View Letter

Dear Editor and reviewers:
Many thanks for the review comments that we received with respect to our paper. They have contributed to considerably improving the quality of the manuscript. We have carefully addressed the reviewers' comments and suggestions. Typos will be corrected. Below are our point-by-point responses to each of the reviewer's comments in blue.

COMMENTS FROM EDITORS AND REVIEWERS

Interactive comment on **"Projection of future glacier and runoff change in Himalayan headwater Beas basin by using a coupled glacier and hydrological model"** *by* **Lu Li et al.**

Anonymous Referee #2

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The manuscript by Li et al. investigates the impact of climate change on glacier melt contribution to discharge in a medium-sized catchment in the Indus basin. To this end, a calibrated glacio-hydrological model was driven by statistically downscaled climate projections from one GCM under two GHG concentration scenarios. The simulations build on ensemble projections of glacier extent derived from a previous study by Lutz et al. (2016) who have already provided a more comprehensive assessment for the entire Indus basin. The manuscript mainly reports on model application in a particular basin and generally lacks novelty.

- Reply: Thanks for your comments. Although there have been studies (i.e. Lutz et al. 2016; Li et al., 2016), looked at the hydrological projections in Indus river basin under climate change, but there is no common conclusion from the studies about the water future of western Himalaya region. Both studies reveal that more studies need to be done in the region.
  Furthermore, in Lutz et al. (2016), they used a corrected precipitation dataset (which is based on 0.25 degree gridded dataset APHRODITE) for historical period, because it was found that there is underestimation of precipitation at high-altitude area in Himalaya region (Immerzeel et al., 2015). In our study, we used a high-resolution (which is 3 km) dynamical regional climate model (WRF) precipitation data for correcting the underestimated precipitation (Li et al., 2017). Moreover, we will add two more ensembles of four GCMs the same as in glacier projections of Lutz et al. (2016) for a more comprehensive comparison and uncertainty investigation for the future water cycle and availability in this Himalaya headwater Beas river basin. In the results, the uncertainty partition of hydrological projection from GCMs and statistical downscaling methods will be analyzed. We agree that the structure of our earlier version of the manuscript is not clear enough and made some confusion. We will sort out the whole structure of the manuscript. The combined WRF and gauge data will be moved to the first part of the method and results. We will re-calibrate the model based on the new corrected precipitation. Then we will do the other analysis and future scenarios based on that.

The glacio-hydrological modeling capitalizes on projections of future glacier extent from

Lutz et al. (2016). Data derived from the Lutz et al. study should be moved to Materials and Methods and should be separated more clearly from the GSM-WASMOD modeling results obtained in the current study. This concerns section 4.3 including figures 10 and 11.

- Reply: Thanks for your comments. Yes, we will correct re-structure the manuscript of Data, methods and Results according to the comment.

The manuscript has a poor structure and is more often than not hard to follow. For example, modeling results are presented and superficially discussed in “Results and discussion” which is however followed by a “Discussions” section that in fact introduces a completely new modeling experiment including data, methods, results and discussion. The additional material addresses the issue of uncertainty in precipitation data in high altitudes. This topic is without question relevant for hydrological modeling in the study region, however falls largely out of the scope of manuscript. In the remainder part (section 5.2) this topic is further discussed while a critical discussion of the main results presented in sections 4.2 - 4.4 is largely missing.

It is only mentioned by the end of the results section that only one GCM was down- scaled to drive the glacio-hydrological simulations while all previous sections give the impression that a GCM ensemble was used. A plethora of previous studies has shown that GCMs contribute a large share to total uncertainty in simulated hydrological impact and it is consequently common practice to drive (an ensemble of) impact models with a GCM ensemble. In this regard, the study clearly falls behind the state of the art and the material does not support significant conclusions.

The manuscript contains a large amount of figures and tables, 21 in total, of which some seem redundant and the authors should make an effort to streamline the mate- rial. For example, Table 4 listing all possible combinations of GCM, RCP and method of bias correction is largely identical in content to Table 2.

- Reply: Thanks for the comments. We agree and accept all of them. We feel sorry that failed to present our work well in the original version. As we mentioned above, we agree that the structure of the manuscript is not clear enough and made some confusion. We will re-structure the whole manuscript. Furthermore, we will add two ensembles of four GCMs the same as in glacier projections (lutz et al. 2016) for a more comprehensive comparison and uncertainty investigation for the future water cycle and availability in this Himalaya headwater Beas river basin.
  The rewritten manuscript will follow those three main questions: “(1) How much uncertainty is in the precipitation over the ungauged high-altitude in Beas river basin? (2) How will the future water availability change due to higher glacier melt under warmer future in Beas river basin over the Himalaya region? (3) What are the uncertainties of the future water from GCMs or Statistical downscaling methods? To answer these questions, a combined precipitation from a high-resolution regional climate model, i.e. WRF and gauge data is investigated and used for the hydrological simulation as the historical baseline.
  In the study, we use a glacio-hydrological model together with two ensembles of four GCMs, i.e. under two generation of scenarios of RCP 4.5 and RCP 8.5, and two statistical downscaling (SD) methods. We firstly focus on the simulation of the present day water cycle and validation of the simulated discharge by using the observed discharge. The uncertainties of the precipitation over high-altitude area and

