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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Quantification of different flow components in a high-altitude glacierized catchment (Dudh Koshi, Nepalese Himalaya)" *by* Louise Mimeau et al.

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This manuscript proposes to quantify the origin of streamflow in a Himalayan basin, using a physically-based snow hydrological model. The underlying research question is interesting for the readership of HESS but I have the following major concern:

In this paper, two definitions of the origin of streamflow are used: A) annual contributions of snow fall, rainfall and ice melt to total runoff, and B) fractions of contributions coming from different areas. Both definitions can answer different questions and both are certainly useful. But my main question is: Is the water partioning and associated water flowpaths reliably enough represented in the used hydrological model to give re-

liable answers under definition A and B? What evidence do you have for such a reliable representation?

Based on the model description, I am not confident that this is the case. Delayed water release by glaciers is e.g. emulated with a deep soil under glaciers (as far as I understand), which does not necessarily give wrong results but the implications should be clearly discussed. Overall, the paper does not yet convincingly convey that the obtained results reliably represent the dominant hydrological processes. The paper validates snow and glacier mass balance simulations but no evidence is provided for a reliably parameterization of water partioning and release from the subsoil.

Detailed comments:

- Abstract: it is stated that "In general, it is shown that the choice of a given parametrization for the snow and glacier processes has a significant impact on the simulated water balance." Should there not be a more quantitative statement, including why the approach is nevertheless deemed useful to quantify the origin of water? I suggest mentioning all used validation data in the abstract (MODIS, mass balances)

- Introduction: it would be nice to better say why it is interesting to know the proportion of snow / ice melt and rainfall. One reason is that this can give insights into how much water is seasonally delayed and that this delay might change in the future. Another reason is that snow melt / ice melt might have a completely different hydrological pathway (in particular in terms of groundwater recharge) than rainfall. This might e.g. cause a shift in the overall water balance if the ratio snowfall to rainfall changes (Berghuijs Woods, Nature Climate Change, 2015). Another interesting question is how much water is currently available that has been accumulated long time ago in the glaciers.

- How does the model handle transpiration by vegetation? The loss via transpiration should be accounted for in the equations 4- 7 to quantify runoff production

- Results on winter flows controlled by release from the englacial water storage: what

are the similar results in the literature? What provides confidence that the model parameterization is reliable?

- Throughout the paper: what is net rainfall? There is no generally accepted definition.

- The cited observed geodetic mass balances have a very wide range of uncertainty and stem from different areas / different time periods. It is unclear why they are nevertheless useful for validating the modelling results. This should be justified. If the geodetic estimates are from a completely different period (period is not given), this might be questionable.

- Figure 10: I do not clearly see which model version is the best; in terms of RMSE, v3 might (slightly) outperform v0. What about the bias? Is it a good or a bad thing that v0 has less variability of the point mass balances than v3?

- Gauging curve uncertainty: what is the design of the gauge? Does the cross-section move? Is the uncertainty estimate not far too conservative? Please provide more details than "A 15

- General comment on conclusion: I strongly suggest to separate the discussion from the conclusion, it is very unusual to discuss results in the conclusion section

- Conclusion: can you really affirm that the model has an improved parameterization of the storage and transport of melt water within glaciers, or is the modified model just emulating it with the selected parameters? - Conclusion: instead of just stating that "The albedo parametrization (..) enabled to simulate the snow cover spatial distribution and the glacier mass balances more accurately", would be useful to refer to the validation data used

- Conclusion: "water is withdrawn every year from the catchment through ice melt"; strange formulation, difficult to understand; better something like "part of the streamflow leaving the catchment results from negative glacier mass balance changes".

- Conclusion: "Thus, if the precipitation regime (in terms of both intensity and phase)

does not change within the next decades, the access to water resources is likely to be reduced, especially during the fall and the winter seasons, as the glaciers outflow will decrease due to glaciers shrinkage, even without taking into account climate warming." This sentence should be deleted, it is pure guessing and perhaps wrong. Continued glacier retreat means continued negative mass balances, means water input in addition to annual precipitation. The moment of peak water remains to be determined.

- I am not a specialist in debris covered glaciers but I think that there should be some more literature review on how important a good representation of debris cover in glacio-hydrological models is, especially in the Himalaya

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