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

Review of "Projection of future glacier and runoff change in Himalayan headwater Beas basin by using a coupled glacier and hydrological model", by Li et al., HESS

General comments

Comment #1:

The manuscript addresses a relevant topic: the impact of climate change for hydrology in Asian high mountain catchments, which supply water to the irrigated areas downstream. Although the assessment is relevant and provides new insights on climate change impacts for water resources in Himalayan catchment, I believe that some major revisions are required to make the work publishable. These are specified in the comments below.

- Reply: We thank to the reviewer for your positive evaluation on our work in general and for your professional and constructive comments detailed below.

Comment #2:

Section 3.4 on statistical downscaling requires more elaboration. See also the specific comments below.

It is unclear which precipitation input has been used for the historical period. The paper mention gauge based precipitation, which I assume has been used. In the latter part of the paper the improvement of precipitation forcing using a combination of WRF modelling and gauge data is introduced. It is however unclear if this is used in the study. To me it seems that is was not used although the authors indicate that this method yields better precipitation data. If it was not used, I suggest to redo the modelling using this improved precipitation fields.

- Reply: thank you for the comment and sorry that we failed to describe this part clear enough in the original version. Yes, the gauge rainfall was used as input for the historical period. The combined WRF and gauge precipitation was only evaluated in the experiment analysis part. In this revised manuscript, we will redo the modeling using the combined precipitation for the historical period as baseline. In this case, we will split section 5.1 and fill it into three parts: 1) section 2.2 Data, 2) section 3.1 precipitation data correction by WRF and 3) section 4.1 results of model calibration and validation.

Comment #3:

The model resolution (10x10 km) seems to coarse to me for hydrological modelling of mountainous areas with large variability over short horizontal distances. I think this coarse resolution is problematic for proper simulation of melt process, which are very much dictated by elevation and lapsing of temperature fields. Besides I believe that routing will be problematic at this resolution. Since the authors mention that they have higher resolution precipitation forcing data (3x3 km), my suggestion would be to setup the whole model at that resolution.

- Reply: Thanks for the comments. The reason for using 10*10 km resolution in our original version of the manuscript is the following. In our study, we have only seven rain gauge stations and four meteorological stations (i.e. temperature and relative humidity). According to the previous studies and analysis of the influence of interpolation and station density on gridded daily data (i.e. 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 reasonable for the simulation. Considering the routing, 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. We will add those clarification and citations in the methodology part related to the model resolution chosen and routing method.

  Following reviewer's suggestion, we are now test running the 3km resolution simulation and checking the results. We will replace with the simulation in higher resolution of 3*3 km in the revised manuscript, if it turns out that 3km is truly necessary for getting better result although with limited gauge data.

- 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.
- Gong L., E. Widén-Nilsson, S. Halldin, C.-Y. Xu, 2009. Large-scale runoff

routing with an aggregated network-response function. Journal of Hydrology, Volume 368, Issues 1-4, Pages 237-250, doi: 10.1016/j.jhydrol.2009.02.007. Copyright 2009 by Elsevier, reprinted with permission.

Comment #4:

The two used statistical downscaling techniques yield very different climate projections although they were used to downscale the same GCM. This implies that the quality of the downscaling for at least one of the methods is questionable. Also the sudden jumps in forcing when going from the historical period to the future period are unnatural and would be smooth if the downscaling performed well. My advice is to validate both statistical downscaling methods for a historical period, then use the one that performs best for the remainder of the study, if the performance is sufficient. If the performance of the downscaling method is insufficient, another method should be used. See also the specific comments below

- Reply: Thank you for your advice. We are now doing more comprehensive validation, and if one method is found to be significantly better than another, we will use the better one.

Comment #5:

The paper now has a 'Results and Discussion' section and a 'Discussion' section. This is double and should be restructured as a 'Result and Discussion' section or a separate 'Results' and 'Discussion' section.

- Reply: Thanks for the careful review. We have corrected it and there are two parts, i.e. "Results" and "Discussion".

Comment #6:

There are many textual errors. Please have the whole text reviewed by a native English speaker before submitting the revised manuscript.

- Reply: Thank you for the comments. We will do that.

Specific comments

L14: Be more specific about 'future water change'. Do you mean changes in water availability, water resources, hydrological regimes, or a combination?

- Reply: We meant both water availability and hydrological regimes. We will clarify it in the revision.

L45-46: What is the message of this sentence? I do not understand the relation here between the importance of glacier melt and the increase in precipitation.

- Reply: Thanks. We have corrected the sentence to be "The impact of glacier melt on river flow is noteworthy in the future in the Himalaya region."