hydrological simulation are discussed. Besides, the future climate change and glacier extent change and hydrological changes will be investigated. At last, the uncertainty from GCMs and statistical downscaling methods will be analyzed and discussed before presenting the main conclusions."

In this revised manuscript, we will remove Fig 3, Fig.5 and Table 2. We also will split section 5.1 and fill into three part: 1) section 2.2 Data, 2) section 3.1 of precipitation data correction by WRF and 3) section 4.1 results of model calibration and validation.

The standard of English needs to be improved throughout the manuscript. While the meaning is usually (but not always) clear, there are a lot of grammatical errors (far too

many to list) and diction is often poor.

- Reply: Thanks for your comments. We will carefully check the typo and grammatical errors through the whole revised manuscript and a native speaker colleague will also correct it.

Specific comments

L. 11: Why would the glacier melt lead to extreme rainfall?

- Reply: Thanks for the comment. That is obviously a mistake. We have corrected it: "The changes in glacier melt may lead to droughts as well as extreme floods in the Himalaya basins, which are vulnerable to the hydrological impacts."

L. 13: I strongly disagree with the use of the term RCM when referring to the two methods of GCM bias correction/downscaling applied in this study. The term RCM describes numerical prediction models.

- Reply: Thanks for the comment. We changed them all in the revised manuscript to be "statistical downscaling methods".

L. 30-32: Colloquial, please rephrase.

L. 36: Please correct to "CMIP5"

L. 67: Correct to "Mishra 2015"

L. 88: Unclear, please rephrase.

L. 115-117 : This section describes the study basin/region; information on the model and data used should be moved to the corresponding sections.

L. 115: Please correct to "meteorological"

L. 130: Was the GSM module developed in the scope of this study? If not, please add the reference to the original publication.

- Reply: Thanks for the careful review. All above corrections/comments from L.30 - L.130, will be corrected in the revised manuscript.

L. 148: What was the reason for choosing a modeling resolution of 10 km? Most of the input data sets do seem to support a higher modeling resolution; please clarify.

- Reply: Thanks for the comments. The reason for using 10*10 km resolution in our original manuscript was because in our study, we have only 7 rain gauge stations and 4 meteorological stations (for temperature and relative humidity). According to the previous studies and analysis of the influence of interpolation and station density on gridded daily data (Dirksa et al. 1998; Hofstra et al., 2010; Xu et al., 2013), the results showed that the network density could introduce biases in the mean and variance of the grid values (i.e. precipitation and temperature) compared to those expected for the true area-averages. Especially, it was found by Hiofstra et al. (2010) that both the mean and variance of daily precipitation and temperature are reduced through interpolation unless the network density is extremely high. In this case, we chose 10 km in the study in order to balance the low gauge density of measurements and the resolution of hydrological modeling, which can be fairly well for the simulation.

  Following reviewer's suggestion, we are now running the model with 3km resolution and ing the results. If the higher resolution of 3*3 km model run will yield better results although with limited gauge data, we will replace the old results.

- Dirks, K.N., Hay, J.E., Stow, C.D. and Harris, D., 1998. High-resolution studies of rainfall on Norfolk Island: Part II: Interpolation of rainfall data. *Journal of Hydrology*, *208*(3-4), pp.187-193.
- Hofstra, N., New, M. & McSweeney, C. Clim Dyn (2010) 35: 841. https://doi.org/10.1007/s00382-009-0698-1
- Xu, H., Xu, C.Y., Chen, H., Zhang, Z. and Li, L., 2013. Assessing the influence of rain gauge density and distribution on hydrological model performance in a humid region of China. *Journal of Hydrology*, *505*, pp.1-12.

L. 149: It was mentioned earlier that potential evaporation was only available from one station. Were these station values used for the entire basin? Please clarify.

- Reply: The data of pan-evaporation is only available at one station from 1996-2011. In this case, we used combined potential evaporation, which is calculated by the relative humidity (1990-2004) from four meteorological stations and one-year data (2005) from this one pan-evaporation station (which has bias corrected and uniform with previous potential evaporation data). We have added this clarification in the Data section of the revised manuscript.

