Response to the Referee # 1

We thank the referee for his comments. However, we respectfully disagree on most of the referee's comments and thus his/her recommendation. Following is the point by point response (in black) to his/her comments (in grey) and the agreed changes in the revised manuscript (in blue).

Major comments

1. The paper is too long. Lot of information, already known through earlier publications of different researchers, are repeated or falsely presented as new materials (and this is a severe problem with this paper). The unnecessary wordy sentences and redundancy of various statements have contributed to the length of the paper to become annoyingly long.

It is to clarify that the paper is seen for a broader audience and submitted to an inter-disciplinary and multi-disciplinary journal of the Earth System Dynamics where articles ranging from the Geoengineering to the thermodynamics to the socio-economic issues are published. In view of the broader audience, it is indispensable to present basics about the study area and its hydroclimatology, the present status of research etc.

However, without considerable loss of information, the revised manuscript will be condensed to the extent possible and redundancy will be removed.

2. The English of the paper is not free flowing. Sentence constructions in many places are awkward. In places, certain phrases or words are used strangely. There are grammatical errors. There are excessively long and loquacious sentences which make the readability of the paper very poor. The paper should be copy edited by someone with a better command on the English language. [To give some examples, look at Lines 7 – 9 on page 585 – Does it carry any substance or is it just a gibberish to create a place for self-citation?; or .look at Lines 14 – 18 on page 581 or read Lines 14 – 16 on page 585; Lines 7 – 12 on page 586; there are plenty of such examples throughout the paper].

It is not agreed that readability of the paper became poor due to long sentences and (strange) phrases, as noted from the examples given by the referee. For instance, on Page 585, lines 7-9 introduce the diversity of the UIB in terms of its contrasting hydrometeorology and abode cryosphere, and that, such diversity is defined by the interactions between two large-scale circulation modes and their modulation by the complex HKH terrain. In order to introduce the field significance analysis, which the referee liked the most, given information on the diversity of the UIB and sparse meteorological network was thought necessary to be reported first. For further details the reader is directed to the recent work from the authors as suggested by the referee under point #1. Further, it is to clarify that since the cited authors' publications are further cited at relevant places in the article, there was no need to create a place here for self-citation.

For Page 581, lines 14-18, Page 585 Line 14-16 and page 586 line 7-13, it is very much clear what has been said.

However, the grammatical errors will be corrected and efforts will be made to improve the readability of the manuscript.

3. The tenor of the language used in the paper is repelling to workers interested in this area of research. The underlying tone of the paper is that the authors are the ones who for the first time have done a thorough comprehensive job in everything presented in this paper and with the exception of a few, they either give a little credit to previous works that are also repeated in this work or give no credit to some earlier works by not referencing those. This is

tantamount to academic dishonesty. For example, the authors "reinvent" delineation of UIB and provide a lengthy discussion on how their delineation is by far the best and give a cursory mention of the work of Khan et al. (2014) [Line 17, p. 587]. But the fact of the matter is that Khan et al. (2014) have already resolved the issue of proper delineation of UIB and their estimate of the area of UIB up to Besham Qila is as good as that is presented in this paper. This sort of self-crediting, self-gratifying, and self-congratulatory writing easily alienates other researchers in this area and does not help the authors to achieve the very objective of theirs in writing so – i.e. to establish credibility and earn respect for their work. On the other hand if the authors review all relevant previous work and give due credit to those then they would easily earn the trust and respect of the peers familiar with the topics presented in this paper. In that process, if the authors disagree with any of the earlier studies that is fine. However, the reasons for such disagreements must be backed up with sufficient analysis and convincing arguments and must be presented respectfully without trying to just trash those out simply because the authors have conducted a "reanalysis of the same data" used by some of the previous workers.

The use of the word "repelling" has no place in a scientific debate. We kindly urge the reviewer to take it back. We continue the review putting this major issue of academic respect aside.

The referee first raises a serious allegation of academic dishonesty in a dramatized way for giving a little or no credit to the previous work, and in last, asks for whether there is a disagreement. It is to clarify that some publications have appeared during the preparation of the manuscript and since its submission (from second half of 2014 till now), and the authors already intend to refer such lately published articles in the revised version in order to comprehensively summarize the previous findings, regardless of the fact that the manuscript is not a review paper.

