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Hydrology and Earth System Sciences
Discussions

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

G. Ali (Referee)

Genevieve.Ali@ad.umanitoba.ca

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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 modelling 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.

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.

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).

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. . .

* 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.

* 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?)

* 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?

Minor edits

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

* P12539 L18: landscape-imposed redistribution of soil water is major causes for –> landscape-imposed redistribution of soil water is a major cause for

* P12539 L22: distributed hydrology models (DHM) that simulates –> distributed hydrology models (DHM) that simulate

* P12540 L11: delete 'however'

* P12541 L8-9: Maybe rephrase what is currently described as "an existing hydroecological model of hierarchical model framework"

* P12544 L11: Time-series daily maximum –> Time-series of daily maximum

* P12545 L13: time-series daily climate data –> time-series of daily climate data

* Caption of Figure 2: two typos simualted –> simulated represnts –> represents

* Caption of Figure 7: deificit –> deficit

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 12537, 2013.