L. 155-156: Unclear, please rephrase.

- We will add more explanation here in the revised manuscript: "The routing method in GSM-WASMOD is called NFR routing algorithm (Gong et al. 2009, 2010), which was developed to adapt to the coarse resolution hydrological modeling. This is a scale-independent routing method for network-response function using high-resolution aggregated hydrographgy HYDRO1k. The algorithm preserves the spatially distributed time-delay information in the form of simple network-response functions for any low-resolution grid cell in a large-scale hydrological model."

Section 3.4: 1) The authors miss to describe and reference the 21st century GCM ensemble data used in the study. Please add a section or paragraph.

- Thanks for the comment. We used only one GCM data here and we will add the reference in the section 2.2 of Data: "The version 1.1 of the Beijing Climate Center Climate System Model (BCC_CSM1.1) developed at the Beijing Climate Center (BCC), China Meteorological Administration (CMA) (Wu et al., 2013) is chosen as the GCM model for use in regional statistical downscaling future simulations." Furthermore, we will add two ensembles of four GCMs (lutz et al. 2016) for comparison in the revised manuscript and more clarification will be added in the Data section.

2) Lutz et al. (2016) applied the same GCM ensemble but a different downscaling approach to simulate the future glacier extent used in this study. Why did the authors choose a different downscaling technique? Given that the downscaling technique is found to have a profound effect on projected precipitation and temperature (which drive both the simulated glacier extent and melt), how does this inconsistency affect the results for the Beas river basin and the conclusions drawn?

- Reply: Thanks for the comment. We agree that the different SD methods may result in inconsistency in the simulations. In the revision, we will add the statistical downscaling method: Advanced Delta Change (ADC) in the study of lutz et al. (2016) for keeping the consistency. It is proved that different downscaling methods can cause uncertainty in future precipitation and temperature simulation. In this case, we chose two different statistical downscaling methods in the original study, which are popular and widely used in the world to simulate the different future climate changes in our study. Both of the downscaling methods are regression-based downscaling technique, which belongs to the perfect prognosis (or change factor). These methods construct the regression relationship between the historical large-scale atmospheric factors and site-specific climatic variables (i.e. precipitation and temperature) and then use this relationship to generate future variables. As we mentioned in the earlier reply, we are now doing more comprehensive validation of those two statistical downscaling methods, and if one method is found to be significantly better than another, we will use the better one. We will also analysis the uncertainty from different SD methods. In this case, we can better answer the question here that you concerned about.

4) Sections 3.4.1 and 3.4.2 need to be rewritten to enhance comprehensibility. In the current version, it is impossible to understand how both downscaling approaches work.

- Thanks for the comment. We will rewrite this part and make it more understandable in the revised manuscript.

L. 209-215: "SSVM is directly used to construct the relationship between hydrological data and atmospheric variables" and "The calibration of downscaling models used the station-scale hydrological data and GCM historical atmospheric variables to construct the relationship": I understood from the earlier text that both techniques were used the downscale GCM simulated atmospheric variables to station-scale meteorological data which subsequently were used to drive the glacio-hydrological simulation. Did the authors establish a direct statistical relationship between atmospheric variables and hydrological fluxes? Please clarify.

- Reply: Thanks for the comment. There is no direct statistical relationship between atmosphere and hydrological fluxes in the method of the study. A direct statistical regression relationship is just established between large-scale atmospheric factors and site-specific climatic variables. The introduction of downscaling method can be found in the previous answer to 'Section 3.4: 2) Lutz et al. (2016) applied…'. Then the generated future climatic variables (i.e. precipitation and temperature) are used as the input of GSM-WASMOD to simulate the future hydrological fluxes. We will add more detailed explanation in the revised manuscript.

Section 3.5: 1) In L. 220, Li et al. 2013a or Li et al. 2013b?

- Thanks for the comment. We have corrected the reference to be Li et al. 2013a.

2) Glacier mass balance data were apparently used for calibration, but this data-set has not been described or mentioned yet. Please add a description to the data section.

- We will add more information about the glacier mass balance data that we used in the section 2.2 of Data: "The annual glacier mass balance data of Chhota Shigri Glacier used in the model calibration are taken from the previous studies of Berthier et al. (2007), Vincent et al. (2013), Azam et al. (2016) and Enghardt et al. (2017)." Besides, we will also add more explain in the section 4.1: "There is an intra-regional variability of individual glacier mass balance in High Mountain Asia (HMA) and less negative mass balance than most other estimates according to the recent study of Brun et al. (2017). From the study, the annual glacier mass balance is -0.49+/-0.2 m w.e.yr-1 in Spiti-Lahaul region (where Chhota Shigri glacier locates) during 2000-2008 based on ASTER and 0.37+/-0.09 m w.e.yr-1 in Western Himalaya region from RGI Inventory during 2000-2016 based on ASTER. Besides, a detailed map of elevation changes during 2000-2011 in Spiti-Lahsul region based on SPOT5 DEM is given in the study of Gardelle et al. (2013), which showed that the changes of the glaciers in the Beas river basin are quite similar as the changes in Chhota Shigri glacier during 2000-2011 in general, although there is variability both in independent glacier and over the region. Furthermore, in our study basin, the glaciological mass balance series published in Spiti-Lahaul region (of HMA) that is available for comparison are the Chhota Shigri glacier and Bara Shigri glacier (Berithier et al. 2007). In which, the only one is long enough to be comparable to our simulation period is the Chhota Shigri glacier (2002-2014), which has also geodetic mass balance for validation (Azam et al. 2016). So we used the mass balance data of Chhota Shigri glacier as a representation for the glaciers in our small basin."

