The Cryosphere Discuss.,
doi:10.5194/tc-2016-216-RC1, 2016

The Cryosphere
Discussions
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Interactive comment on "Climate sensitivity of snow water equivalent and snowmelt runoff in a Himalayan catchment" *by* Emmy E. Stigter et al.

Anonymous Referee #1

Received and published: 30 November 2016

This paper presents a study focusing on the modelling of snow accumulation and melting in an Himalayan catchment and the response of this catchment under different climate scenarios in terms of snow water equivalent (SWE) and melt runoff. This study addressed an interesting topic in a region where snow storage is crucial for water supply. The authors use data assimilation (Ensemble Kalman Filter, EnKF) of ground-based and remotely-sensed snow data to determine optimal parameters values in their modelling system. These optimal parameters are then used in climate sensitivity tests.

My main comments about this study concern (i) the data assimilation method, especially the choice of variables to assimilate and the effects of these choices on final results and (ii) the limits of the climate sensitivity tests carried out with the optimized model. These questions need to be clarified prior to publication in TC. They are listed below (General comments) followed by more specific and technical comments.

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Discussion paper

General Comments

1) In the study, the EnKF is used to assimilate snow cover area per elevation band and snow depth at two locations. Four parameters are calibrated using the EnKF. My comments on this method concern (i) the choice and benefit of assimilating punctual snow depth measurements and (ii) the assimilation of MODIS snow cover.

The assimilation of punctual snow depth is associated with high uncertainties due to the very limited representativeness of punctual snow depth measurement in mountainous terrain (e.g. Grünewald and Lehning, 2015). For example, wind-induced snow transport can lead to erosion or accumulation of snow at the location of station. What would be the impact of such event when carrying out data assimilation with EnKF? Were the snow depth measurements assimilated in this paper impacted by such event? The benefit of directly assimilating snow depth measurement is hard to identify throughout the paper. It would be interesting to have results obtained when only snow cover data are assimilated. In the present version of the manuscript, the advantage of simultaneous assimilation of snow cover and depth is not clear enough. Results in Section 3.3.1 and 3.3.2 could be presented (i) without assimilation, (ii) with assimilation of snow cover only and finally (iii) with simultaneous assimilation of snow cover and depth.

The assimilation of MODIS snow cover requires an observation operator to convert SeNorge output into simulated snow cover extent. Are the authors using a simple threshold value of SWE or snow depth to determine the presence or the absence of snow? Or are they using depletion curves? MODIS snow cover are averaged per elevation band prior to assimilation. Can the author justify this choice? Indeed, averaging the information per elevation band reduce the information content brought by MODIS and remove the intra-band variability resulting from (i) the contrast between north-facing and south-facing slopes and (ii) the heterogeneous spatial distribution of precipitation.

2) The authors used the optimized version of their model to carry out climate sensitivity

tests. They use the delta method and applied changes in temperature and precipitation for different climate scenarios (Table 3). The authors do not discuss the uncertainties associated with this method. Such discussion is really relevant in a paper dealing with climate sensitivity. The delta method assumes constant changes in space and time for temperature and precipitation. How relevant is this assumption for this region? - Are the changes on temperature and precipitation expected to depend on the season? What are the expected effects for the hydrological cycle in this region? - The authors use the monthly precipitation pattern of Collier and Immerzeel (2015) to spatially distribute precipitation, both in present and future climate. The authors should discuss the validity of this assumption of constant monthly spatial pattern under future climate.

The study period (Jan. 2013 to Sep. 2014) should be compared to the present climatology of the catchment for temperature and precipitation. Is this period considered as cold or warm and wet or dry? Is it representative of the averaged current climate conditions in the Langtang catchment? The author apply the delta method to a short time period (from a climate perspective) and this short time period must be better characterized.

In section 3.5 at P 13 L1, L 13-14 and L 17-18, they authors discuss how the SWE and changes in SWE depend on elevation. This discussion is supported by Figures 7 and 8 that provide maps of SWE for the study period and change of SWE in the different climate sensitivity tests. I recommend the authors to provide complementary figures showing these variables as a function of elevation. It would help the reader to clearly identify the influence of elevation.

Specific comments

Introduction: the introduction is rather short and only presents earlier studies carried out in the Himalayan region. I recommend the authors to write more general paragraphs on (i) data assimilation of ground-based and remotely-sensed snow data in snowpack model and (ii) distributed snowpack modelling applied in mountainous region to simulate the cryospheric and hydrological response of mountain catchments under present and future climate. They should present in this introduction how techniques developed in other mountainous regions could be applied to an Himalayan catchment.

