

Interactive comment on “Tradeoffs for the implementation of a process-based catchment model in a poorly gauged, highly glacierized Himalayan headwater” by M. Konz et al.

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The paper describes an experience of rainfall-runoff modelling with regard to a Himalayan catchment. Applications of hydrological models in this mountain region are not common. For this reason, I think the paper treats a topic that is potentially interesting. To understand the effect and significance of the different runoff generation mechanisms in poorly known and largely ungauged regions is a relevant issue that deserves to be explored.

The Referees provided many important remarks about the paper. The main point that

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was raised by both the Reviewers is related to the use of a complex model in a poorly gauged catchment. I agree that this is a relevant issue. Recent literature, generally developed in the context of the Prediction in Ungauged Basin project, suggests the use of the "downward" approach when investigating the significance of different runoff generation processes in poorly gauged catchments. For an example of application of the downward approach, please see the contribution by Montanari et al. (2006), and the subsequent corrigendum (2006, see the list of references here below) (I am citing one of my own contributions just to provide an example. It is not absolutely necessary to cite it in the paper).

In the downward approach models of increasing complexity are subsequently applied in order to identify a satisfactory compromise between model complexity and data availability. Indeed, if the number of model parameters is excessively high, equifinality might induce a problem of parameter identifiability, which may translate in an imprecise identification of the relevant runoff generation processes. I believe the authors should provide an evidence that this is not the case in the present analysis. I think that this issue might be resolved by comparing the current results with the output of a simpler approach. But other solutions are possible, such as a sensitivity analysis, and the authors' knowledge of the investigated problems will help them to identify the proper one.

Moreover, I have the following minor remarks.

- 1) Line 21: a fully distributed model is used with a (partially) lumped input. I believe this procedure should be discussed (see my general remark above).
- 2) Line 28: probabilities are expressed for monthly occurrences?
- 3) If I well understood, the coefficients of the downscaling relationships are estimated at the monthly time scale, while the same relationships are applied at the daily time scale. Are monthly coefficients reliable when applied to daily data?

4) The model has 36 parameters. Equifinality is not explored here, but one may argue that it might be a relevant problem (see my major remark above).

I think this study is interesting and therefore I sincerely hope the authors are willing to revise the paper that in my opinion would deserve to be published. However, I think a through revision is necessary. I recommend the authors to address (or discuss in their rebuttal) all the remarks provided by the Referees.

References

L. Montanari, M. Sivapalan, A. Montanari, Investigation of dominant hydrological processes in a tropical catchment in a monsoonal climate via the downward approach, *Hydrol. Earth Syst. Sci.*, 10, 769-782, 2006.

L. Montanari, M. Sivapalan, A. Montanari, Corrigendum to “Investigation of dominant hydrological processes in a tropical catchment in a monsoonal climate via the downward approach” published in *Hydrol. Earth Syst. Sci.*, 10, 769-782, 2006, *Hydrol. Earth Syst. Sci.*, 10, 923-924, 2006

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