We are thankful to the Referee # 2 for his/her comments, however, we respectfully disagree with most of them and thus with his/her recommendations. Following is our point by point response (in black) to his/her comments (in grey) along with the suggested changes in the revised manuscript (in blue).

Response to Anonymous Reviewer #2

#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) 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.

The presented analysis is based on a correct dataset, received after problem with the earlier dataset was communicated to the PMD. The following table shows a comparison of the long term annual precipitation with earlier studies. The figures given in the text will be corrected.

	Archer and Fowler (2004)	Sheikh et al (2009)	Tahir, 2011 and Tahir et al. 2011	Hasson et a., 2015
		1951-2000		
Astore	516.7 (1954-97)	512.8	501 (1954-2007)	454.7 (1962-2012)
Bunji	126.3 (1952-97)	151.1	-	163.8(1961-2012)
Chillas	-	192.7	-	184.3 (1962-2012)
Gilgit	131.2 (1894-1999)	133.8	132 (50-year record)	137.3(1960-2012)
Gupis	-	166.8	-	204.4(1961-2010)
Skardu	222.3 (1894-1999)	218.5	-	239.2(1961-2012)

#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.

Lines 8-20 page 587 will be revised as given below. Since the issue is also raised by the referee # 1, kindly refer to the detailed response to his/her major comments # 3.

As summarized in Reggianni and Rientjes (2014), total drainage area of the UIB has long been overestimated by various studies (e.g. Immerzeel et al., 2009; Tahir et al. 2011; Bookhagen and Burbank, 2010). Such overestimation is caused by limitations of the automated delineation procedure in a GIS environment that result in erroneous inclusion of the Pangong Tso watershed (Khan et al., 2014), which instead is a closed basin (Hungton, 1906; Brown et al., 20103). Khan et al. (2014) have provided details about the precise delineation of the UIB based upon ASTER GDEM 30m and SRTM 90m DEMs. For this study, the UIB drainage area is estimated from the lately available 30 meter version of the SRTM DEM, which was forced to exclude the area connecting the UIB to the Pangong Tso watershed in order to avoid its erroneous inclusion by the applied automated delineation procedure. Our estimated area of the UIB at Besham Qila excluding the Pangong Tso watershed is around 165515 km^2, which is consistent with the actual estimates of 162393 km^2 as reported by the SWHP, WAPDA.

#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.

In view of the new delineation of the UIB using SRTM 30 m DEM (Discussion Figure 1 below), this major comment is not relevant any more. However, it is to clarify that previously, ArcGIS basin tool was applied on the DEM, forced to an automated delineated UIB boundary that was buffered out to a certain threshold. The resultant small basins were combined together excluding the internal drainages identified by Khan et al. (2014); and, the river network was manually forced within the newly achieved boundary. Similar approach can apparently be noted from the Figures # 2 in the Mukhopadhyay et al. (2015) for the Shyok basin and from the Figure # 2 in the Mukhopadhyay and Khan (2014) for Zinskar river, featuring no uniform distance from the exterior boundaries instead rivers touching the watershed boundary. Also, kindly see response to Referee #1, major comment #3.

#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.

Since this issue is raised by the Referee # 1 as well, kindly see our detailed response to Referee # 1, specific comment # 25, where it is clarified that no attempt has been made to derive flows at the Shigar gauge and how the additive or multiplicative approaches are insensitive to the way discharge is derived for the Shigar-region.

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

We agree with the reviewer that most of the trends are statistically insignificant. However, we note that such insignificant tendencies feature a better agreement for the similar pattern/direction of change, which is interestingly further consistent to what has been suggested by the significant trends (discussion Table 1 in color scale in response to Referee # 1). We believe that such an agreement amid statistically insignificant trends, which are further consistent with the statistically significant trends, provide as valuable information as the statistically significant trends do. Thus, in view a shorter length of the analyzed dataset and sparse location of the analyzed observations, both the insignificant and significant trends collectively exhibit a consistent and detailed picture of prevailing changes over the regions and need to be discussed.

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

It is to clarify that stations at the valley bottom should not necessarily be in agreement with the high-altitude stations that are more representative of the topoclimate; however, still their better qualitative agreement with the valley bottom stations for spring (summer) months warming (cooling) suggests that the region is more-or-less under the influence of similar phenomenon. The period of 1995-2012 is considered not by choice but due to the limited accessibility of the high-altitude stations data. Moreover, trends over the period of 1995-2012 truly tell about the prevailing climatic state during such a period. Stations at the valley bottoms are also analyzed for the same period for sake of their comparison with the high-altitude stations over the same length of record.