For citing previous work, it is to clarify that in the specific Comments # 7, referee asked to replace the Archer, 2003 and Fowler and Archer, 2006 with Mukhopadhyay and Khan (2015). Since the suggested study came up during or after the submission of the manuscript, how could the authors cite such a study? Note similar case for the specific comments # 1.

Interestingly, in the specific comments # 2, the referee seeks citation for the Mukhyopadhyay and Khan (2014a) considering it a better and more recent reference. However, the study does not present any concrete supportive analysis, as desired by the referee himself in case of his specific comments # 1 and #7. On the other hand, disagreement with the Mukhopadhyay and Khan (2014b) is already given in the manuscript on Page 601, lines 9-19 and reinforced in the response to specific comments # 25.

For Khan et al. (2014), it is to clarify that authors have delineated the UIB for their own work, as anybody else will do it for his own work. Thus, the authors have reported their work in a way it has been carried out, as anybody else will report their work as they would have done it. During the UIB delineation, the Pangong Tso and small internal drainages have been eliminated based upon the conclusion reported by Khan et al. (2014), for which due credit has been given by citing the study. Against this background, it is beyond understanding that what kind of credit the referee wants for Khan et al. (2014) from the authors and what leads him to be highly obsessed with this study. The referee might think that after Khan et al. (2014), nobody else is allowed to delineate the UIB. It is also to clarify that in fact, Khan et al. (2014) are not the first ones who said the Pangong Tso drainage is a closed basin. Such fact is already well established over more than a century by the published geological studies and field surveys and recently by others (e.g. from Hungtington, 1906 and before to Alford, 2011, as cited by Khan et al., 2014 themselves). It is also depicted by around half-century old UIB drainage area estimates from the SWHP WAPDA reports.

The Khan et al. (2014) has been cited in the manuscript as they have lately investigated the relationship of the Pangong Tso with the Indus basin and discussed based on the SRTM 90 and ASTER GDEM V2 30m DEMs that the lake is roughly 24-28 meters lower than the critical lake drainage barriers. Being curious to the referee's obsession, it is learnt that such additional evidence is however highly uncertain in view of the reported vertical accuracy of the employed DEMs and their precision required for this specific analysis.

For instance, it is implicitly assumed that the vertical accuracy of the ASTER GDEM V2 estimated over the US (i.e. ±17.01 meters at 95% confidence interval with full range interval of -137.37 to 64.80 meters) is equally applicable in a highly complex terrain of the Karakoram. Even though it is assumed to be true, such vertical accuracy is not precise enough to be certain to accurately identify the real height difference between lake level and critical points. Similarly for the SRTM, Farr et al. (2007) have been cited for linear absolute height error of less than 16m at 90% confidence interval but unfortunately not for their statement that "*… the greatest errors are associated with steep terrain (Himalayas…*", which implies that the rest of 10% confidence interval should equally applies to this region of high relief and not to another planet. Further, the reported accuracy is based upon 1/8^{o} resolution and mainly contaminated by a random error, thus it is not equally applicable on a specific 90meters grid cell. In view of different vertical datum and intrinsic problems of the instruments for heterogeneous surfaces in a high relief area, the reported vertical accuracy feature high uncertainty for such a precise analysis.

The inter-dataset differences further reinforce the uncertainty issue. For instance, height of the critical point 3b in SRTM and GDEM v2 is offset by 7 meters, which is roughly an order of magnitude difference between height of critical point 3b and lake level in SRTM. In fact lake surfaces were very 'noisy' in the original DEMs and set to constant heights afterwards. Even then, the most reliable lake level height derived from ICESat altimetry data is 4219.68 m on 08/10/2004 (Srivastava et al., 2013), suggesting that SRTM and GDEM overestimate lake level by 22 and 10 m, respectively. When considered over the complex terrain and heterogeneous surfaces, the inter-dataset difference is expected to be even large.

Against this background, investigation of the critical points being few meters higher or lower than the lake level is an application the employed DEMs are not yet tested to be suitable for, in the study region. In view of such uncertainty associated with the additional evidence, it is more convincing to believe earlier studies stating that the Pangong Tso is a closed basin, and subsequently, not excluding the small internal drainages. In view of "reanalysis of the same data" comments, recently available 30-meter version of the SRTM DEM is considered as a more appropriate choice for re-delineation (Kindly see the discussion Figure 1 in response to the referee # 2).

