Earth Syst. Dynam. Discuss., 6, C437–C449, 2015
www.earth-syst-dynam-discuss.net/6/C437/2015/

Earth System Dynamics Discussions

Interactive comment on "Prevailing climatic trends and runoff response from Hindukush–Karakoram–Himalaya, upper Indus basin" *by* S. Hasson et al.

Anonymous Referee #2

Received and published: 14 July 2015

Following are review comments and recommendations of the manuscript titled as: Prevailing climatic trends and runoff response from Hindukush – Karakoram – Himalaya, upper Indus Basin

S. Hasson, J. Böhner, and V. Lucarini

Submitted to: Earth System Dynamics Discussion Manuscript No. ESD-2015-12

Comments and Recommendations

Since publication of IPCC (2007) report and its erroneous argument about the Himalayan glaciers (i.e the glaciers of the Himalayas will disappear by 2035), the moun-

tain region of the Hindukush-Karakoram-Himalaya (HKH) remained main focal area of hydro-climatic research. This paper is also a continuation of the same series, and attempts to explain hydro-climatology (and the Karakoram Anomaly) of the HKH region; the Upper Indus Basin (UIB), using historic hydro-climatic data trends. The trend analysis consist of long and short time series trends of maximum, minimum, average and diurnal temperatures, monthly, seasonal and annual precipitation and flows data.

The findings of the paper can be summarized as that: (in general) winter to spring/early summer temperatures are rising, while summer temperatures are declining, together with precipitation rise (mostly winter precipitation) in the Karakoram region and decline in monsoon precipitation in some sub-basins of the UIB. The decrease in summer flows has been noticed and is argued to be mainly caused by the decline in temperature and early melt. These findings have been argued to be in good agreement with the "Karakoram Anomaly"; positive glacier mass balance.

However, there are several substantive issues, which make the above results doubtful, and need to be addressed prior to make this paper suitable for publication. Some of these issues are related to the accuracy of data, while others are related to the methods (such as time series length, statistical significance of trends, flow data estimation) and wrong/biased results' citation of the available literature. Therefore, based on vital drawbacks my decision is rejection of the current paper for publication, however, the authors are encouraged to re-visit the accuracy of data sets, methods and re-write and submit a new version for re-consideration. Hope my critical constructive review and comments will enable the authors in their re-writing and re-visiting their work. Main drawbacks followed by specific comments of the current study are as follows:

1. The quoted precipitation data sets for low altitude valley based stations are far different from already available other published papers' data sets for the same stations, obtained from the same sources, although there is slight difference in time periods (and can be ignored for long term averages). For example for the Gilgit station long term average annual quoted precipitation is below 50mm (see Line 30 page 588, Line 18

page 591) as opposed to long term average annual precipitation for the same station ∼130 mm (see for example in Archer and Fowler, 2004; Tahir 2011; Mukhopadhyay and Khan, 2014a). Similarly, for the Skardu station the quoted annual precipitation is more than 1000mm (see Line 3 page 589 and Line 4 page 591), whereas for this station the long term annual precipitation is about 223 mm (about 1/5th of the present study) in various published studies (such as in Archer and Fowler, 2004; Tahir 2011; Mukhopadhyay and Khan, 2014a). Interestingly, all previous studies' long term average annual precipitation estimates for their studied stations are in good agreement, besides there are also slight differences in study time periods. Due to difference in time periods, the difference among current study's estimates and previous studies' data cannot be too large (∼ 1/3rd to 1/5th). This, indicates that there are some serious accuracy issues for datasets used in current study, at least in low altitude valley based stations' precipitation data (or wherever data is shown/provided). The temperature and high altitude stations' data could have not been compared due to either limited available published data or due to non-provision of estimates in the current study. Use of inaccurate data and their trends cannot provide true representation of the Hydro-Climatology of the study area, therefore the results of the current study are doubtful, else otherwise all above previous studies' results and trends are inaccurate and biased. In sum, the authors need to check the accuracy of their collected and estimated data sets, and a Tabulated comparison (in re-submitted version) with previous studies could/will be useful.