L50: You mention 'few studies' but you cite only one. If there are a few, please cite them all.

- Reply: Thank you. Yes, we will add two more citations here (Li et al. 2016; Hasson et al. 2016).
- Li H, Xu C-Y, Beldring S, Tallaksen TM, Jain SK, 2016. Water Resources under Climate Change in Himalayan basins. Water Resources Management 30:843–859. DOI:10.1007/s11269-015-1194-5.
- Hasson, S.U., 2016. Future Water Availability from Hindukush-Karakoram-Himalaya upper Indus Basin under Conflicting Climate Change Scenarios. Climate 2016, Vol. 4, Page 40 4, 40. doi:10.3390/cli4030040

L79: You could add citation to (Palazzi et al., 2013), providing an overview of the variation in precipitation estimates in gridded products.

- Reply: Thanks. We will add the citation in here as: "An overview of the variation in precipitation estimates of gridded products was provided by Palazzi et al. (2013), in which six gridded products are compared with simulation results from a global climate model EC-Earth despite having different resolutions."

L88-90: Is this referring to the current study or to the cited Li et al., 2017 study?

- Reply: This is referring to the current study. We will correct it like this: "… This high-resolution WRF model from Li et al. (2017) provides a first estimation of liquid and solid precipitation in high altitude areas, where satellite and rain gauge networks are not reliable.
  Due to the underestimation of summer precipitation in WRF at the foothill of the basin in its western part, we combined these high-resolution WRF data with gauge data in our study. A solid understanding of the water cycle in the Himalaya headwater basin is interpreted. The comparison of the simulations from gauge precipitation, WRF precipitation and the combined precipitation is further assessed. There is a large uncertainty of winter precipitation over high altitude area in Himalayan river basin. In the study, a glacio-hydrological model has been applied in the Beas river basin in Himalaya for assessing the current and future water resources by statistical downscaling method under climate change scenarios. …"

L95-98: I would expect that you would answer question 3 first, because it also affects the answers to questions 1 and 2.

- Reply: Thanks for the comment. We agree with reviewer's suggestion. We will change the order as suggested and rewrite: "(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? "

In this revised manuscript, we will redo the modeling using the combined precipitation for the historical period as baseline.

L99-105: I would not sum the sections but just describe your approach in 2 or 3 sentences: To answer these questions we use ... and ... etc.

- Reply: We will re-write this part as this: "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, 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."

L107: Mention the percentage of the basin area covered by glaciers.

- Reply: Yes, we have added it: "The study area is Beas river basin, upstream of the Pandoh Dam with a drainage area of 5406 km^2, out of which 780 km^2 (14.4%) is under permanent snow and ice".

L119: Include also the glacier outline data and glacier mass balance data you used in this section. Also mention the future climate data (the one downscaled GCM).

- Reply: Thanks. We will add those information in Data section:
  "The basin boundary in the study is delineated based on HYDRO1k (USGS, 1996a), which is derived from the GTOPO30 30-arc-second global-elevation dataset (USGS, 1996b) and has a spatial resolution of 1 km. HYDRO1k is hydrographically corrected such that local depressions are removed and basin boundaries are consistent with topographic maps. Daily precipitation of 7 rain gauge stations, daily minimum and maximum air temperature of 4 meteorological stations and daily potential evapotranspiration of one station are obtained from Bhakra Beas Management Board (BBMB) in India were used for GSM-WASMOD modelling. The outlet discharge station of Thailout was used for GSM-WASMOD model calibration and evaluation, which was also obtained from the BBMB. Glacier outlines were taken from the recently published Randolph Glacier Inventory (RGI 6.0) (2017) (https://doi.org/10.7265/N5-RGI-60). The annual glacier mass balance data of Chhota Shigri Glacier that are used in the model calibration are taken from the previous studies of Berthier et al. (2007) and Vincent et al. (2013). 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 of future simulations. Furthermore, the daily precipitation from a horizontal 3 km WRF simulation by Li et al. (2017) is also used in the study for further experiment and discussion on the precipitation uncertainty."

L120: Add a citation for the Hydro1k dataset

- Reply: Yes. We have added it. Please see the answer above.

L123: Also show the locations of the stations where temperature and potential evapotranspiration is measured in Figure 1. Are you sure that potential evapotranspiration is measured there, or should this be actual evapotranspiration?

- Reply: Yes, we have added those 4 meteorolgical stations. Please see the new Figure 1 in the reply to L243-246. It is potential evaporation from Pan evaporation.

L133: Is there a specific reason you used the GLIMS dataset and not the more recent Randolph Glacier Inventory (Arendt and 87 others, 2015)? Did you do any quality control of the GLIMS data over your basin? Given that your basin is not that large, it may be worthwhile to do your own mapping of glacier outlines using remote sensing data, if the quality of GLIMS or RGI are insufficient over your basin.

