

Anonymous Referee #1

Received and published: 1 June 2011

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 km² 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 km², 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.

This work presents hydrologic modelling results for changes of climate and glacier coverage in a data limited catchment in the Hindu Kush, Karakoram, and Himalaya (HKH) region. As this work is submitted as part of a special issue on prediction in ungauged basins (PUB) work, the methods put forward and modelling carried out are appropriate. The authors have done a nice job highlighting some of the potential limitations of this pure modelling work. This is important from a PUB perspective. There are a few general comments that I urge the authors to address and/or consider as this paper moves towards publication.

Reading through the paper, there is no real issue with the assumptions made and approaches adopted. However, in the discussion (around Pg 3769) some flags start to go up for me. My concern

is that the work presented has been put into a context of another study (Akhtar et al., 2008) in a nearby region with conflicting results. The discussion around this point loses some rigor (e.g., Ln 5, Pg 3769 “Eventually, we may state that our results here broadly speaking agree with their findings”) and needs to be better presented. Further, and more importantly, it appears that one of the main strengths of the approach presented in this current study (particularly from a PUB perspective) is that the approach is “simple enough that portability to catchments nearby should be reasonably practicable”(Ln 16, Pg 3769). Bringing this all together: Why not port the modelling results to the catchments considered in Akhtar et al. (2008)? This seems like a good PUB exercise that the authors could reasonable undertake.

We tried here to make a comparison against results from the available literature for the purpose of providing the reader with a wider picture about possible climate change impact within the area. However, the few contributions available within the present literature are hardly mutually comparable. Besides the fact that such approaches may be carried out within to the “same” area of Karakoram, different hydrological models are used, and different hypothesis are made about the hydrological cycle, particularly with reference to ice and snow melt dynamics.

Concerning future projections, different climatic inputs are used from, and different time window are considered.

Also, future ice cover is assumed based upon different percentage, but it is not understandable how such coverage is distributed.

In our paper we made clear that lower ice belt melt entirely first than higher ice belt, but such hypothesis is not made, at least in our understanding, in other studies.

Given the tremendous importance of the ice cover in feeding in stream runoff, different assumptions about ice cover may lead to different results.

In conclusion, accurate comparison between different studies is inherently difficult, and one can in fact limit himself to qualitative benchmark.

Concerning the specific case of Akhtar et al. (2008) study, notice that as explained we carried out our research within the framework of a project (PAPRIKA project) defining specifically as a target catchments the one here investigated, and as reference period the next fifty years or so.

That is the main reason we focused upon this period.

However, another consequence of our choice of case study basin is that we have no information concerning catchments outside this area. Data retrieval in this area, as also depicted in the paper, is complicate, and we could gather only data for the catchment study.

Another point is with ice melt data, that are not collected routinely.

We possessed ice melt data for the Baltoro glacier, which were gathered by the scientists cooperating in the study, and used within the area under an assumption of reasonable homogenous behavior, while the ice melt data for other glaciers, if present, were not available to us.

In conclusions here, we wanted to report how a minimal hydrological model, based upon semi-distributed catchment partitioning, using temperature and precipitation data, together with experience from one or more field campaigns can be used for flow prediction within a poorly gauged high altitude catchments, and assessment of future water resources therein.

Other scientists may use this approach for their catchments of interest, but provided a least amount of data is available, as specified.

Along similar lines, it is difficult to assess the appropriateness of the hydrological modelling (Ln 17-20, Pg 3755) put forward. Previously, it appears that the modeling has been published in national journal (Groppelli et al., 2011b) and is currently under review in another international journal (Groppelli et al., 2011c). The relative ‘newness’ of this model is interesting as it is being used in a difficult application (i.e., a data limited PUB environment). A new model in an ungauged system almost seems like a ‘doubleblind’ experiment.

The model is very simple indeed, as reported, and a similar model (semi-distributed altitude based, daily, but with no calculation of evapotranspiration and infiltration terms) was also used before for investigation of a small high altitude glacierized basin (Bocchiola et al., 2010, reported in the reference section). However, the paper referred to as Groppelli et al. (2011c) is now published upon NHESS (2011, 11,1-17, reported in reference section), and full reference to model's accuracy can be found.

Perhaps a parallel study in a nearby system (e.g., the catchments from Akhtar et al., 2008) where more information and/or orthogonal modelling results are available could help.

Again, we do not possess the data to do such modelling, nor data from other catchments than the Shigar were made available to us.

Lastly, the choice of future projections of daily flows into the years 2050 through 2059 is interesting. What is the motivation behind that choice? Would it have made more sense to try to overlap somewhat with the other studies done in the region where analysis was carried out from 2071 through 2100?

The motivation is related to the Paprika project, requiring description of water resources for the next 50 years or so. Notice that there is no reason to a priori consider a specific time window. In the future, we may consider longer or farther time periods. Also, given the differences highlighted in view of the different models (climate, hydrological) adopted, and the modelling hypothesis, even using the same time window no more than a qualitative comparison would be drawn.