2. The authors argue that the UIB boundary has long been overestimated by various researchers, and they have estimated it precisely/accurately. There are two major drawbacks in their statements in Line 8-20 page 587. a) The cited reference studies (03 out of 04 cited studies) have not overestimated/over-quoted basin areas (except 01: Hasson et al 2014a). According to WAPDA the UIB at Besham Qila is about 162,393 km2, while the cited studies have provided nearly the same estimates, such as Alford (2011) has quoted an area ∼ 166,069 km2 (see his section 1.1, page 7), Sharif et al. (2013) have provided an area ∼ 168,000 km2 (see their section 2, page 1505),

and Young and Hewitt have used an area of WAPDA (i.e 162,393 km2, see their Table 2). The maximum difference (overestimation) is < 3.5% (for Sharif et al. 2013), however, such slight differences can be ignored due to difference in projection systems, difference in delineation methods and use of different Digital Elevation Models (DEMs) (Also see specific comment (x), where some examples of various area estimates are provided and are plausibly due to use of different projection). Although Hasson et al. (2014a) significantly overestimated the UIB boundary but this study is for the entire Indus Basin, and no separate estimate (numerical estimate) of the UIB has provided, therefore such an example is also not easy to follow. Another study, Hasson et al. (2014b), should have been cited, instead. In this study the estimated area for the UIB is ∼ 271,359 km2 (∼ 67% greater than WAPDA's basin). There are many other studies, which overestimated the UIB boundary, and their areas are > 23% than the WAPDA's estimate (see for example Immerzeel et al., 2009; Tahir et al. 2011; Bookhagen and Burbank, 2010). Such detailed examples of overestimation can be found in Khan et al. (2014) and Reggianni and Rientjes (2014) studies. Therefore, the authors need to avoid biased citation of previous studies, and have to revisit the available literature. b) The argument that the authors have precisely and accurately estimated the basin boundary is an example of self-praise and not crediting previous researcher's work, and should be strictly avoided. Besides some other available precise estimates for the UIB, a first comprehensive study was presented by Khan et al. (2014), where reasons of such overestimations have been discussed in detail. This study was followed by Reggiani and Rientjes (2014), where the studies with overestimation and precise estimate have been provided. The authors should duly consult/cite these studies. The authors also need to provide details about delineation method and source of the SRTM DEM.

3. During delineation of a watershed boundary the stream network (particularly the start point of a stream) is generated based on either flow area (or number of cells draining to a downstream cell). This provides a stream network, well within the basin's boundaries. This provides nearly a uniform distance of stream network from the basin's

boundary. However, the stream network provided in Figure-2, page 648 does not provide nearly uniform distance from the exterior basin's boundary. In no case a stream should cross the basin's boundary (except at the basin's outlet), whereas near to the eastern part of the Shyok basin the stream 2 in following Figure B (zoomed part of Figure 2, page 648) crosses the outer basin's boundary. Similarly, stream 3 also nearly touches the boundary. The distance between boundary and streams is significantly variable (see streams 1-4, following Figure B). All this makes the delineation of the UIB doubtful. The authors need to address this issue, and have to carry out a re-delineation, together with a revision of the Figure.

4. The authors have adopted an additive method for estimation of missing flow values for the Shigar basin (in addition to some other parts of the UIB). This is provided at S.No 11 in Table 1, page 638, where flows of the Yogo and Kharmong stations have been subtracted from Kachura station's flows. During flow estimation the area between the downstream station (Kachura station) and upstream stations (Shigar, Yogo, and Kharmong stations) has been ignored. Ignoring such upstream areas can generate significant biases, particularly near to the highly glacierized basins. According to the areas in Table 1, page 638, there is about 3,649 km2 (>50% of the Shigar basin's area) ungauged area, which contribute to the flows of Kachura station in addition to upstream gauging stations' flows. Furthermore, sum of the Shigar, Yogo and Kharmong stations (for the available overlapping period of record) is not equal to the Kachura stations' flows. This confirms that a simple additive approach (at least as authors applied herein) may not be suitable for the Shigar's flow estimation. Therefore, the current study's additive approach may contain significant biases in Shigar's estimated flows, and require a re-visit. In addition, other parts of the UIB, where additive approach has been used, needs revisit.