- Reply: Thanks for your comment. We have downloaded the RGI 6.0 and compared it with the data from GLMS. The two glacier shape files have a slight difference but the glacier covered grids are identified the same as that from GLMS. So in this case, it didn't impact the simulation results and conclusions at the end. In the revised manuscript, we have updated the glacier outlines data to be Randolph Glacier Inventory 6.0 in section 3.1: "Those glacier grid cells were defined by ESRI ArcGIS system v. 9.0 (or higher) and set up before modeling based on the glacier outlines from the RGI (6.0) (2017) (https://doi.org/10.7265/N5-RGI-60) (Berthier, 2006; Raup et al., 2007)." Please also see the answer above to the comment of L119.

L137: What is the assumption of 20% based on?

- Reply: This is an empirical estimate. We will clarify it in the revision.

L142: How where these percentages for adjustments of the DDFs obtained? Has debris cover been considered in your modelling? If debris cover is present on glaciers in your basin, this will have very different melt properties (e.g. Vincent et al. 2016).

- Reply: This is an empirical estimate. No, the debris cover is not considered in the modeling right now. We will clarify it in the revision.

L130-144: This section requires more elaboration of the description of the GSM. Did you calculate for different elevation zones and use vertical temperature lapse rate? Or is the same elevation, temperature and precipitation used for all glaciers? If this module was used before, please provide a reference. Otherwise it will be better to write out the equations listed in Table 1 in the main text and complete describe the model.

- Reply: The GSM is calculated based on grids. In each grid cell of glacier, the input data (including elevation, temperature and precipitation) are the same. The temperature and precipitation are interpolated from stations by IDW method and the vertical temperature lapse rate is considered in the IDW method for temperature. This GSM has been used before. We added the citation here (Li et al. 2014; England et al. 2012) and have added more description in method section of GSM:

  "A conceptual glacier- and snow- melt module (GSM) (Li et al. 2014; England et al. 2012) was used to compute glacier mass balances and melt-water runoff from the glacier in the study basin, which was only applied to the grid cells of the glacier-covered area. Those glacier grid cells were defined by ESRI ArcGIS system v. 9.0 (or higher) and set up before modeling based on the glacier outlines from the RGI (6.0) (2017) (https://doi.org/10.7265/N5-RGI-60) (Berthier, 2006; Raup et al., 2007). The gridded temperature and precipitation are interpolated based on the station data by Inverse Distance Weighted (IDW) method, in which the vertical temperature lapse rate of −6 °C km−1 is used to downscale the temperature station to the elevations of the grid cells (Kattel et al., 2013). The daily gridded temperature and precipitation were input data for the GSM module, which calculated both snow accumulation and melt-water runoff."

L148: A spatial resolution of 10 x 10 km sounds very coarse to me considering the size of the basin. I don't think you can get a sufficient representation of the changes in meteorological variables, which vary strongly over short distances in the mountains. Besides, this resolution is probably problematic to do a proper routing.

Reply: Thanks for the professional comments. Our response was presented to the General Comment #3, above. In which we have written, among others, "Following reviewer's suggestion, we are now test running the 3km resolution simulation and checking the results. We will replace with the simulation in higher resolution of 3*3 km in the revised manuscript, if it turns out that 3km is truly necessary for getting better result although with limited gauge data."

L150: Simply interpolation air temperature horizontally will not be sufficient for terrain with strong vertical differences. I advise to use a vertical temperature lapse rate to downscale the temperature field to the elevations of your grid cells.

- Reply: Thanks for your comment. This is actually what we did in the IDW interpolation for gridded temperature, but we didn't explain it in detail and clear enough. So we have added the information in the methodology section of the revised manuscript: "The gridded temperature and precipitation are interpolated based on the station data by Inverse Distance Weighted (IDW) method, in which the vertical temperature lapse rate of −6 °C km−1 is used to downscale the temperature station to the elevations of the grid cells (Kattel et al., 2013)."

L164: Include reference to the paper that describes the glacier change parameterization (Lutz et al., 2013).

- Reply: We have added it: "The glacier changes are the result of a close interplay of projected changes in temperature and precipitation, which are calculated monthly in the parameterization approach (Lutz et al., 2013)."

L181: I do not understand the acronym MLR for linear regression. Where does the 'M' stand for?

- Reply: Thanks for the careful review. 'M' stands for 'multiple and MLR means the multiple linear regression. We will correct it in the revised manuscript.

