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

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.

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

Specific comments

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

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.

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

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

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

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

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.

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

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).

L120: Add a citation for the Hydro1k dataset

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?

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.

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

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).

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.

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.

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.

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

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

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.

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.

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

L198: 'T' needs to be explained

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

L201: subscript 'j' needs to be explained

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

L213-215: Which GCMs are used?

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.

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.

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?

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.

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.

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.

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.

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.

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'.

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.

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.

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.

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.

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.

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.

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.

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

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.

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).

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.

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