5. Most of the discussion and conclusions are based on statistically-insignificant trends. The authors should only focus on statistically significant trends.

6. Short time period hydro-climatic trends may not be true representative of climate

change impact assessment. The long term trends' results are not in good agreement with short term trends' results (Table 4-6), and could be an artifact of the selected short time period's data (1995-2012) for trend analysis. Such unexplained trends can be seen in the Astore basin (for example), where precipitation is rising for the Rattu station and declining for Rama station (see Table 5, page 643). Most of the monthly trends are statistically significant for both stations. This results in questions: such as which trends should be taken for discussion and which should be discarded and why?

7. The manuscript is very long with un-necessary descriptions, such as details about sub-basins. Such details can /should be presented in a Table rather than long descriptions. The authors should also avoid discussion about statistically insignificant trends.

8. There are many confusing/false/biased/without reference statements/arguments/estimates in the current study. Such as in Line 28, page 587 the glacierized area of the UIB has been estimated to be 18,500 km2 ($\sim$ 11.3% of total basin's area). Just on the next page, same paragraph (Line 3-4, page 588), the snow cover is estimated/quoted to be in the range of 3 to 67%, although no reference for the statement is provided (therefore can be assumed an analysis of the current study). Minimum snow cover area can be regarded as perennial snow and glacier cover area (Painter et al., 2012). Assuming the same, one will get a glacier area of about 4,905 km2 as opposed to a total of $\sim$ 18,500 km2 (mentioned above). Such statements need further explanation, and or should be avoided.

9. The authors have conducted homogeneity analysis, and found that some of the datasets are non-homogeneous. How good/bad are these datasets for further trend analysis? Some of the stations' data (e.g Bunji stations' temperature data) have already been evaluated and argued to be non-homogeneous (as mentioned in the paper), then how realistic could be the trend results of such data? The authors ignored homogeneity results due to non-availability of additional record/data, and used the stations' raw data. This arises a question that what is the significance of such an incomplete analysis or should this be included in this paper?

Specific Comments:

i. Line 14-18, page 581, where it is mentioned that around half of the surface water of Pakistan is derived from the UIB. What is the source or background of this information?

ii. Line 20, page 582, similar period should be replaced by same period.

iii. Line 21-23, page 582, which period's data have been analyzed by Sheikh et al. (2009)?

iv. Line 5-7, page 583, what is the time period of data analysis by Rio et al. (2013)?

v. Line 24-27, page 585, is this really the first study? I believe there are also some other recent studies, where high altitude data have been analyzed (see e.g Mukhopadhyay and Khan, 2014b; Farhan et al., 2014; Tahir et al., 2015; Mukhopadhyay et al., 2015).

vi. Line 13-16, page 586, needs a supporting Figure or Figure No (of the existing Figures).

vii. Line 2-4, page 587, the statement needs a reference, as this sounds to be taken from an available literature.

viii. Line 13, page 587, calculated should be replaced by estimated.

ix. Line 14-15, page 587, what is the source of void filled SRTM DEM?

x. Line 18, page 587, what projection system has been used for current study? There are also difference in current study's glacier cover estimates (besides using same glacier data) with available published papers, and could mainly be due to use of a different projection system. This can be noticed by comparing the glacier cover values with other available studies, for example the estimated glacier area for the Astore and Hunza basins in Table 1, page 638 are 527 km2 and 3815 km2, respectively, while for the same basins (and data) the areas are $\sim$543 (Farhan et al., 2015; Tahir et al., 2015; Khan et al., 2015) and 3860 km2 (Tahir et al., 2015; Khan et al., 2015; Mukhopadhyay and Khan, 2014a). Basin areas of Alford (2011), Sharif et al. (2013), and Young

and Hewitt (1988) are also within the same uncertainty level, hence are not examples of overestimated basin boundary. Therefore, limitation of use of different projection system should also be properly explained.