L185: the variables 'u', 'w' and 'W' need to be described. Otherwise this part is completely unclear. A few lines of description in addition to the two equations would also be useful.

- Reply: Thank you for the comment. Now we have added the corresponding description in the 3.4.1 section: "$\hat{u}_t^i$ is the i th corresponding climatic predictor on the t th day; W_t^{sim} is the SDSM-simulated probability on the t th day; w_t^{sim} is the simulated precipitation state on the t th day."

L188: I would not state 'superior ability of simulation' for a method that does well in transforming changes in the mean, but not the standard deviation and extremes, as stated in L181-182.

- Reply: Yes, agree. We will correct it to be "One of the advantages of SDSM is the visual, user-friendly interface that does not exist in most of the downscaling models."

L195 'l' and 'i' need to be explained.

- Reply: Thanks. 'l' is the upper limits of the numbers of the sets{X,Y}.'i' is an ordinal number for the vector X and Y. We will correct it in the revised manuscript.

L198: 'T' needs to be explained

- Reply: Thanks. 'T' means the transposition in the calculation. We have added it in the revised manuscript.

L199: What is meant by 'the parameters'. What kind of parameters?

- Reply: Thanks for the careful review. 'the parameters' means in this equation, both 'W' and 'b' can be adjusted to make the equation balanced. We have added the explanation in the revised manuscript: "where W and b are the parameters which determine the shape of hyperplanes Y"

L201: subscript 'j' needs to be explained

- Reply: Thanks. 'j' is also an ordinal number like, which makes X_i and X_j independent to each other. We have corrected equation (4) to make it clearer: "$K\left(X_i, X_j\right) = F\left(\phi(X_j), \phi(X_i)\right) \qquad \left(i, j \in Z, 1 \le i \le l, 1 \le j \le l\right)$"

L213: Should 'station-scale hydrological data' be 'station-scale meteorological data'? I cannot imagine that the downscaling is done with hydrological data.

- Reply: Yes, thanks for the carefully review. We have corrected it.

L213-215: Which GCMs are used?

- Reply: We changed it to be: "The calibrated downscaling models are then utilized to predict the future climate change with the BCC-CSM1.1 variables from 2006 to 2099 in the RCP4.5 and RCP8.5 scenarios".

L221-222: Do I understand correctly that the observed glacier mass balance at one glacier was used for calibration? Can you elaborate on the assumption that this one glacier is representative for all glaciers in the catchment? You could also compare with remote sensing data (Brun et al., 2017; Gardelle et al., 2013) to see how large the spatial variation in glacier mass balance is in your catchment, to say something about the representativeness of Chhota Shigri.

- Reply: Thanks for the references and comment. In our study area, the glaciological mass balance series published in Spiti-Lahaul region (where Beas river basin locates) 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 geodetic mass balance for validation (Azam et al. 2016). We have compared the mass balance data from previous studies for the Chhota Shigri glacier. In the study of Gardelle et al. (2013), a detailed map of elevation changes during 2000-2011 in Spiti-Lahsul region based on SPOT5 DEM is given, which showed that the changes of the glaciers in the Beas river basin are quite similar as the change in Chhota Shigri glacier in general, although there is variability both in individual glacier and over the region. So we used the mass balance data of Chhota Shigri glcier for representing all the glaciers in our small basin.
  We have added this explanation in the section of Model calibration: "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 glcier as a representant of glaciers in our study basin."

L233: The biases are seem to be large for June-August because of the common problem of underestimated high-altitude precipitation in gauge-based data. If you did not use the improved precipitation fields based on WRF which you discuss in section 5.1, I believe you should include a correction for that in your model. It could be an additional parameter that you calibrate in advance, to make sure that the precipitation input is at least higher than the observed discharge. Have a look at for example (Dahri et al., 2016; Immerzeel et al., 2015). I am not saying you should use an approach as in the cited studies, but at least do a correction on the precipitation input to make it more realistic.

- Reply: Thank you very much for the suggestion. We will use the combined precipitation (from both WRF and Gauge) for historical baseline simulation. We will update it in the revised manuscript of both methodology and results.

L236: Fig 2 and 3 are much repetition and can be combined in one figure. How do slow flow and fast flow relate to rain-runoff? I rain-runoff surface runoff and are slow flow and fast flow both groundwater flow and flow through the soil layer?

- Reply: Thanks for the comment. We will remove Fig.3 and add more information in the results part of the revised manuscript: "In Fig 2, the total discharge includes fast-flow and slow-flow from non-glacier area and discharge from glacier area, which includes rainfall discharge, snow-melt and ice-melt discharge. The fast-flow is generally considered to be surface runoff and the slow-flow refers to groundwater plus water through soil layer. "

L241-243: I think it is a bit misleading to show one of the years where the model has best performance in figure 5 and then conclude that the model 'worked fine' in the study basin. You clearly indicated that there are quite large biases, especially during the high flow season, which is understandable when simulating high mountain hydrology. I would remove figure 5.

