Anonymous Referee #3

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Foreword by authors.

Besides and before the requirements from the 3 reviewers as provided by HESSD, we provided few modifications and improvement to the paper, as follows.

1) Dr Oxana Savoskul, at the Institute of Geography, Russian Academy of Sciences, kindly noticed us via a private email about a wrong estimation of glaciers' surface in the Shigar catchment we reported in the first version of our paper. Our estimation of the glaciers' size within the area was considerably higher than what reported in ICIMOD (2004) cadastre (ca. 4200 km^2 *vs* ca. 2200). Glaciers' coverage within the 10 altitude belts, used in practice by the model to provide ice melt volume was therefore mis-estimated. We duly re-evaluated the glaciers' surface using visible images of the area during summer and we obtained a more reasonable value of ca. 2700 km^2, divide in bare ice and debris covered ice. This value is still different from that in the 2004 ICIMOD cadastre (ca. 20% more), but this difference may be explained by presence of debris covered area, or by other error in classification, and operator's subjectivity (e.g. for fresh snow identification). We decided however to rely upon our estimated values. We then calculated new degree day factors for ice on the catchments. New values are now reported in the manuscript, Section 4.2.
   We kindly acknowledge Dr Savoskul and we are sorry for this mistake that we now amended.

2) To provide better description of snow cover dynamics and in channel flows, we adopted a varying value of snow degree day, which we obtained by consideration of i) SCA according to MODIS, and ii) monthly in stream flows. Albeit the average value of snow melt factor D_{Ds} was in practice coincident with our previous estimation (D_{Ds} = 2.5 or so), using a variable value we obtained a better description of SWE dynamics and of in channel flows. Explained now in section 5.1.

3) We re-applied the CO and climate scenarios CCS1-4 under these new conditions. The results we obtained are slightly quantitatively different from those in the first version, but qualitatively similar, so we think that the modification we made provide minor changes to the main message of the paper.

4) In the Tables of results (4,5) we provided values of hydrological variables (SWE_{av}, ICE_{av}, S_{av}) weighted upon the altitude belt surface, more significant of the average values within the catchments.

General comments The manuscript 'Prediction of future hydrological regimes in poorly gauged high altitude basins: the case study of the upper Indus, Pakistan' reports on an interesting and relevant approach to flow prediction in highly and data scarce environment. However the authors need to attend to some presentation/formatting inconsistency and further explanations of the methodology are requested for some sections of the manuscript.

And one would expect some observations on the reliability/confidence level of the approach, aspect that is lacking in the present work.

When dealing with future evolution of hydrological cycle, as of any other natural process, the assessment of the reliability (meaning, the capability of the model to perform similarly under similar scenarios ?) and confidence (meaning, the interval where the future values of discharges will fall in ?) is utmost complicate.
In fact, the projected modification of the hydrological cycle put forward here depend upon i) model' parameters used, ii) the climate input, and three iii) the hypothesis done about glacier behaviour,
The circumstance that the model reasonably mimics hydrological cycle in the area (i.e. mean monthly discharge during 1985-1997 as available, Figure 6) may indicate that model parameters are reasonably well set. More uncertainty is cast upon snow melt factor D_{Ds}, which we however tried to "independently" cast using SCA images.
As we reported in the discussion, in fulfilment of the Paprika project, we will carry out at least a field study aimed to establish in situ measured melt factors for snow within the (accumulation) area of the Baltoro glacier, which is in our opinion the only way to rule out uncertainty about D_{Ds}.
Possible methods to test reliability may include use of climate inputs as projected by other GCMs.
The hypothesis we considered about glaciers are substantially standard as adopted in the present literature, besides our explicit approach of melting first ice in the lowest belt (which apparently is not explicitly used in other studies, according to our understanding).
Confidence level of the approach (meaning in our understanding the spread of the predicted discharges) may be investigated, say by ensemble simulations (i.e. by feeding the mode with other "synthetic", statistically variable climatic scenarios, based upon the deterministic one from the CCSM3, or other GCMs).
We did so for instance in Groppelli et al. (2011c), where we studied prospective climate change within an Italian catchments, and we simulated a number of statistic scenarios, with statistics equivalent to those by the GCMs (deterministic scenarios) used there. However, besides the day to day variability as given by the synthetic noise bestowed upon the deterministic scenario, the final (mean) results in term of hydrological cycle (monthly flow, flow duration curves, etc..) are substantially the same as from the deterministic scenario itself, which establishes the underlying climate signal.
So we expect that the mean hydrological cycle scenarios as described within our paper are representative of the hydrological cycle under modified climate (by CCSM3) and glacier cover as from the scenarios CCS1-4.
Notice further that here we do not need to assess whether the future hydrological scenarios will be significantly different from those of the control run period, as we already know that temperature, precipitation and ice cover in the CCS1-4 scenarios are different from those in the control run.
These points are now briefly reported in the discussion section.