Moreover, though the limitation in finding and filling sinks in the DEMs is already explained in the ArcGIS online help and in the respective publications, Khan et al. (2014) have shown how such limitation applies to the UIB delineation case, for which of course the study will be cited. In this regard, the text on page 587, Lines 8-20 will be revised (Kindly see the response to the major comment # 2 of the referee # 2. Since the present manuscript is not a right forum to discuss the UIB drainage issues and DEM accuracies, the above discussion will not be included in the manuscript and deemed as distracting from the main subject of the manuscript.

4. The authors' claim that they are using, "for the first time observations from high altitude automated weather station" [Abstract, Line 8, p. 580; Introduction, Line 24, p. 585; Discussion, Line 16, p. 615) is a false claim. Mukhopadhyay and Khan (2014b) and Mukhopadhyay et al. (2014) have already used those data and noted that no trends could be established from those data due to the very short period of record and the scatters present in those observations.

Since this issue of ‘for the first time’ has also been raised by the referee # 2, kindly see the combined response to his/her specific comments # 5.

It is to clarify that based upon a 12-year time series from only four stations Mukhopadyay et al. (2014) have stated that no trend can be established. If it is assumed true, how results from a 12 year time series can be generalized to 18-year time series (with 50% increase in length) from the same stations? Further, how can the results of no trend from four stations with shorter period of record be generalized for the rest of 8 stations not analyzed by Mukhopadyay et al. (2014)? Further, Mukhopadyay et al. (2014) have stated that “*Because the stochastic component is often large, simple regression often results in trends that are statistically insignificant and thereby can be erroneous.”* and implemented a non-parametric trend test procedure with a benchmark smoothing technique to analyze river flow trends. However, surprisingly, they still used a simple regression analysis for ascertaining a trend from four high-altitude stations, ?. It is to clarify that any conclusion based upon their findings cannot be generalized or equally applicable to this study, which in contrast applies a non-parametric trend test with a sophisticated pre-whitening procedure over relatively longer period of record for a larger set of stations.

5. The climatic data used from the automated meteorological stations cannot be used to establish any“ credible long-term climatic trends”. The period of record for those 12 stations is very short. In most cases the period is 1995 – 2012 (18 years, i.e. not even two recent decades) and in some cases it is even shorter (e.g., 17 Aug 1998 – 31 Dec 2011 at Deosai, 15 Jan 1997 – 31 Jul 2012 at Dainyor; and 27 Aug 1996 – 31 Dec 2012 at Shigar). The authors use this period of record for the low altitude stations also [Page 596 (Line 20)]. The actual success of the statistical method implemented here, regardless of its level of sophistication, in establishing meaningful trends in the climatic variables extracted from those station records, is very much apocryphal.

Since the data from high-altitude stations is maximum of 18-years length, neither is it claimed nor any effort has been made to establish “… long-term climatic trends” as said by the referee. The title already makes this very clear. The effort is to present the prevailing climatic trends during the analysis period, based on the maximum available and accessible observational record, and applying sophisticated method in a systematic way. This period of record (1995-2012) has been used for low altitude stations, first in order to furnish a complete picture from all stations for the same time period, and secondly to present a comparison of the prevailing observed climatic changes between the high-altitude and low altitude stations.

Is data being exactly of two decades ensures that the trends will be significant? Or it guarantees that the 18-years data will not feature any significant result? In any of these cases, reference is solicited. The data presented here for most of stations is 18-years, which is beyond the minimum time series length requirement for the Mann-Kendall trend test for detecting a trend.

The TPPW method, applied here, uses lag-1 autoregressive process and hence it is particularly suitable for a long time series. Therefore, most of the results of the trend analyses presented in this study are highly doubtful. This is partially evident from the results presented in Tables 4 3 and 5 where most of the trends have no statistical significance. So the authors should state that fact and should only concentrate on those trends that are statistically significant.