- Reply: Thanks for the suggestion. We will remove Fig. 5.

L243-246: Similar comment as for L221-222. How representative is the glacier mass balance at Chhota Shigri for your entire catchment? Here you compare the simulated glacier mass balance for the entire catchment to the observed mass balance at one glacier.

- Reply: Thanks for the comment. Please see our reply above for L221-222. In our study area, the only glaciological mass balance series published 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, which has geodetic mass balance for validation (Azam et al. 2016). We have compared the mass balance data from previous studies for the Chhota Shigri glacier. In the study of Gardelle et al. (2013), a detailed map of elevation changes during 2000-2011 in Spiti-Lahsul region based on SPOT5 DEM is given, which showed that the changes of the glaciers in the Beas river basin are quite similar as the change in Chhota Shigri glacier in general, although there is variability both in independent glacier and over the region. So we used the mass balance data of Chhota Shigri glcier for representing all the glaciers in our small basin.
  In order to make it clearer in the manuscript. We will rewrite L236-237 and add the location of Chhota Shigri glacier in Fig. 1 in the revised manuscript: "In our study area, the glaciological mass balance series published in Spiti-Lahaul region (where Beas river basin locates) 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 is the Chhota Shigri glacier, which has geodetic mass balance for validation (Azam et al. 2016). The Chhota Shigri Glacier intersect with the northeast boundary of Beas river basin, which is close to Manali and Bhuter."
  We will update the Fig. 1 in the manuscript as below:

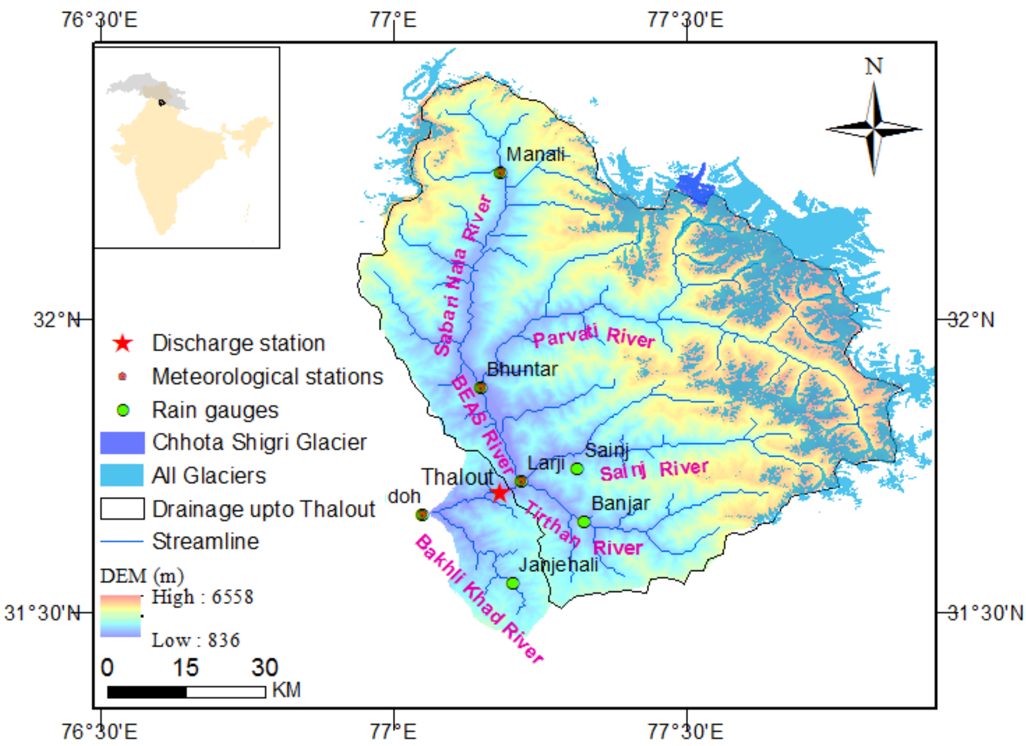

Fig 1. The topography, stream network and glacier cover of Beas river basin up to Pandoh dam with seven rain gauges and Thalout discharge station (The small figure on the upper right corner shows the location of Beas river basin up to Pandoh within Upper Indus Basin (UIB) region and India).

Fig.6: Move the legend outside the plot or draw a clear boundary around it. Now it seems that de symbols in the legend are actual plotted values.

