Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-401-RC1, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessing reliability of hydrological simulations through model intercomparison at the local scale in the Everest region" *by* Judith Eeckman et al.

Anonymous Referee #1

Received and published: 19 September 2017

The manuscript by Eeckman et al., submitted to HESS for consideration for publication, compares two hydrological models in two very small catchments in the Everest region of the Nepal Himalayas. The high mountain water cycle, especially in the Himalayas, is not well understood and possible consequences of climate change on the water availability in these regions require a thorough understanding of the acting processes. These regions are typically sparsely covered by on-site observations and the quality of the data is often rather poor, deeming scientists to relay on modelling and remotes sensing observations.

This manuscript compares two models, 1) the modified ISBA surface scheme and 2)

the J2000 model. Both models have different parametrizations but can basically run with similar input forcing. Following the description in the manuscript, both models do not consider a deep groundwater compartment, nor do they consider any preferential drainage, e.g. by fractures or discontinuities, in the subsurface. The authors have chosen two very small catchments ($\sim$ 5 and 18 km2) where some onside measurements for river discharge and rain measurements exist. One catchment is situated in the central lesser Himalayas and is covered by five distributed rain gauges as well as one stream gauge at the outlet. The second catchment is situated in the very high Himalayas (no glaciers) and is represented by one rain gauge and one stream gauge, both located at the outlet. Unfortunately, very little information is provided about the data origin and its quality as well as its recording. Evapotranspiration, one of the very under-studied parameters in Himalayan water cycle, was here estimated using ground observations from a distant observatory located at the Everest base camp. The results presented here are basically limited to a comparison of the two models runs and to the hydrograph (without discussing the quality of the local measurements). As the authors point out in the conclusions the models perform well during monsoon, when the hydrograph basically mimics the rainfall pattern, while the long recessions during the post monsoon season have a notoriously weak reliability (with respect to the hydrograph). However, this season is the key to untangle the contributions from the different compartments. This weak performance outside monsoon season indicates that the models are probably not well adapted and eventually miss out important settings. At the same time, the periods outside of monsoon are the periods where water availability is most crucial and which might be most affected by a changing climate.

The study by Eeckman et al. presents a hydrological modelling exercise, however, considering the shortcomings in the models design and the relative spars equipped training catchments I think the manuscript is better suited for publication in a more modelling oriented journal. The main points of criticism are as follow: 1) the results do not contribute to understand the process of the Himalayan water cycle; 2) the input parameters of the models are not rigorously presented and the on-site monitoring

network, especially in the high Tauche catchment, is not well designed as test ground for model performance; 3) and last, I doubt that the two very small catchments can be considered as representative for any of the Himalayan geographical units and thus the findings are difficult to generalize in order to provide modelling advice to local decision makers or scientist around the world.

General comments:

* The authors criticize that most of the stations in high mountains are located low in the landscape. From the maps in figure 1, it seems that all the used stations are located along rivers and do not cover the high ridges.

* The explanation why the authors have chosen ISBA and J2000 is not entirely clear. Which other models are available and have been already applied successfully in the Himalayan region?

* One of the co-author, S. Nepal, has already published hydrological modelling results for the entire Dudh Koshi catchment (Nepal et al. 2014). How did that model perform for the two small catchments of this study and can the results be compared? Can the model by Nepal et al. 2014 be improved from the small scale findings of this work? My understanding was that the study from 2014 worked already quite well for a much larger region. Furthermore, how do the results of this modelling work compare to the modelling by Savéan et al. 2015, which is also covering the entire Dudh Koshi river catchment?

Specific comments:

* Acronym ISBA has never been explained (Interaction Sol-Biosphère-Atmosphère).

* Figure 1: maps have no geographic coordinates. Looking at figure 1D, do the authors expect that soil water storage can be an important reservoir in such a landscape?

* Table 2: ET, why did the authors decide to use the method by Hargreaves and Samani 1982 and not the empirical elevation method developed for Nepal by Lambert and

Chitrakar 1989 (Mountain Research and Development)?

* Page 10: The discharge data is not well explained. How has the data been obtained? What are the sensors? How has stage height been converted into discharge?

* Page 10: As for discharge, where is the data coming from? How was it recorded? . . .

* Page 10/line 14: I am surprised by the temporal definition of the monsoon. Usually April to beginning of June is termed pre-monsoon and is characterized by snow melting at high elevations. The DHM normally expects the start of monsoon around the 10-15th of June.

* Page 10: The precipitation interpolation method IDW needs to be better explained, to be understandable without reading secondary literature, at least the specific techniques applied in this work.

* Page 11: Please give more details on the method to spatialize radiation, pressure, humidity. . . For the non-Nepal Himalayas expert audience, a more detailed explanation of the Pyramid station is appropriate.

* Page 14: Can groundwater explain the mismatch during low flow?

* Page 15: Please discuss the soil water storage differences between the two models in more details.

* Page 16: It would be helpful if Dunne and Horton runoff are defined somewhere in the manuscript. Especially the differences between the two and how the two models treat these two components.

* Page 16: The authors find that most of the water drains through the soils rather than along the surface. Is this something other studies have already documented? What are the consequences of a preferential drainage through the soil compartment?

* Page 16 line 15: Please explain what you mean by “contribution of drainage to discharge”.

* Page 17: How do the snow water equivalent and snow cover findings compare to studies from the Himalayas, e.g. Wulf et al. 2016 (Advances in Water Resources) or Putkonen 2004 (Arctic, Antarctic, and Alpine Research) or the two publications of the Dudh Koshi hydrology Nepal et al. and Savéan et al.?

* Page 18: The authors claim that for the first time they apply the models at such a high spatial and temporal resolution in mountains. Can the authors explain what is the gain of such highly-resolved modelling, especially considering that the input data is much coarser or even from different locations? Has it been tested if such a high resolution is needed or would a rather coarse resolution provide similar results. In that light, what are the errors that are propagated from interpolation of the input data through the model into the results?

* Page18: If the time resolution has such an impact on the precipitation phase partition but the aim was to run both models with the same input parameters, why has the same temporal resolution not been used for both models?

* Page 19: In the final lines the authors state that the models can be used to predict water availability for power generation as well as under changing climate conditions. Can this conclusion really be drawn from the two very small scale studies? How can the models be scaled to different regions? What is the minimum input information needed to obtain quality results?

* Figure 5: Why is there so sharp steps in the measured discharge curve during low flow? And secondly, did it not snow in winter 2015/2016?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-401, 2017.