Exactly opposite is true. The pre-whitening is particularly required for the shorter time series, for instance, of sample size n<=50 (Bayazit and Önöz, 2007; Yue and Wang, 2002). The cited studies noted that the effect of short memory process either becomes negligible or diminishes away for the longer time series. It is also to clarify that if the AR(1) in a time series is statistically significantly different from zero, it has to be removed for the reasons well explained in the manuscript and in the cited literature. Moreover, the pre-whitening procedure is mainly used to force the falsely high rate of rejecting the null hypothesis of no trend to nominal rate when trend in fact does not exists in a time series.

It is true that most of the trends are statistically insignificant. However, authors emphasize that a wider agreement amid statistically insignificant tendencies that is further highly consistent with the significant trends (Discussion Table 1) is almost as valuable as the statistically significant trends themselves, particularly in view of the data scarcity in the region. Both, the statistically significant and insignificant tendencies consistently suggest a general pattern of change over the study region.

Based on the above given discussion, particularly on the suitability of pre-whitening application, the authors have serious concerns about the doubts the referee has on the presented trend analysis. A careful consultation of the relevant literature cited in the manuscript and elsewhere is solicited in this regard, as amid series of publications; issues pointed out by one are resolved by others. Thus, only partly reviewing can lead to further confusions. A nice brief summary is therefore presented in the introduction and method sections of the manuscript for the multi-disciplinary readership.

6. The way authors have done flow analysis of certain discharge data clearly shows that the authors have ignored some fundamental rules of hydrologic flow balance and therefore there are serious errors in their hydrologic calculations.
7. The authors should understand that the additive (subtractive) method of flow balance in deriving flows at an upstream gauging station from the flow data from one downstream and couple of upstream gauges is fraught with errors (explained in details in the specific comments below). On the other hand the multiplicative (ratio and proportion) method is a much more robust method.

Since comments #6 and #7 are repeated in the specific comment section, kindly find the response to these comments in the respective section under specific comment # 25 and # 26.

8. The authors have attempted to explain the trends in discharge in the light of trends in temperature only. However, temperature is an inappropriate proxy to the energy input that causes snow and glacial melting in the elevation range of 3500 – 5500 m in UIB. Not temperature, but insolation is the prime source of energy for the cryospheric melting process in this terrain. So the explanations they offer are too simplistic and do not explain both rising and falling trends of river flows at various locations of UIB.

It is to clarify that though the insolation is a prime source of energy however it is not solely responsible for the cryospheric melt processes, understanding of which in fact requires a precise estimation of available energy budget. For instance, regardless of changes in the insolation, energy budget can be perturbed by the albedo in case of fresh snow events and that such events are inversely proportional to melt water availability as explained in the manuscript on Page 624, lines 15-23. Moreover, wind speed/air mass stability is another factor, which can considerably perturb the cryospheric melt processes. Thus, any conclusion drawn on solely the insolation will also be too simplistic. Moreover, availability or accessibility of the relevant variables that are required for the computation of fully resolved energy balance is much more difficult in such a data-sparse study region as compared to temperatures. Thus, in order to fully explain the melt processes and their relationship with the climatic and flow variables, authors should change their approach and use hydrological and radiative transfer models, which is beyond the scope of this study. However, authors take this suggestion as a possible input to the future work, more oriented on the modelling of melt-runoff from the region.

9. The main contributions of this work are actually given in pages 604 – 629. However, by the time a reader arrives here he/she is already tired of reading pages 580 -604 (half of the paper with no new substance). So the authors are strongly advised to write the background, data, and method very succinctly and then condense the result and discussion section so that the reader can remain focused on the key findings and does not get lost in the maze of longwinded discussion.

Since this comment is not different from the major comment # 1, here response is the same. The manuscript will be shortened to the extent possible, but without considerable loss of information in view of targeting the multi-disciplinary readership.

10. The authors find the trends of the climatic variables for the period 1995 – 2005 different form the trends for the period 1961 – 2012. As noted above this is perhaps an artifact of the short period (for the high-altitude climatic stations) which does not really allow to detect any long term climatic trends

It is reiterated that no 'long-term climatic trends' are intended from the 1995-2012 period. Instead, focus is on the prevailing patterns of change during this period as depicted by high altitude stations, which are relatively more representative of the high altitude climatic patterns. Trend analysis over 52 year period suggests prevailing pattern of trend changes over that period and trend analysis over recent 18-years suggests findings for that period. How it comes that the trends over the short period only from the high-altitude stations are subject to an artifact? Kindly see details in response to major comment # 5.