- Reply: Thanks. We have corrected the figure (see the Fig. 2 below) and will update it in the revised manuscript:

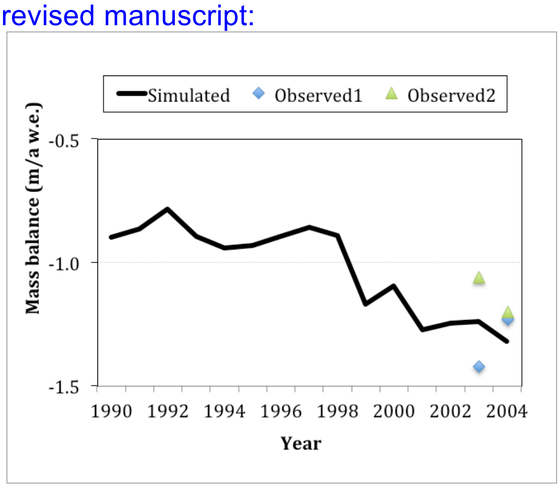

Fig. 2 Simulated and observed (Observed 1 (Azam et al., 2016) and Observed2 (Vincent et al., 2013)) annual glacier mass balance of Beas River basin (mainly from Chhota Shigri) from 1990-2004.

Table 4: I do not understand the line below the headers (0, mean, mean, mean). I

also do not understand why the column indicating the statistical downscaling method is headed 'RCM'. I also do not understand the meaning of the header 'Glacier – GCM'. I also do not understand what the line at the bottom of the table 'CMIP5: Bcc-csm' should indicate.

- Reply: Thanks for the comment. We have changed the Table 4 in order to make it clearer to read and understand as below:.

Table 1: Climate change scenarios for GSM-WASMOD at 21st century (2006-2099)

Index	SDs	RCPs	GCMs	The scenarios of Glacier extent *
1	SDSM	4.5	BCC-CSM1.1	CamESM2_r4i1p1
2	SDSM	8.5	BCC-CSM1.1	CSIRO_Mk3_6_0
3	SVM	4.5	BCC-CSM1.1	CamESM2_r4i1p1
4	SVM	8.5	BCC-CSM1.1	CSIRO_Mk3_6_0
5	SDSM	4.5	BCC-CSM1.1	Inmcm4_r1ip1
6	SDSM	8.5	BCC-CSM1.1	IPSL_CM5A_LR
7	SVM	4.5	BCC-CSM1.1	Inmcm4_r1ip1
8	SVM	8.5	BCC-CSM1.1	IPSL_CM5A_LR
9	SDSM	4.5	BCC-CSM1.1	IPSL-CM5A-LR_r3ip1
10	SDSM	8.5	BCC-CSM1.1	MIROC5
11	SVM	4.5	BCC-CSM1.1	IPSL-CM5A-LR_r3ip1
12	SVM	8.5	BCC-CSM1.1	MIROC5
13	SDSM	4.5	BCC-CSM1.1	MRI_CGCM3
14	SDSM	8.5	BCC-CSM1.1	MRI_ESM_LR
15	SVM	4.5	BCC-CSM1.1	MRI_CGCM3
16	SVM	8.5	BCC-CSM1.1	MRI_ESM_LR

**: the scenarios of galcier extent are taken from Lutz et al. (2014)*

Table 4: You used different GCMs to generate future meteorological forcing for the hydrological model than were used for the future glacier projections. Ideally these should be the same since they are part of the same system. However, I understand that you took glacier projections from another study and included them in yours. I agree that it is better to use some glacier projection is stead of none, but you need to describe the disadvantages of the mismatch in future meteorological forcing used for the glacier evolution model and the hydrological model. This should be more elaborated than in line 300-305.

- Reply: Thank you for the comment. In order to keep consistence and have a more comprehensive future picture of water availability of Beas river basin, we will add the same two ensembles of four GCMs (lutz et al. 2016) as the future projection of glacier in the study of Lutz et al (2016) and we will add discussion for clarifying this mismatch uncertainty of the future scenarios results.

Fig 7: In the caption you mention 'RCMs'. I do not think you can do that because you did not use RCMs in your study, but statistically downscaled GCMs. I suggest to replace ‘RCMs’ by ‘downscaled GCMs’.

- Reply: Thanks. We have replaced “RCM” by “Statistical Downscaling methods” in the whole manuscript.

L250-254, Fig 7: The two different downscaling methods lead to very different changes. This needs explanation of the underlying reasons. I wonder why the two methods were used. I suggest to validate both downscaling methods for the historical period, select the method that performs best, then use that method for the future projections.

