Dear Dr. Ali,

We greatly appreciate your precious time in reviewing our manuscript. We addressed each of your comments in the revised manuscript. Our responses to your comments are listed below and marked in blue following each specific comment.

If you have further suggestions for changes, please let us know.

Dr. Guoping Tang
On behalf of co-authors

Interactive comment on "Does consideration of water routing affect simulated water and carbon dynamics in terrestrial ecosystems?" by G. Tang et al.

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General comments

The manuscript by Tang et al. examines how the consideration (or omission) of water routing (lateral flow) in a model structure affects simulated biogeochemical dynamics, especially water and carbon dynamics. To run their comparison, the authors used RHESSys with two modifications: (i) a rasterization to allow cell-based simulations and the emergence of (explicit) spatial patterns of hydroecological variables, and (ii) a new control interface to allow for a new water routing routine to be switched on and off within R-RHESSys. I enjoyed reading this manuscript and I especially liked the fact that the effects of water routing on simulation results were evaluated using a single modeling framework, thus reducing the bearing of model structure uncertainty when comparing simulations with and without water routing. In addition to dealing with a question/topic that will certainly be of interest to the HESS readership, the manuscript is well written and well organized, with high quality figures and tables and concise conclusions at the end.

Response: We appreciate your good comments.

My main issue has to do with the way model performance was evaluated (or put simply, the approach taken by the authors to assess whether the influence of water routing on simulation results could be deemed significant or not). The authors chose to compare simulated monthly average daily values of major hydro-ecological variables for the month of July 1994 only, arguing that temperatures were higher then and hence the effects of considering water routing on simulated water and carbon dynamics would be easier to detect. However, I would argue that doing month-to-month or season-to-season model evaluation is needed, i.e. a season-based sensitivity analysis that could highlight the relative influence of high summer temperatures versus intense fall storms on simulation results for example (I am speculating on the effects of different climate variables here since I do not have the data...). Is the watershed located in such a region that the hydrological year can be divided into a dry season with a relatively high water deficit (i.e., water limited conditions that are not prone to lateral flow) and a wet season with a relatively high water surplus (when lateral flow is more likely to occur)? Without that season-specific (or condition-specific) assessment, we are left wondering how generalizable the conclusions in fact are and whether those conclusions are just an “artefact” on the month of July 1994 that was relied on to derive (most of) them.

Response: *We appreciate your good comments. The Biscuit Brook watershed is humid watershed and precipitation does not have distinct dry and wet cycles. After fixing a bug in R-RHESSys codes, we repeated model simulation using same model parameterization and forcing data. The new simulation results indicated, even in this mesic ecosystem, that the consideration of water routing has important effects on simulated carbon and water dynamics. Specifically, based on new model simulations, the simulated three water fluxes (evaporation, plant transpiration and total AET) from land to the atmosphere, when averaged for the entire watershed, were greater respectively under simulation with water routing than those without water routing (see section 3.3 in the revised manuscript). In addition, the simulated carbon fluxes (NPP, soil autotrophic and heterotrophic respiration) were smaller under simulation with water routing than those without water routing (see section 3.4 and 3.5 in the revised manuscript). We revised related texts based on our new model results. In addition, as you suggested, we compared simulated monthly values of major hydro-ecological variable in 1995 under two contrasting climate forcing scenarios: one “wet” and another “dry” scenario. In the “dry” scenario, we set all daily precipitations for months May, June, July, and August in 1995 as zeros. The comparison between the “wet” and “dry” scenario indicated that consideration of water routing has more remarkable effects on simulated monthly carbon and water dynamics under the “dry” scenario than under the “wet” scenario. We presented and discussed related results in the revised manuscript (section 3.6 and 4.6 in the revised manuscript). We also revised our main conclusions. Finally, the new Figure A2 can also demonstrate the magnitudes of simulated carbon and water fluxes were distinct from each other between simulation considering and ignoring water routing (see Fig A2 in revised supplementary materials).*

Another (though not major) issue is raised on P12546 L21-23 when the authors wrote: “These statistics (Table S1, Supplement) suggested that R-RHESSys was able to accurately simulate daily stream and base flow regardless of whether water routing was considered”. This begs the question of whether the rasterization has, in fact, a much bigger impact on simulated water dynamics (as opposed to the regular RHESSys) than the water routing step/routine. I understand that the authors likely did not present simulation results from the regular RHESSys model here in order not to defeat their own purpose (i.e. having a single model structure with an ad-hoc routine that could be switched on and off) but it might be worth discussing in greater length why rasterization alone might lead to significantly improved results, especially when/if the aim is just to get accurate spatial averages (rather than spatial patterns).

Response: Thanks for your good comments. In fact, RHESSys is capable of being run in fully distributed mode. We clearly described this in the section 2.2 in the previous manuscript. As the leading author, however, Dr. Tang hasn’t gotten time to learn how to run the original RHESSys in fully distributed mode. Nevertheless, we believe that both RHESSys and its successor R-RHESSys can accurately simulate river streamflow and baseflow as well as other major hydro-ecological variables. The ability of RHESSys in simulating integrated carbon, water, and *nutrient dynamics has been justified by a number of studies. In addition, because almost all algorithms for simulating carbon and water dynamics were kept identical between R-RHESSys and its predecessor, it is thus a challenge to draw such a conclusion that the rasterization in R-RHESSys greatly improved the model's simulation without further comparison between the two models. We admit that it is important to do such a comparison study and this can be our future research.*

I have minor comments for additions to the text and edits below.
Other comments
* In several places in the text and in the abstract the authors refer to RHESSys as the "regional hydroecological simulation systems" while previous publications referred to the "regional hydroecological simulation system" (singular). Not sure whether the model was recently renamed: : :

Response: We revised related texts. RHESSys equals to "Regional Hydroecological Simulation System". The "system" is singular.

* About the study area description: basic information about climate and catchment architecture (drainage area, mean or median slope gradient and slope length) would be useful to have in light of the lateral flow processes examined in this paper.

Response: We added related information in the revised manuscript (see section 2.1 in the revised manuscript).

* About the land cover and soil data section: the authors were able to find land use and soil data at a similar spatial resolution (30m) but no information is provided about elevation data (which digital elevation model (or maybe LiDAR dataset) was used?)

Response: The USGS National Elevation Dataset at 1 arc-second spatial resolution (about 30 meters) were used in this study. We provided related information in section 2.4.

* One quick question about Figure 2: regardless of whether it is with or without water routing, during calibration or validation, NS values for baseflow are always as high if not higher than those achieved for streamflow; is that surprising?

Response: Thanks for mentioning such an interesting model behavior, which might result from two factors. First, the subsurface flow was technically simulated ahead of the overland flow in the model's design. The estimated subsurface flow was immediately utilized to modify soil moisture saturation deficit, which itself was used to calculate surface overland flow. Such a model design may more or less affect the simulated overland flow and further stream flow. In addition, it is challenging to balance the simulations of stream flow and plant transpiration. According to some previous studies, plant transpiration can account for two-third of total actual evapotranspiration in summer. In R-RHESSys, one way to increase plant transpiration is by way of increasing vegetation leaf area index. In our simulation, we attempted to make the simulated leaf area index to match observed values as best as we can, which can range from 1.2 to 3.9 m^2 m^{-2}. By doing so, the consequent simulated stream flow is slightly smaller, which also might have *contributed to the low NS values for the stream flow. We will check this kind of model behavior to improve the model's performance in the future applications.*

Minor edits
* Figure 2: on the y-axes of the graphs in panels (b) and (d) we should read BF rather than SF

Response: Revised.