Specific comments Formatting/presentation The authors are not consistent on the referencing throughout the manuscript, in the introduction references have 'e.g'(page 3745, lines 2, 9, 13, 16, 22, 27) for some authors but not for all of them (page 3745 line 8) same observation for page 3746 in addition to the fact that here the reference line 18 has 'see' prior to the author's name.

Now "e.g." and "see" were dropped when not appropriate.

The authors use several acronyms in the manuscript without providing at least once the full description, reference made to page 3747 line 10 (TRMM), page 3749 line 1 (EVK2CMNR), line 6 (CCSM3 GCM, IPCC), page 3750 line 1 (BWK), page 3751 line 14 (RGB),page 3754 line 21 (ICIMOD), page 3767 line 20 (SCS-CN), page 3769 line 8 (SRM)

Acronyms were now introduced in the first place.

Table 1 There is a note underneath that is misplaced Table 2.

Typo error, removed.

The label [.] is not appropriate for items without units.

Now used [-].

Table 3 What is the unit of the water content, wilting/field capacity?

Is dimensionless, because $\theta = S/S_{Max}$

Table 4 Total precipitation unit not correct

Now used $mmyear^{-1}$

Figure 2, the grid size label will convey more information to the reader

Fixed

Page 3764 line 19 unit is missing for soil moisture (47)

Fixed. Values now slightly changed as due to weighting, reported in the foreword.

References Page 3746 line 26, reference Ming et al., 2007, not consistent with the year in the reference list.

Now it is correct.

Page 3748 line 2, reference Seibert et al., 2009 should read Seibert and Beven 2009.

Fixed

Similar change to be done for reference Gabriel et al., 1991 line 7.

Fixed

Similar observation for reference Soncini et al., 2011 page 3752 line 4.

Fixed

Page3751 line 26, reference Bocchiola and Rosso, 2007, the year is not consistent with the reference list.

In reference was Bocchiola and Rosso, 2009.
Bocchiola and Rosso, 2007 is another paper, now added.

Reference not done per norm page 3753 line 8.

I don't get this.

Phrasing Page 3746 line 46 , kindly rephrase 'seems still limited'

Rephrased

Page 3767 line 5, kindly rephrase Page 3769 line4 and 5, comments seem to be misplaced.

Comment dropped.

Content, Data, Page 3752, more description needs to be provided on scene A2

Description added

Page 3758, the authors should explain why Askole was seen to be the most representative station of the study area, any statistics to support their statement on line7?

As reported in Section, we used data from Askole simply as it was the most complete (i.e. with more measured days during 2005-2009) of the two daily stations (Askole, Urdukas). No statistical indicators used.

Page 3758, authors used the 500m spatial resolution DTM, is it appropriate to cater for steep gradient in mountainous region like the HKH?
Can authors explain why they did not use finer datasets of DEM that are freely available?

This was a mistake, DEM was at 30 m.

Modelling Page 3755, line 22, the authors refer to 'some others parameters , they should provide the full description.

Reference is now made to Table 3 containing all the parameters mentioned here.

Assumptions Page 3766, line 5, the authors reported that 'no noticeable down wasting should occur by 2059' what are the underlying assumptions?

Because within our CCS1-4 scenarios we found continuous snow cover in Belt 6, one may imply that no ice melt occurs in (and above) that belt, as snow would protect (and also feed) ice cover. This in turn implies that no down wasting of ice cover should occur in (and above) that belt.

Sensitivity and uncertainty analysis. The authors refer several times in the manuscript to the sensitivity analysis (page 3766, line 14, page 3768, line1; page 3769 line 23)but nowhere in the manuscript results of the sensitivity analysis are presented.

In the first case, sensitivity is to ice cover scenarios (-0% to -50%). Modified

Second case, clearly with different climatic inputs (temperature, precipitation) different results would be obtained. Modified.

Third case, we simply suggest hints for future research, of using other GCMs and/or sources for climate inputs. Specified now.

Page 3763,paragraph starting line 20,the authors acknowledge that accuracy is not key to their approach, what is the added-value of this study especially to water managers and planners? Is this just another academic exercise without any practical relevance on the ground?

The present study will not depict future conditions with high accuracy (i.e. by providing estimation of future discharges that are very close to those actually occurring), which is intrinsically impossible given that foresight of the future would be required.
However, the present study may give an order of magnitude of the expected hydrological cycle 50 years from now under reasonable hypothesis concerning the evolution of climate, and depending upon evolution of the glacier coverage.
In this sense this analysis, together with the few studies already carried out in this area, may aid initial brain storming about possible initiatives for adaptation to the future hydrological conditions.
Also, we presented a methodology that may be used from other investigators dealing with high altitude basins worldwide, with sparse and/or scarce data coverage.

Discussed in the conclusions.