- Reply: Please see the reply at the beginning. We will do more comprehensive comparison of the two downscaling methods, if one is found to be significantly better than another, we will use the better one.

Fig9: For precipitation, there is a large mismatch between the two downscaling methods, even at the start of the future simulation. For SVM they start around 1000 mm/yr whereas for SDSM they start at around 1500 mm/yr. This means that at least one of them shows a large sudden jump going from the historical period to the future period. If the downscaling was done properly, the transition from the end of the historical period to the start of the future period should be smooth.This needs to be addressed.

- Reply: Thank you very much for the careful review. There are errors in the data loading and we will correct the Figure in the revised manuscript. We will address the transition from the history period to the future period of the statistical downscaling methods with more detailed contexts in the revision.

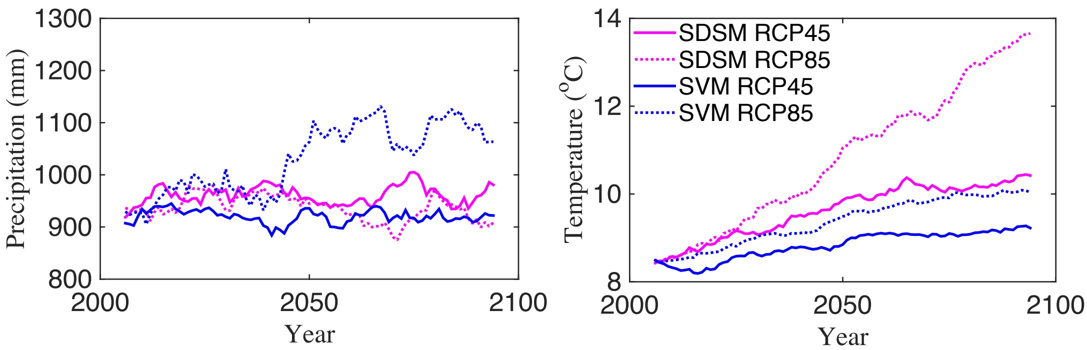

Fig. 3 Ten-yrs moving average of annual precipitation and temperature of the Beas river basin (2003-2099).

The difference in temperature projections for the SDSM and SVM method are enormous towards the end of the century. Describe the reasons for this large difference in the manuscript. Since these methods where used to downscale 1 GCM, the quality of the downscaled forcing for at least one of the downscaling approaches is questionable to me.

- Reply: The temperature simulation algorithm for SDSM is multiple linear regression. It is different from SVM non-linear simulation method. Their response for the future tendency is also somehow different. In this study, we found the temperature goes higher in the future by SDSM, which may mean that multiple linear regression can reproduce higher climate change in the downscaling (Chen et al., 2011). We are now doing more comprehensive validation of the two statistical downscaling methods, and if one method is found to be significantly better than another, we will use the better one. Besides, another statistical downscaling method Advanced Delta Change (ADC) method (lutz et al. 2016) will be added into SD method comparison.
- Jie C, Brissette F P, Annie P, et al. Overall uncertainty study of the hydrological impacts of climate change for a Canadian watershed [J]. Water Resources Research, 2011, 47(12):1-16.

Table 5: The change in discharge is very negative, although you have positive changes in precipitation. It seems that it can partly be explained by the increase in evapotranspiration and by losing the additional water from the negative glacier mass balance in the future. Nevertheless, it feels to me that the decrease in total runoff cannot be that large when precipitation amounts are increasing. Please provide a check of the simulated water balance components (precipitation, evapotranspiration, ice melt, snow melt, rainfall discharge, fast flow, slow flow, and changes in storages) of the catchment for your reference and future runs in the revised manuscript.

- Reply: Thank you very much for the carefully review. We have checked the results and the water balance is correct. While there is a temperature 'jump' between historical period and future period from the statistical downscaling, which resulted in a higher impact of glacier retreating in the total runoff. The discharge significantly decreases because of the glacier retreating. We have made the correction and will update all the results. Furthermore, in order to limit the uncertainty of a single GCM, in the revised manuscript we will add another two ensembles of four GCMs (lutz et al. 2016) and have a more comprehensive investigation of uncertainty band and future change in hydrological cycle and water availability of Beas river basin.

Fig 12: The different lines of the ensemble member are indistinguishable. I suggest to show the ranges as a shaded area with a line for the mean. Since the figure shows all the members from Table 4, I do not understand why all precipitation projections here start around 1000 mm/yr, whereas in Figure 9 the precipitation projections show large difference for the two downscaling methods.

- Reply: Thank you very much for the suggestion and careful review. There are errors in the data loading and we will correct it in the revised manuscript (see

Fig 7 in this document). We also improved the Fig 12 according to the suggestion.