3) The efficiency criteria listed seem to refer to simulated discharge only. How was model efficiency evaluated with respect to glacier mass balance?

- We manually adjusted the parameters of glacier module according to the annual glacier mass balance. The bias is used for evaluation with respect to glacier mass balance. We will add more explanation in the revised manuscript.

4) Were discharge and glacier mass balance calibrated simultaneously?

- We first 'pre-calibrate' all parameters according to the total discharge. Then we manually adjusted the parameters of glacier module according to the glacier mass balance. At last, we set the glacier module parameters and re-calibrate the other parameters according to discharge data one more time. We will add more clearly explanation in the revised manuscript.

L. 242 "worked fine": Colloquial, please rephrase. Further, I cannot see how Fig. 5 adds important new information. If its only purpose was to show that the model "worked fine", the figure can be removed.

- Thanks for the comment. We have removed Figure5.

L. 245: It was mentioned earlier that glacier mass balance data were used to calibrate GSM-WASMOD; are those the same data as used here for validation?

- No, the mass balance data are only used for calibration. We didn't use it for validation of the model performance.

L. 250: Table 4 formally belongs to the methods section and should be referenced

there.

- Thanks for the comment. We will change it.

L. 255-265: The two downscaling methods seem to introduce a large uncertainty with respect to future climate in the region. How does this uncertainty compare to the spread between the different GCMs?

- We will add two ensembles of four GCMs (Lutz et al., 2016) in the revised manuscript and we will be able answer this question. We will add this analysis in the revised manuscript.

L. 294 "It shows that the summer peak of runoff sifts to the other seasons in Beas river basin": Cannot be inferred from the figure.

- Thanks for the careful comment. We have removed it.

L. 300 and following: It is mentioned here for the first time that only GCM was down- scaled to drive the glacio-hydrological model. This should have been made clear in the methods section.

- Thanks for the comment. We will correct it and add clearer contexts in the method part.

Tab. 2: Please rephrase the caption and correct to "glacier evolution"; "Selected model" in the table heading is rather ambiguous and could be replaced by "GCMs"

Table 3: Please correct to "validation", "Nash-Sutcliffe coefficient" and "NS_d" (row 6); typographical error in the last row; missing space before table number.

Table 5: Please provide a more informative caption. I assume ensemble median and range are show. "Change" should be spelled lower case. Does the table show changes over the glacierized area or for the entire river basin?

Fig. 2: In the legend, please correct to "Simulated dis"

Fig. 3: Please add the observed discharge for reference

Fig. 4: Please correct to "Monthly hydrographs". The quality of the Figure should be improved.

Fig. 6: The observed data shown seem to be mean values over certain time periods rather than estimates for a single year (e.g. 1999–2004 in Vincent et al. 2013), but are depicted as points in the figure which is misleading. Please correct. Further, please add a table listing all external glacier MB data including reference period and estimation method.

Fig. 7+8+9: I strongly disagree with the use of the term "RCM" when the authors actually refer to bias correction methods, please correct. Please revise the captions. Do the figures show the ensemble mean? If yes, please add the ensemble range.

Fig. 10: Y-axis label should read "Glacier"

Fig. 11: Is this the ensemble mean?

Fig. 12: The figure needs profound revision. 1) I can only guess that the numbers in the legend refer to the index given in Table 4. Listing all ensemble members in the legend is

somewhat obsolete since they are not distinguishable in the plot. 2) The caption claims that results for only one GCM are shown (CANESM2) while the figure apparently shows the whole ensemble. 3) Are both RCPs shown? If yes, please color- code accordingly. 4) In all simulations glacier melt discharge approaches 0 by the end of the century while according to Table 5 glacier cover remains larger than 0. Please explain. 4) Why is glacier-melt discharge given in negative numbers?

Fig. 14: The two subfigures seem to show exactly the same data with respect to the single ensemble members. Please double-check.

- Thank you so much for the careful review and corrections! We will correct them according to the above comments about the Tables and Figures in the revised manuscript. Besides, all the figures and tables will be updated by the new results in the revised manuscript with respect to the adding new GCMs.