For the Ramma and Rattu stations, it has already been explained on Page 588, lines 23-25, that the hydrology of the region is influenced by two large scale circulations, where such influence is further modulated by the complex terrain present in the region. The opposite change depicted by two stations may be a best example of such topographic modulation. Provided the abode stations in a particular region exhibit opposite responses, field significance is a best indicator to yield a dominant signal over that region, which can further be verified against the integrated signal of change from the stream flow record, as have been done in the manuscript. Recently published study of Immerzeel et al. (2015) have addressed in detail the precipitation uncertainty over the whole UIB, motivating the analysis of direct high altitude observations alike the presented analysis does in the manuscript.

#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.

Since this issue is also raised by the Referee # 1, kindly see our response to his specific comment # 7, in which we agreed to remove the description of the sub-basins as most of the information is already summarized in Table 1. For statistically insignificant trends, kindly see our response given above to the major comment # 6.

#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 (~ 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 ~ 18,500 km2 (mentioned above). Such statements need further explanation, and or should be avoided.

It is not true that the minimum snow cover area can be regarded as glacier cover area for the study region where substantial portion of the glaciers are under debris cover. Kindly again consult Painter et al., 2012 and also Rittger et al., 2013, who state inability of the employed MODIS MODSCAG product (which is based on spectral mixture analysis and is superior to the MODIS standard products) in detecting the debris covered ice and dirty snow. Second, the snow cover estimates given in the manuscript are based on Hasson et al. (2014b), who used the MODIS standard daily snow products, which too are unable to detect the debris covered glacier ice and dirty snow/ice. In addition to these, there are several other reasons that lead towards substantial differences between the minimum snow cover and the actual glacier cover, emphasizing not to regard the both as a proxy of each other, as explained in Hasson et al. (2014b) for the study region. Since the issue is not the focus of the study, such discussion will not be included in the revised manuscript.

#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?

It is to clarify that the statistically identified change points in the data (particularly when found only in the minimum temperature) may not necessarily be considered as an inhomogeneity until there is a documentary evidence stating the reason for such shifts in the data. Otherwise, in view of the high altitude topoclimate, role of topography in modulating the climatic effects, and also presence of substantial internal variability, shifts in the data may be present for real. Thus, it is not a pragmatic idea to dispose off the stations with statistically identified data shifts in view of lacking inhomogeneity evidence. Rather, it is more convincing to present the analysis from such stations raising caution to the reader and hoping any better explanation of such behavior in future. Moreover, the scarcity of stations within the region, and more importantly, the large consistency amid suggested changes by the stations featuring data shifts and those of homogeneous stations reinforces the idea to present the analysis from all stations, as have been done in the manuscript.

<u>Specific Comments</u>

1. 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?

The authors have estimated it from the long term (1961/62-2005/06) mean inflows of Indus at Tarbela against the long term mean inflows at the River Inflow Measurement (RIM) stations of the Indus river system (IRS), including Ravi at Balloki, Sutlej at Sulemanki, Chenab at Marala, Indus at Tarbela, Jhelum at Mangla and Kabul at Nowshera. According to the WAPDA data, Indus at Tarbela constitutes on the average 43.2% of the total IRS inflows with a range between 38.2 and 51.7 % as minimum and maximum contributions during the maximum and minimum water availability years, respectively.

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

   ‘similar’ will be replaced with ‘same’

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

   The analysis period of 1951-2000 will be mentioned.

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

   The analysis period of 1952-2009 will be mentioned.

5. 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).

   It is agreed that few studies, appeared online in late 2014 or in 2015, have presented only the subset of the data from few of the automated stations analyzed in the manuscript, for a relatively shorter period and mainly as a supported/side analysis. For instance:

   - Farhan et al. (2014) have used the Burzil station, which is in fact outside the UIB and located in the Jhelum basin. Thus, it is not relevant here.
   - Mukhopadhyay and Khan, 2014b have used mean temperature and precipitation from the Shigar station only for the 1999-2010 period.
   - Mukhopadhyay et al., (2015) have used mean temperature and precipitation from only four stations of Naltar, Ziarat, Khunjrab and Hushe for the 1999-2010 period.
   - Tahir et al., 2015 have used mean temperature and precipitation from the Ramma and Rattu stations for 1995-2008.
   - Mukhopadhyay and Khan (2014b) have graphically shown the annual cycle of precipitation for the unknown period.

   None of the above studies has presented the mean temperature and precipitation from five high altitude stations of Deosai, Yasin, Ushkore, Dainyor and Shendoor. More importantly, none of the above mentioned studies has presented the minimum and maximum temperature datasets from any of the high altitude stations.

   Nevertheless, ‘for the first time’ will be removed from Page 585, line 26.

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

   Here, we will refer to Figure (2), in a sequence.

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

   Archer (2003), Fowler and Archer, (2006) and Hasson et al (2013) will be cited

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

   "calculated" is generally used in a GIS environment for areas and geometry calculations.

