

Interactive comment on “Climate sensitivity of snow water equivalent and snowmelt runoff in a Himalayan catchment” by Emmy E. Stigter et al.

Anonymous Referee #3

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General: The paper presents an interesting analysis of current and future snow dynamics for the Langtang catchment in the Himalayas (Nepal). The paper is well-written and follows a clear line of argumentation. The approach of including as much as possible local and satellite data has a lot of merit. At the same time, I have major concerns about the methodology as detailed in the following. In my opinion, climate change scenario calculations based on simple (parameterized) snow models are unreliable as they necessarily present an extrapolation beyond the state for which the models have been calibrated. The problem with such simple snow models has been exemplarily shown by Magnusson et al. (2011). In this publication, a physics-based model and a model similar to seNorge are shown to produce similar results for a current climate but very diverging results for climate change scenarios. The data assimilation via Kalman filtering potentially makes the modelling in the presented paper even more vulnerable

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to extrapolation than when using robust standard parameters. This is a major objection I have towards the methodology. What is aggravating the problem described above is that the paper appears to completely ignore a large body of literature, which is based on physics-based snow modelling of climate change impacts. This has already been pointed out by RC1. I do not want to necessarily suggest that a paper based on temperature index modelling of future climate needs to be rejected in all cases. But if such an analysis is retained it needs to show a very careful assessment of potential errors through extrapolation and a discussion and comparison with results obtained with physics-based models. Computational restrictions do no longer prevent physics-based models to be applied to larger areas and for significant climate change studies. A recent example is Marty et al. (2017), which has just appeared in TC and which is a good starting point for the authors to find additional studies, which they need to discuss in context of their analysis. Interestingly, the results of the latter study (for the Alps) qualitatively agree with what the authors find for Langtang and this is a good sign. But this also means that the results are qualitatively not new and quantitatively highly uncertain for the argument presented above. This is my major point about the paper and I otherwise agree with the points raised by RC1. In general, presentation, figures and form of the paper are already at a very advanced state and almost without problems.

References:

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