagine using MODIS LST or Ecostress LST to perform this evaluation/calibration. And indeed the co-authors have an ongoing study in which they are doing just that. However, we strongly argue that that type of evaluation is out of the scope of this study as it involves multiple datasets, a new calibration strategy for tiling schemes, etc… As such we aim to publish that work in a different study. Furthermore, simply including the comparison to the spatial mean VARANAL data would be highly misleading at it misses the point as to what we are aiming to accomplish with this work. For these reasons, we are leaving the evaluation/calibration for a subsequent paper and instead focus exclusively here on the description of the new parameterization and the sensitivity analysis to explore the parameterization. That being said, we agree that the paper should mention the planned strategy to compare to observations. We have updated the discussion to describe a path towards properly validating and calibrating the parameterization; this is now discussed in the first subsection of the discussion section.

Moreover, the explanation of using a constant and uniform velocity for the HRU-level impulse response function is not very convincing. The authors stated that "Since the fixed flow velocity assumes the flow to the channel is not impacted by more/less water down the hillslope, this assumption will be valid." This assumption "the flow to the channel is not impacted by more/less water down the hillslope" is not quite rigorous. At an hourly or shorter time step, this assumption is certainly not correct, because overland flow velocity and the travel time will be affected by the surface runoff depth, as indicated by the well-known kinematic wave equations, e.g., Manning's equation. At a daily time step, such a statement may be ok, if slightly rephrased, since the impacts of runoff depth are somehow negligible in terms of travel time, but then all the other advantages of having two-way land-river interactions and sub-grid heterogeneities might not be significant at a daily or longer time step as well.

We thank the reviewer for this constructive feedback. We agree that implementing a kinematic wave to solve surface runoff along the hillslopes would be a valuable contribution moving forward. As the reviewer points out, there will be important limitations when it comes to modeling the flow velocity when we are considering ponding in regions near the channel. To evaluate its impact we have enhanced the sensivitity analysis performed in this study by including the uniform flow velocity parameter. We also now use a Sobol sensitivity analysis—a more formal variance-based sensitivity analysis to evaluate the role of each parameter. As shown in Figure 12, the results show that although the model is not as sensitive to the uniform overland flow velocity parameters as other channel-specific parameters, its impact should not be ignored. As such, in future updates to HydroBlocks we will add a kinematic wave solver for overland flow along the hillslopes. However, for this paper, we are comfortable leaving the uniform velocity approach. We have updated the discussion to indicate the need think more critically about this challenge and how it could be addresed moving forward.

We would again like to thank the reviewer for their time and helpful comments.