Anonymous Referee #2

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.

The manuscript is focused on the third pole region (Hindu Kush, Karakoram and Himalaya). The aim is to predict future hydrological regimes in poorly gauged, high altitude, basins. In particular, the case of the Shigar river (7000 kmˆ2, more that half glacierized, elevation ranging from 2000 to 8000 m asl) is presented. The basin is indeed a poorly gauged catchment: only two (daily) meteorological stations are available (at 3015 and 3926 m asl) and 8 stations with monthly weather data (all positioned below 2500 m). Only one discharge gauging station is available, located at Shigar bridge (7000 kmˆ2 upstream; 2200 m asl). Monthly mean discharges registered in this section over the period 1985-1997 are used to calibrate the model. The proposed exercise, aimed at evaluating climate change effects on water resources in the study area, relies on a rather classical approach of feeding a hydrological model with downscaled GCM data. However, after reading the paper, given the extremely complex condition where the application is done, I remain mostly unconvinced of the appropriateness of the adopted modelling framework and feasibility of some of the hypotheses. The most critical points in my opinion are discussed hereinafter:

1) The Authors propose to "use yearly total precipitation from 8 [meteorological] stations to evaluate the presence of monthly lapse rate of temperature and precipitation" (3752:20-24). My objection is that estimating a precipitation/temperature rate from two stations located at relatively low elevation and then extrapolating the trend up to 8000 m is not a "feasible hypothesis". The Authors seem to be somehow aware of this weakness when they state "At high elevation this may lead to an overestimation of precipitation. However, this may have a little impact on the hydrologic balance, because with increasing altitude contributing area decreases significantly" (3753: 1-5). The contributing-area effect is a well known effect, but it cannot be used as a justification in this case and, more in general, when the sensitivity of a snow- and glacier-driven regime is investigated (due to the direct effect of the snowline migration, induced by temperature increase, on the melted volumes and, hence, on discharge). In this sense, also the results shown in Figure 5, where SWE (snow water equivalent) in elevation band 6 is found to increase during the calibration period 1985-1997, could be an effect of an uncontrolled increase of precipitation at high elevation due to the use of Eq. 1.

The concern of the reviewer is indeed meaningful. The discussion about the existence and magnitude of precipitation drift *vs* altitude is an important one.
We overall discussed this point with several participants to the Paprika project, and with scientists dealing with the issue of water resources assessment in the HKH region under climate change.
We further investigated the available literature in this field.
However, providing a final response about such matter seems way to come.
The lack of long term records from high altitude gauging stations (higher than 3000 masl here) makes it very difficult to evaluate the total precipitation drift.
Here we used yearly total precipitation from the 8 PMD stations during 1980-2009 (and not from two stations as discussed by the reviewer) to assess an altitude drift, which we took based upon a power law, according to the approach proposed by Winiger et al. (2005).
They demonstrated (Figure 8 therein) that total precipitation within Karakoram region may be interpreted to vary according to a power low up to 5000 masl. We therefore used a similar approach, and we extended use of such power law above 5000 masl. Albeit this may indeed result into overestimation of precipitation at high altitudes, given the lack of observations, there is little way to correct this approach.
As reported, we possessed short term (four years, 2005-2008) precipitation data from two stations, at Askole (3015 masl) and Urdukas (3926 masl), which we used to tailor downscaling procedure.
However, due to the considerable amount of missing days (especially during winter for snowfall), we could not use such data to increase sample dimensionality (and range of altitude) for dependable estimation of precipitation-altitude dependence.
We also used 10 years average TRMM precipitation data kindly provided by Dr Bookhagen, as reported, but we did not find any clear pattern of precipitation against altitude.
In fact, TRMM estimates (precipitation upon cells with 5by5 km^2 size, and altitude ranging from 2000 masl to 7000 masl) indicated in practice no significant change (either increase or decrease) of precipitation against altitude.
However, the noticeable noise in precipitation estimation using TRMM (as any other satellite) data makes the approach uncertain, so we preferred to rely upon ground observations, albeit few.
Concerning belt 6 (5375 masl-6010 masl), one should remind that it is not known exactly how the dynamics of snow cover in this area is played. The (starting of) accumulation area of Baltoro glacier, above which snow cover is expected to accumulate in time (and be transformed into ice, albeit here such process is not considered so far), is placed approximately between 4800 masl and 5200 masl (Mayer *et al.*, 2006; Mihalcea *et al.*, 2008).

Thus, the circumstance that belt 6 here displays snow accumulation seems not an artefact of having to much precipitation, but rather a temperature controlled phenomena, consistent with present knowledge.
Eventually notice that our MODIS images (2005-2009) display always permanent snow cover in belt 6, as reported in the manuscript, so giving further ground to our findings.
Notice that in Belt 5 (4740-5374 masl), covering quite well the expected (start of) accumulation region, a more variable snow dynamics is seen, with seasonal snow cover in practice (Figure 4).
Therefore, snow dynamics as depicted by the model seems consistent with the present knowledge about snow dynamics of the Baltoro glacier.
We may still have to much precipitation, but this is unlikely to affect the representation of snow dynamics given by the model.
Now, we discussed this issue in more depth in Sections 4.1 and 5.2.