xi. Line 3-4, page 588, what is the reference of snow cover estimate?

xii. Line 1-3, page 590, it is argued that around 45% of total available surface water comes from the UIB. What is its source or how this has been estimated?

xiii. Line 10-13, page 592, glacier cover of the Astore basin is around 14%, while minimum snow cover 2-4%. How? Needs further explanation.

xiv. Line 8-11, page 593, is repetition of Line 13-15, page 583. Other such repetitions should also be discarded.

xv. Line 13, page 613, select should be replaced by selected.

xvi. Line 1-14, page 618, the authors should also consult Forsythe et al. (2015), which is about cloud cover variation in the UIB. In addition, warming influence varies with respect to altitude, therefore the authors should consult some relevant articles (such as Mountain Research Initiative, 2015), and should caution readers about their results.

xvii. Line 10-14, page 621, trends of different seasons and months are compared. How these are comparable?

xviii. Line 24-27, page 623, decline in July flows have been argued to be a sign of positive mass balance. However, this can also be due to negative mass balance, where available ice volume may has reduced, together with a reduction in July precipitation. Therefore, needs further explanation and elaboration.

xix. Line 10-11, page 624, flow trends have been argued to be mainly driven by temperature trends. This could be wrong. For example July flows and stations' precipitation are declining, and could be a main cause of flows decline (provided trends are true).

xx. Line 10-15, page 625; positive mass balance in the Karakoram. . .. . . Gardelle et

al. (2013) study only covers part of the Shyok basin (eastern Karakoram). A negative mass balance has been estimated by Kaab et al. (2012; 2015). Kaab et al. (2012) shows slightly negative mass balance in the western Karakoram and significantly negative in the eastern Karakoram. The latest study (Kaab et al., 2015) provide a significant negative mass balance in the eastern Karakoram (Shyok basin). Mukhopadhyay et al. (2015) also provide details about trends of the western and eastern Karakoram, and is good agreement with the mass balance studies. It is therefore, suggested to consult and include these studies.

xxi. Line 3-9, page 626, is an example of very long sentence. Necessary editing should be carried out for such sentences in the entire paper.

xxii. Use of article "the" is haphazard, for example in some places the authors write the UIB whereas at other places only UIB. Such minor English writing corrections should also be considered in the revised version, if any.

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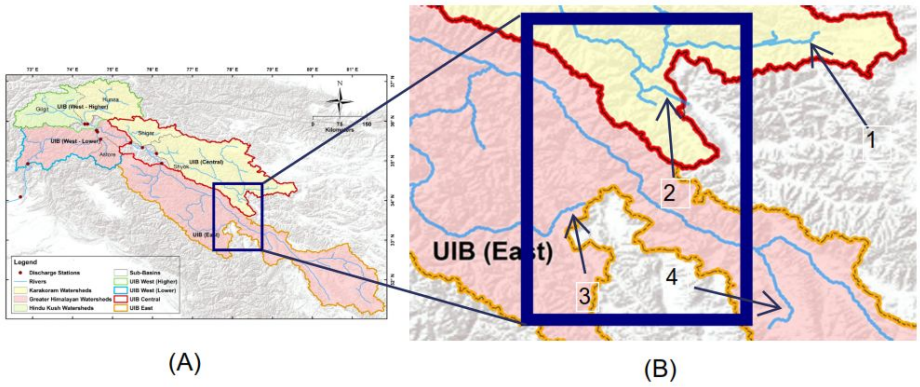
Interactive comment on Earth Syst. Dynam. Discuss., 6, 579, 2015.



(A) (B)

Fig. 1. Delineation errors. Figure A is manuscript's Figure 2 at Page 648, while B is zoomed part of Figure A.