In the caption you mention that the plot shows glacier melt discharge for CanESM2. In Table 4 you indicate that 2 out of the 16 members use glacier projections for CanESM2. How come all the 16 members are shown in Figure 12? This is very unclear. It is also unclear how one could derive the in the caption mentioned tipping point years (2026 and 2052) from the plot.

- Reply: Thank you very much for the careful review. It was a typo and we have corrected it in the caption: "Fig. 1 Annual precipitation, total discharge and glacier-melt discharge under Climate change (see more information in Table 4) from 2005 to 2099". In the study, CanESM2 is used in 2 out of the 16 members in glacier projections. We have checked this error and will correct it in the revised manuscript.
  Besides, in the revised manuscript, we will add two ensembles of four GCMs (lutz et al. 2016) for a more comprehensive comparison and uncertainty analysis, in which we will explain more clearly in the methodology section.

Glacier melt contributions around 500 mm/yr seem rather high compared to the plot you showed in Figure 2. There the annual sum of the 'Glacier ablation' seems to be much lower (around 250 mm/yr as far as I can estimate). This would also imply a sudden 'jump' going from the historical period to the future period, which is unnatural. This makes the whole story somewhat questionable. To gain confidence about the projections please provide a check of water balance components as indicated in the comment to Table 5.

- Reply: Thank you very much for the careful review. We have carefully checked the results. It is because there is a temperature 'jump' between historical period and future period from the statistical downscaling. We have made the correction and will update all the results in the revised manuscript.

L306: See comment on the use of 'RCM' at Figure 7

- Reply: Yes. We have corrected it to be "statistical downscaling methods".

L308: I think you refer here to the 'jump' I point out in my comment about about the glacier melt in Figure 12. I think this is something that needs to be addressed before the projections have sufficient reliability to be published in HESS.

- Reply: Thank you for the comment. Yes, as we mentioned in the previous reply to the comment of Fig 12, there is a temperature 'jump' between historical period and future period from the statistical downscaling. We have made the correction and will update all the results. Furthermore, in order to limit the uncertainty of a single GCM, in the revised manuscript we will add two ensembles of four GCMs (lutz et al. 2016) the same as in glacier projections (see Table 4) for a more comprehensive comparison and uncertainty investigation for the future water cycle and availability in this Himalaya headwater Beas river basin.

L313-337: This part comes out of the blue. It is unclear if you used the combined precipitation and WRF forcing in this study. If you did not, I suggest you redo the study with this precipitation dataset if it has a better representation of precipitation. If you did, integrate this part then in the manuscript (i.e. the methodology to the Methods section, and the results to the Results and Discussion section).

- Reply: As we mentioned in the earlier reply, the combined WRF and gauge precipitation is only evaluated in the experiment analysis part. In the revised manuscript, we will redo the modeling using the combined precipitation for the historical period as baseline

Technical comments

L11: Remove 'the' at the end of the sentence

L13: remove 'the'. 'Climate' shoud be with lower case 'c'

L18: remove 'impact'

L21: Better to reword to: 'This will result in a general decrease in river runoff for all the scenarios.'

L23: I guess you mean 'WRF precipitation projection'

L28: Remove the first 'The'. From here I will stop correcting the redundant use or absence of 'the'. Please have the manuscript checked by a native English speaker. It is advisable to do this before submission for any future manuscripts.

L29: Reword to: 'Hydrological models have been developed and used as a main assessment tool in the Himalayan region to estimate the impacts of climate change for future water resources.'

L35: 'the' should be 'an'. 'GCM' should be 'GCMs'.

L38: Change 'More' to 'An increase in'

L39: Change ‘by’ to ‘according to’

L54: Introduce Regional Climate Models before using the acronym RCM

L83: ‘simulations’ should be ‘simulation’

L95: Reword to ‘The main questions we try to answer in this study are:’

L110: add m asl (metres above sea level)

L115: correct ‘meteorological’

L137: No parentheses needed here

L141: No parentheses needed here

L167: ‘totally’ should be ‘in total’

L178: Remove ‘was’ and ‘which’

L243: Included ‘simulated’ between ‘The’ and ‘annual’

General: There are many textual errors. Please have the whole text reviewed by a native English speaker before submitting the revised manuscript. Please do this for future submissions before the initial submission of the manuscript.

- Reply: Thank you so much for the careful review and correction! We will correct all theses in the manuscript. Besides, we will ask help from our native speaker colleagues and correct the further textual errors in the whole manuscript.

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- Reply: Thanks a lot for the recommended citations. We will add them in the revised manuscript.