Specific Comments

1. Page 581 (Lines 25 – 27) – Page 582 (Line 1): First of all, snowmelt and glacial melt contributions to river flows do not remain constant. They vary with location as well as season. Second, none of these references you cite here provides the quantitative estimates of snowmelt and glacial melt contributions to river flows in UIB. None of these works has seriously made any attempt to estimate these proportions. On the other hand there is a recent study that is exclusively devoted to this problem (Mukhopadhyay and Khan, 2015, Journal of Hydrology, 527, 119 - 132). Consult this reference and rewrite this section.

   This is not true. The SIHP, 1997 states the fact based on extensive field work over several years, while Immerzeel et al. (2009) state quantitative estimates based on a multi-year modelling study that incorporates inter-annual variation of and compensation between the snow and glacier melt. The comment is however only true for Archer and Fowler (2004) who state this fact without supportive analysis. Since lately available 'exclusively devoted' study of Mukhopadyay and Khan (2015) has presented similar fact based upon distinct analysis of hydrograph separation, the study will be cited in place of Archer and Fowler (2004). The results from all these studies consistently support what has been said on Page 581, line 25-27.

2. Page 583 (Lines 13 – 14). There are better and more recent references than SIHP (1997), e.g. see Mukhopadhyay and Khan (2014a, Journal of Hydrology, 509, 549 - 572). Also see Archer (2004 in Nordic Hydrology) for altitudinal shift of thawing temperatures.

   Since the SIHP report is based on multi-year extensive field work covering wider area of the study region, this seems to be more relevant reference suggesting active hydrologic altitudinal range as given in the manuscript. None of the mentioned studies present this fact backed by a concrete analysis, as desired by the referee in the specific comment # 1 and # 7.

3. Page 584 (Line 4). The stochastic component of a time series is called "white noise" NOT "red noise". Do not use wrong terms.

   In an AR(p) process the signal is indeed a red noise. The "forcing" term on the rhs of the equation describing the process is a white noise process. The AR(p) process is the stochastic component on top of the deterministic, slow trend or time modulation. So it is a red noise. These terms are well known and already explained briefly on page 599, lines 3-10 and thus need not to be explained further.

4. Page 585 (Lines 13 -14). Explain here what is meant by "field significance". I know you have explained it later on page 600 (Linea 11 – 13).

   "field significance" will be briefly explained on Page 585, Lines 13-14 as well.

5. Page 586 (Line 12 -13). There is no diverse hydrologic regime within UIB. The hydrologic regimes throughout the UIB are uniform as evidenced from the uniform characteristics of annual hydrographs from various parts of the basin [see the discussion on hydrologic regimes in UIB as given in Mukhopadhyay and Khan (2014a)]. It appears that you are making the same mistake as Archer (2003) did in calling hydrologic regimes for different genetic sources of river water. See Krasovskia (1995) for the correct definition of hydrologic regime (reference given in Mukhopadhyay and Khan, 2014a).

   Instead of Krasovskia (1995) the flow regimes are in fact originally defined in Krasovskia (1994) mainly for the study area of the FRIENDS (Flow Regimes from International Experimental and Network Data) project. The following extract and the Table 2 from the Krasovskia (1994) clearly suggest the sub-types of high flow regime as the Mountain nival and Mountain glacial flow regimes as quoted below:

   "Mountain regime types have in general the same character as the NorthScandinavian type, with a distinct maximum in late spring/summer and low flow in winter. They occur at altitudes higher than 500 m. The nival sub-types are characterized by earlier maxima compared to the glacial-fed sub-types which have their maximal flow later in summer."

   In Table 2, Krasovskia (1994) clearly name these types of flow regime as Mountain Nival and Mountain Glacial. These sub-types of high flow regime can easily be differentiated based on peak flow timings as stated in the manuscript on Page 589, lines 232-26. Since the sub-regions within the UIB exactly feature Mountain nival and Mountain Glacial flow regimes, the statement given in the manuscript is correct. Thus, neither the Archer (2003) is mistaken nor the authors blindly followed him.

