Reply to Anonymous Referee #3

We thank Anonymous Referee #3 for the thorough review. We provide our reply to the referee suggestion/comments below (in italics)

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.

We appreciate this positive feedback and below we explain our choice for the methodology and we will cover the concerns outlined by Anonymous Referee #3.

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.

We agree that in a perfect world a full energy balance model driven by observational data would be the ideal basis for a climate change impact study. However, the core of our paper is to show how a smart integration of remotely sensed snow cover imagery, data assimilation and a snow model can result in improved spatial estimate of snow water equivalent. We show the benefit of assimilating snow cover and snow depth into a snow model that simulates SWE and snowmelt runoff. This data assimilation approach would not have been computationally feasible in a physically-based model, due to the extensive parameterization and high number of dependent state variables. Previous approaches for snow(melt) modelling in the Himalaya dominantly relied on modelling a melt flux from a snow covered area (lacking information of SWE; e.g. Bookhagen and Burbank, 2010; Immerzeel et al., 2009) or used an inversed melt approach (Wulf et al., 2016) *which only provides information on the maximum SWE for a snow season. This study is novel as both the snow water equivalent and snowmelt runoff of a Himalayan snowpack is explicitly simulated. This novel approach then also allows assessing changes in SWE and snowmelt runoff as result of changes in temperature and precipitation. This study is not intended to be a full-fledged study on climate change impacts as the data set is short and detailed information on changes in temperature and precipitation (patterns) is lacking (as already pointed out by RC1). In addition we are aware of the limitations regarding the use of parameterized snow models for climate change scenarios. Therefore we present climate sensitivity tests, showing the sensitivity of SWE and snowmelt runoff to changes in temperature and precipitation. This gives additional information about the sensitivity of the Himalayan snowpack under a changing climate, without stressing the use of a parameterized snow model for purposes that it is unsuitable for.*

We agree that in particular the title may have given the reviewer the wrong impression about the focus of our study and we will modify the title to: 'Assimilation of snow cover and snow depth in a snow model to estimate snow water equivalent and snowmelt runoff in a Himalayan catchment'. In addition we will make it clearer in the manuscript that it is not a climate change impact study but merely a sensitivity experiment. We will also include a paragraph in the introduction on physics based versus "simple" snow models and we will include the Magnusson et al. (2011) reference.

The data assimilation via Kalman filtering potentially makes the modelling in the presented paper even more vulnerable to extrapolation than when using robust standard parameters. This is a major objection I have towards the methodology.

We agree that the Ensemble Kalman Filter (EnKF) has the potential to increase the vulnerability of a snow model to extrapolation as the model could be parameterized to fit the current climate and not future climate. However, in addition to our previous reply, we believe that this will be limited in this particular case because the posterior parameter distribution shows plausible values for the parameters. Using the EnKF actually provides valuable insight in the parameter and simulation uncertainty, compared to a deterministic simulation. In our case the narrow posterior distribution of parameter values together with plausible values shows the robustness of the parameterized snow model.

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.

We refer to our reply about the focus of our paper and we do not see where reviewer 1 indicated that physical-based snow models are a prerequisite for climate change impact studies, however we agree that a more thorough review regarding the pros and cons of physical-based models and temperature index models would improve the manuscript and provides a better context for our choices. In the revised manuscript a more extensive review will be given in the introduction about different model approaches (physical-based vs temperature index) and the results from previous studies. In the discussion the potential errors arising from non-stationarity and the extrapolation by a parameterized model will be addressed and placed in a proper context.

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.

Indeed there are no longer computational restrictions that prevent the use of physics-based models for larger areas. However, it is the limited data availability in the Himalayas that constrains the use of physics-based models. Physics-based models require extensive input data that is often unavailable in the Himalayas. Therefore it is necessary to use a parameterized snow model (requiring less input data) to perform SWE and snowmelt analysis of a Himalayan snowpack. To the authors' knowledge there is currently no study available that simulates the snowpack spatially distributed with a physics-based model for a Himalayan catchment. This supports our choice for a simpler approach.
We thank the referee for providing this interesting study. It will definitely help us to find more literature and it will enable us to put our results in a better context.

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.

The same qualitative results show the capability and potential of the parameterized snow model to simulate climate sensitivity of SWE and snowmelt runoff. We disagree that these results are not new. To our opinion assimilating snow depth and remotely sensed snow cover into a snow model with parametrizations on melt modelling, albedo decay, avalanching, and snow compaction, and its first time application in a remote Himalayan catchment with the aim to understand the spatial patterns and climate sensitivity of SWE is quite novel.

This is my major point about the paper and I otherwise agree with the points raised by RC1.

See the replies to 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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Bookhagen, B. and Burbank, D. W.: Toward a complete Himalayan hydrological budget: Spatiotemporal distribution of snowmelt and rainfall and their impact on river discharge, J. Geophys. Res. Earth Surf., 115(3), 1–25, doi:10.1029/2009JF001426, 2010.

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