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Interactive Comment

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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General comments

This paper addresses the important issue of hydrologic prediction in data-sparse regions. To accomplish this task, the authors extend an existing model (TAC-D) by adding algorithms for simulating glacier hydrology, which are mainly based on well-established process representations as used in other models. The authors also apply some simple approaches for pre-processing input data to account for data gaps. Overall, the manuscript blends existing concepts and models, but does not introduce significant innovations.

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My main criticism of the manuscript is that, while the authors have demonstrated that the modified TAC-D model can be run in a Himalayan catchment, there is little attention to evaluating how much accuracy is gained relative to alternative approaches or models. In addition, despite the "process-based" modelling approach, there is relatively little attention to whether some of the process descriptions are appropriate (e.g., in relation to effects of frozen ground, glacial water routing). To warrant publication in HESS, the authors need to demonstrate that this application of TAC-D provides significant improvements and thus constitutes a contribution to hydrologic knowledge and/or practice.

Specific comments

1. The title is somewhat misleading, as the manuscript does not really focus on trade-offs (i.e., alternatives are not tested). I suggest that the title be shortened to "Implementation ...."

2. The model efficiencies are somewhat lower than those typically found in snow- and glacier-dominated catchments. For example, Micovic and Quick (1999, J. Hydrol. 226:101-120) reported consistently higher model efficiencies for an application of the UBC Watershed Model to the Astore River catchment in the Indus basin, which has 20 percent glacier cover. The authors should compare the performance of the modified TAC-D to other model applications in high-mountain environments, particularly the Himalayas, to provide a broader context for assessing TAC-D.

3. Model performance statistics are important, but visual inspection of hydrographs provides the strongest basis for evaluating model performance and diagnosing prediction errors. I recommend the authors provide hydrographs for all simulation years, not just the three shown in Figures 5, 6 and 8.

4. The authors note that the TAC-D runoff algorithm was able to model winter baseflow without resorting to the inelegant approach of adding a constant discharge component, as was the case for an earlier application of an HBV model. However, how did the

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simulations compare otherwise? Did TAC-D provide superior simulation during the warm season?

5. How reliable are the discharge records? The hydrographs in Figures 5, 6 and 8 include abrupt jumps and drops. Could these be due to problems with the stage recorder, or are they real manifestations of processes such as ice jamming/release as is common in many regions with cold climates?

6. I imagine that seasonally frozen soils should occur at higher elevations in the catchment. How might soil freezing and thawing influence runoff generation? Is there also permafrost in the catchment? If so, how extensive is it, and how would it influence runoff generation?

7. The authors should shorten the discussion of the spatially distributed melt modelling algorithm as they have no basis for evaluating whether or not it provides superior performance compared to the non-distributed version.

Technical corrections

1. The lettering in the figures is too small and is thus difficult to read.

2. Section 7.5 presents new results (Figure 8) that should be moved to the results section.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3473, 2006.