This is also related to the intrinsic unlikelihood of having different methods based upon different tools to provide strictly adhering forecast of discharges for a period of time so far ahead.

Also, presenting the daily discharge dynamics for these future predictions might be a bit unnecessary. The climate projections do not give any real estimate regarding daily values. I would not have expected to see much more presented than annual discharge changes and long term trends for the future scenarios. This would still deliver the main message regarding ice coverage influence and (perhaps) drive home important limitations regarding expectations around PUB.

Comprehensive statistics are reported in Tables 4 and 5, and in the Figure 8. These answer the requirements about "mean" behavior of discharges in the future.

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 melt approach (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 the complexity of the hydrological cycle.

Because GCMs like CCSM3 here carry information of the variability in time of the distribution of rainfall, also at a daily scale, use of a daily model may give reason of the response of the catchment to such variability. Further on, from daily outputs, one can still evaluate mean yearly/monthly values for water resources management purposes.

In addition to the above general comments, the following is a list of minor/ detailed/editorial comments to be address or corrected:

Ln 9-10, Pg 3745: change "contribution in the scientific available" to "contributions in

the available scientific”

Fixed

Ln 12, Pg 3746: The sentence starting with “In view of: : :” needs rewritten.

Rewritten

Ln 3, Pg 3747: Here and everywhere, you have already presented an abbreviation for Hindukush-Karakoram-Himalaya as HKH.

Fixed here and where needed

Ln 22, Pg 3747: change “In facts” to “In fact”. This error occurs in several locations. Please check thoroughly.

Fixed.

Ln 10, Pg 3748: change “area” to “areas”

Changed

Ln 13, Pg 3748: change “albeit” to “although”

Changed

Ln 17, Pg 3748: change “Nor the” to “Nor are the” and delete the “are” later in the sentence.

Fixed

Ln 17, Pg 3749: change “are” to “is”

Changed

Ln 1, Pg 3750: You have not defined BWK yet

Defined

Ln 22, Pg 3751: This “yes/no” confuses me. Could you present this better?

Changed to presence/absence

Ln 24, Pg 3755: change “are considered two mechanism of flow formation” to “two mechanisms of flow formation are considered”

Changed

Ln 17, Pg 3758: the statement “discharge under this form is only available to us” makes is sound like you were the only people with access to this data. I think you mean that there is no other data available. Consider rewriting.

Rewritten.

Ln 19, Pg 3758: Do you mean “piece wise” instead of “path wise”?

Dropped, not necessary

Ln 20, Pg 3759: change to “(at a monthly scale we saw little sensitivity)”. That is you should add the parenthesis.

Changed slightly, but not with parenthesis, I do not see how put these, sentence would make no more sense.

Ln 17, Pg 3763: change “more complicate” to “a more complicated”

Changed

Ln 24, Pg 3764: again, “In facts” to “In fact”

Changed

Ln 18, Pg 3765: change “here projected” to “projected here”

Changed

Ln 19, Pg 3765: change “cumulated” to “accumulated”?

Changed

Ln 21, Pg 3765: change “provide” to “provides”

Changed

Ln 22, Pg 3765: change “so” to “thus”?

Changed

Ln 23, Pg 3765: change “no more permanent” to “no longer permanent”?

Changed

Ln 24, Pg 3765: delete “alone”

Deleted

Ln 25, Pg 3765: delete “here”

Deleted

Ln 7, Pg 3766: change “what” to “what is”

Changed

Ln 9, Pg 3766: the phrase “down wasting up the an area” sounds strange. Rewrite?

Rewritten

Ln 17, Pg 3766: change “worst drought spells” to “worsening drought spells”?

Changed

Ln 21, Pg 3766: change “model” to “models”

Changed

Ln 23 Pg 3766: change “in practice unavailable” to “in practice than available”

Can't understand this. Changed to “not available here”

Ln 28, Pg 3766: change “a least” to “some”?

Changed.

Ln 29, Pg 3766: delete “least”?

Deleted

Ln 4, Pg 3767: Do you mean “noise” or rather “uncertainty”?

Both may fit, but here I mean exactly “noise”, as given by introduction of a synthetic signal of rainfall variability.

Ln 19, Pg 3767: “soil retemption”? I guess this should be “soil retention”?

Yes, sorry.

Ln 20, Pg 3767: change “were” to “where”

Changed.

Ln 24, Pg 3767: delete “henceforth”

Deleted.

Ln 18, Pg 3768: delete “As reported in the introduction”

Deleted.

Ln 27, Pg 3768: change “consistently” to “consistent”

Changed.

Ln 3, Pg 3769: change “highest” to “higher”

Changed

Ln 22, Pg 3769: again “retemption”?

Changed.

Table 1: There is a strange line of text at the bottom of this table. Perhaps editorial error?

Seems a typo, or question from editorial staff, don't get the meaning anyway.

All Tables and Figures: the abbreviations are not introduced such that the reader can understand them. It took me very long to understand the CO implied control runs.

I tried to introduce abbreviations in the text, and in the Table caption.