

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1  
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## Comment on hess-2022-51

Jonathan D Mackay (Referee)

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Referee comment on "A snow and glacier hydrological model for large catchments – case study for the Naryn River, Central Asia" by Sarah Rose Shannon et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-51-RC1>, 2022

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A well written and clearly presented manuscript that introduces an updated version of the DECIPHeR hydrological model which includes a simple energy balance model to simulate snow and ice ablation and accumulation. Overall, only a few comments from me which should be easy for the authors to address (see attached PDF with comments). Of these comments, two are slightly more "major" points which I hope the authors can address in their response and manuscript revisions. The first is on the question of novelty. It's not entirely clear from the introduction what the novelty of the work is exactly. In the Conclusions section it states that:

"The motivation for this work was to develop hydrological model that can be used to simulate discharge in very large glaciated and snow-fed catchments, at a high spatial resolutions, whilst maintaining the ability to explore model uncertainty."

So I'm assuming that the novelty is the model itself, but my understanding is that there are already models out there that can be used to do this (I've mentioned some in the attached). Could the authors please spell out what the novelty of the work is in the introduction. If the novelty is the model then I think the manuscript would really benefit from a more explicit explanation of the limitations of current models available and what exactly this model offers to address these. A good starting point might be the review of Van Tiel et al. (<https://wires.onlinelibrary.wiley.com/doi/full/10.1002/wat2.1483>).

The other point regards the application of GLUE and the use of the top 0.5% of model simulations to represent a population of behavioural models. The 0.5% seems arbitrary and, therefore, it's not clear to me what the merit of including these in the analysis is. What do the uncertainty bounds of an arbitrary population of models mean? Could the authors please justify the use of using the top 0.5% of simulations rather than, say, defining a more objective set of "good behaviour" criteria e.g. based on the different

metrics of model performance used in the study.

Please also note the supplement to this comment:

<https://hess.copernicus.org/preprints/hess-2022-51/hess-2022-51-RC1-supplement.pdf>