2) The other weak point of the paper is related to the selection of the model time-scale. The Authors, despite having most of the data available at monthly time step (included discharge at the control section, used for calibration), decide to use a daily time step (and to disaggregate the data). The reason why disaggregation is adopted, rather then addressing the problem at the monthly time-scale, is not evident, especially in a "typical ground of application of PUB concepts, where simple and portable hydrological modelling is necessary"
Contrary to this purpose, in fact, the downscaling procedure introduces additional parameterization, both in the disaggregation step (described at page 3753) and in the (daily) modelling part (whose parameters are listed in Table 3). In addition, the verification of model outputs (versus monthly averages at Shigar bridge) appears a rather weak technique to validate model structure.
As a consequence, the "additional information" resulting from disaggregation, e.g. the sensitivity of the flow duration curves to future climate scenarios, will be likely affected by high uncertainty and/or "inaccuracy", as stated by the same Authors at lines 3763: 12-13.
I suggest the Authors should try to reframe their modelling approach, by evaluating the feasibility of adopting a monthly-scale process description.

Daily modelling is required in our perspective to display the possible range of variability of flows under the proposed scenarios.
Also, notice that modelling of snow and ice melt requires at least use of daily data, *i.e.* for degree day approach, which is probably the mostly diffused one for the purpose. Use of monthly data may in fact hinder ice and snow melt estimation (e.g. in case of monthly average low temperatures, but with days of above zero temperature, resulting into snow and/or ice melt).
Further, soil moisture, actual evapotranspiration, and sub-superficial flow production are non linear processes related to temperature and rainfall, thus use of monthly such data would not provide an appropriate description of these variable, and eventually of the complexity of the hydrological cycle.
Thus, we want to demonstrate here that merging coarse (monthly) scale data (8 PMD stations) with finer (daily) scale data (2 EVK2CNR stations), also featuring different periods of observation (namely, much shorter for the 2 EVK2CNr stations) may provide a way to profit of all the available information, including the data at coarse scale, to obtain a hydrological framework which still represents the ongoing processes (ice melt, snow melt, runoff, evapotranspiration, etc..) at a meaningful time scale.
Clearly PUB requires simple models, but yet the processes underlying hydrological cycle need to be described at their characteristic scale. In this sense, our disaggregation strategy is a tool to extract information from the few data available.
Often, dealing with poorly gauged basins boils down to having disparate sources of data with different extent and resolution, and the case study we propose and the disaggregation method out forward are exactly dealing with such a situation.

Notice also that use of disaggregation method is nowadays diffused and does not represent a bottleneck at all for hydrologists.
Validation against monthly data (in fact means over a period) is not a voluntary choice, but it is boundary given the available information.
From daily outputs, one can still evaluate mean yearly/monthly values for water resources management purposes. So doing, the “noise” (or variability) introduced by disaggregation is filtered out, and the necessary information for water resources management retained.
Similarly, in building the flow duration curves, that are obtained here using ten years of data (13 for the control run), i.e. at least 3650 daily values of discharge, the noise introduced by disaggregation is filtered out.
Notice further that because GCMs like CCSM3 (plus downscaling) carry information of the variability in time of the distribution of rainfall at a daily scale, use of a daily model may give reason of the response of the catchment to such variability.

To conclude I feel that, to make their contribution suitable for publication in HESS, the Authors should urgently tackle and discuss the two major methodological problems listed above.

Moreover, while revising the paper, they must be particularly careful with paragraph 5.3 (Future hydrological cycle) which, despite being the main result of the study, appears rather confused (and confusing) in that: i) the difference between “unchanged” and “changed” glacier coverage scenario is not well explained, nor very intuitive, in the current paper version and ii) the reference to the “index flood” behaviour (3764:14) sounds somewhat unexpected and groundless in a paper aimed at the prediction of the sensitivity of water resources of high elevation basins to climate change.

We tried to rework section 5.3 to make it clearer.
Calculation of index flood is used to evaluate another indicator of flow characteristics, and particularly the highest yearly floods.
Several indicators of high and low flows are reported in Table 5, so index flood is just another one.
Of course the aim here is not to deepen into extreme floods estimation in Shigar river, but to provide an idea of relative variations under climate change.

In addition, the Authors should make an effort to polish the language and style of the manuscript (see e.g. lines 3749: 15-19); as well as the accuracy of the exposition (e.g., the acronyms not always are explained, as in the case of the snow covered area SCA).

We tried to improve English, and to introduce all the acronyms in the first place.