P 4 L 16-17: the description of the location of the snow depth measurements is confusing. Are the 4 sites measuring snow depth located along the 2 transects? Figure 1 suggests that this is not the case. The authors should clarify this point.

P 4 L 28: which uncertainties are taken into account with the correction factor *precip*? Does it include: - uncertainties in solid precipitation measurements at the station due to wind undercatch? - spatial and temporal representativeness across the catchment of the precipitation measured at the station?

P 6 L 3-4: please mention that in Brock et al. (2000) the snow albedo remains constant when the maximum air temperature is below 0 °C.

P 6 L 22: the sentence "Separate transport ... this study" should be reformulated. It suggests than when wet snow avalanches occur the ice and liquid phases are transported separately. This is not the case in the nature. It seems that the authors mentions this point only because seNorge treats separately the solid and the liquid phase in the snowpack.

P 7 L 8: the runs used for the sensitivity analysis are not clearly described. For each run, are the authors using the model to simulate the evolution of snow cover and SWE over the whole study period (January 2013- September 2014) and the whole catchment? Or are they using different time period and sub-domains?

P 7 L 10: how are computed the mean snow cover extent and snow depth? Are they averaged over the whole period and the whole domain? This point is similar to my previous point regarding the characteristics of the simulations used in the sensitivity analysis.

P 7 L 10 (and in the rest of the paper): the author should precise how they compute the snow cover extent from the output of seNorge. Cf my general comments about the observation operator.

P 8 L 22-23: how is modified the maximum air temperature in the climate sensitivity tests?

P 10 L 19-25: this paragraph should also discuss model results in the elevations zones above 5000 m. For example, could the author discuss the differences between summer 2013 and 2014 in terms of snow extent in the elevation zones 5000-5000 m and >5500 m? What can explain the underestimation of SCE in these zones for summer 2013 whereas better results are achieved in summer 2014?

P 10 L 29: differences in classification accuracy with and without calibration are hard to identify on Figure 4. A map of differences of classification accuracy could help the reader to better identity the regions where large differences are found between the two simulations.

P 10 L 30-31: the authors associate the low classification accuracies in the northern part of the catchment with model errors due the avalanching parametrization. However, it seems that this difference can also arise from errors in the meteorological forcing used to drive seNorge. For example: (i) errors in precipitation phase and amount, (ii) errors in the spatial distribution of precipitation. Indeed, the spatial distribution of precipitation is based on monthly precipitation patterns derived from Collier and Immerzeel (2015). For a given precipitation event, the spatial distribution of precipitation can vary from the monthly pattern from Collier and Immerzeel (2015) and strongly affect the snow cover. Please add a discussion about the different potential sources of error.

P 11 L 23-24: please consider reformulating the last sentence of this paragraph. Indeed, the improvement for Kyangjin in 2014 is not really clear.

P 11 L 25: the authors point out the lack of independent stations for the evaluation of snow depth and SWE. Are glacier mass balance data available for a glacier in this catchment to bring complementary values for evaluation? For example, winter mass balance data can provide interesting evaluation on the cumulated precipitation during the winter.

P 11 L32: the absence of underestimation or overestimation concerns snow depth and not SWE.

P 12 L 5-30, Section 3.4: This section does not contain new and original results and only presents the effect of well-established parametrizations introduced in seNorge to improve the snowpack dynamics without comparison with measurements. I recommend the authors to remove the discussion concerning the snow compaction and the snow albedo since it does not bring additional value to their paper. Concerning the avalanche parametrization, the discussion at lines 7-10 (P 12) suggests that avalanching strongly affects the simulation results. It would be really interesting if the authors could illustrate how the avalanching parametrization improves the representation of the snow depth distribution in the model. Figure 7 shows that, in the simulations, snow accumulates at the bottom of the steep slopes of the catchment. Are these zones of additional snow accumulation identified on the LandSat images at 30-m resolution? Such discussion on avalanche processes and a comparison with remotely-sensed observation would substantially improve the quality of this section on snow processes. Otherwise, I recommend to remove this section from the paper.

Technical comments

Text

P 16 L 25: modify the reference to Immerzeel et al. (2014)

References (not included in the submitted manuscript)

Grünewald, T., Lehning, M. (2015). Are flatâĂŘfield snow depth measurements representative? A comparison of selected index sites with areal snow depth measurements at the small catchment scale. Hydrological Processes, 29(7), 1717-1728.

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