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

   Instead of void filled SRTM 90m DEM, the 30 meter version of SRTM DEM available from the U.S. Geological Survey will be used in the revised manuscript. Kindly see response to Referee # 1 major comment # 3.

10. 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 ~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.

    The WG84 and UTM projected system for the North 43 zone has been used for areal estimates. Given that the projection is equal area, it should not be the reason of small differences in the areal estimates. Kindly note that for the same basins, estimated drainage areas amid above studies are not the same, for instance, it ranges between 3903 and 3990 for the Astore basin. In fact, small differences in the drainage areas may arise due to slight along-stream shifts while snapping the outlet to the accumulated raster for delineation. Thus, small differences in the basin shapefile can create small differences in the glacier estimates.

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

    All snow cover estimates are based on Hasson et al. (2014b), which will be added here.

12. 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?

    Since it is repeated, kindly see answer to specific comments (i).

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

    Hasson et al. (2014b) have explained that the minimum snow cover does not necessarily corresponds to the glacier area due to debris covered portion of the glaciers as well as due to skill (though limited) of the MODIS snow products in differentiating between the snow and the glacier ice. Anyhow, this text will be removed in response to specific comment # 7 from the referee #1.

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

    The repetitions will be removed

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

    It is to clarify that here, 'select' has been used as an adjective not as a verb

16. 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.

    Forsythe et al., (2015) will also be consulted here. The signal of elevation-dependent warming is not clear among the studied stations as already shown in Figure 7, and will be discussed in relation to the MRI, 2015.

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

    The text will be removed.

18. 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.

    In view of the overall stable areal extent of the regional glaciers (Bolch et al., 2012) and typical surface melting property of the cryosphere, it is not the case that a negative mass balance of few centimeters (Kaab et al., 2015) can explain reduction in the discharge, until the available energy for the melt is reduced, as already explained. Further, kindly see on Page 626, line 13-24, explaining how reduction in the solid precipitation has ironically an opposite effect on the melt discharge. The reduction in rainfall however may reduce the discharge, but meager amounts of rainfall received in summer months do not yield perceptible river runoff, particularly when the evaporation is considered (Mukhopadhyay and Khan, 2014). Thus, the case presented above is highly less likely.

19. 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).

The July discharge is largely generated from cyrospheric melt and only little contribution comes from the rain (typically true for even whole high flow period - Archer and Fowler, 2004; Mukhopadhyay and Khan, 2014). Thus, changes in the available energy for melt are mainly responsible for the discharge perturbation. Further, the influence of precipitation on discharge is already explained on Page 626, lines 13-24. Kindly also see response to the specific comment # 18.

20. 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.

    The above referred contradictory findings will be briefly mentioned in the manuscript.

21. 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.

    Long sentences will be shortened.

22. 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.

    The use of article will properly be taken care of.

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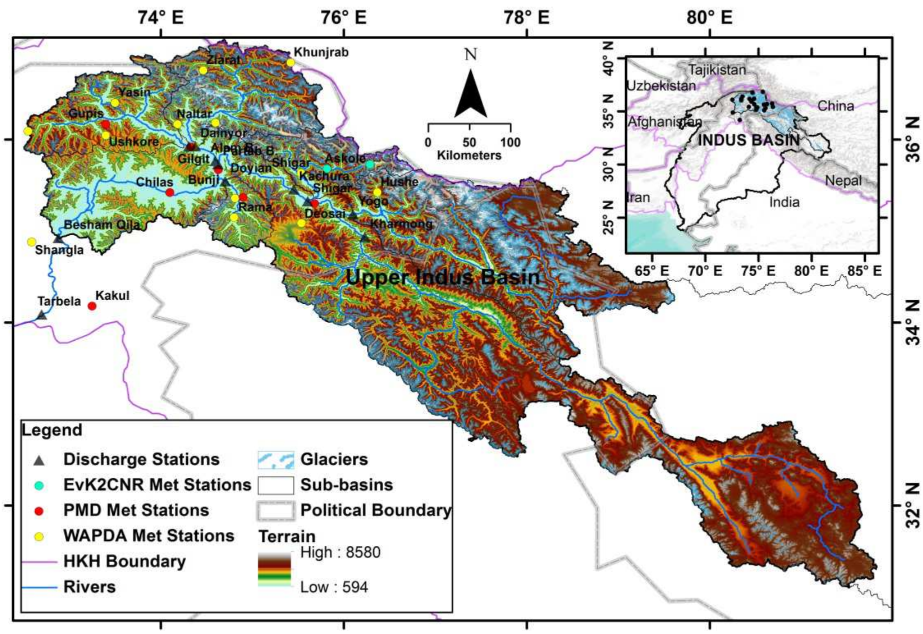

Discussion Figure 1: The UIB delineated from the SRTM 30meter DEM. The stream network based on coarser DEM is shown for illustrative purpose