   Moreover, in view of the multi-disciplinary nature of the manuscript and the targeted audience, it seems strange to codename these sub-types of high flow regimes as H1 and H2 only as done by the Mukhopadyay and Khan (2014a). Instead, it is more convenient to name them as have done by Krasovskia (1994) himself.

6. Page 586 (Line 23). So you are now giving us the "right direction" and all previous workers were so stupid that they provided wrong directions, ha? Stop such self-patting. It does not help your cause.

It is to clarify that “right direction” for the climate community here particularly emphasizes on the water availability assessment from the region additionally under the prevailing climatic trends, since neither any of the study so far (to the best of authors’ knowledge) has considered summer cooling nor the climate models are able to reproduce or project such phenomenon. As a result, the climate impact studies suggest signs of change, even for the near future water availability, exactly opposite to what is expected under the prevailing climatic patterns. Kindly see detail on Page 626, lines 13-22 and in Hasson et al. (2014b).

7. Page 587 – Page 592: Section 2. All of the information given in this section are well known and have been described by various previous workers. You need to condense this section to couple of paragraphs

   It is realized that explanation of the sub-basins of the UIB is to-some-extent already summarized in Table 1. Thus, (03 pages of) text between the Page 590, line 6 and Page 592, line 20 will be removed. For the text between page 587 and 589, as stated in response to comment # 1 above, the multi-disciplinary audience does not necessarily know the region and its physio-geographical and hydro-climatic characteristics and related peculiarities. Thus, it is not convincing to shorten this introduction of the study area.

   giving proper reference to previous works [e.g. refer to Mukhopadhyay and Khan, 2015 in relation to Lines 14 – 21 on page 589; Archer (2003) and Fowler and Archer (2006) are not the relevant references in this case since in those work this particular issue has not been addressed].

   Based upon correlation analysis with valley-based stations and discharge, Archer (2003) has presented the distinct hydrological regimes, which have been reiterated in Fowler and Archer study. Lately, Mukhopadyay and Khan, 2015 have concluded similar facts through hydrograph separation analysis. The Fowler and Archer reference will be replaced with Mukhyopadyay and Khan on Page 589, line15.

   This is not your Ph. D. thesis where you need to write all background information to satisfy you supervisory committee. Readers familiar with UIB know all of these very well and they get irritated when they see that you are presenting this material as if for the first time someone is describing this river basin and providing all those details.

   What about the readers not familiar with the UIB? The response to such repeated comment is already given in major comment # 1 and # 9.

8. Page 592 (Line 25). Delete “data collection”. Just “three different organizations” [they are not just data collection organization; also phrasing of the words is wrong].

   Regardless of what else these agencies do, here have been introduced particularly in the context of data collection. However, “data collection” will be removed as it does not affect the clarity of the sentence.

9. Page 593 (Lines 9 -10). Repeated from Section 2. Do not repeat statements or information. Also in this regard (“active hydrological altitudinal range” – strange phrase) – see Fig. 8 in Mukhopadhyay and Khan (2014a).

   The expression “active hydrologic altitudinal range” will be replaced with “active hydrologic zone”, exactly as stated by the SIHP, (1997). Repetition will be removed.

10. Page 593 (Line 15). Instead of "solid moisture input (another awkward phrase) simply say "snow" or "snowfall". Also hydrology is NOT dominated only by snows (seasonal snow to be more precise), but also by glacial melts. So your statement here is not correct.

    It is to clarify that regardless of the fact that it is ephemeral, intermediate or perennial snow, firn, clean-ice or debris-covered ice etc., the hydrology of the region dominates with the solid moisture melt. For general clarity, "input" will be replaced with "melt".

11. Page 593 (Lines 28 -29). No; they do not cover "most of the vertical extent of …..altitudinal range". Most of the frozen water reserves are above 3500 m and extends all the way up to 8000 m. There are only couple of DCP stations above 3500 m (e.g. Deosai and Khujerab) and only a few above 3000 m.

    On Page 593, line 29, 'the vertical extent of UIB frozen water resources and' will be deleted as statement is only appropriate for the active hydrologic zone which extends up to roughly 5300-5500 m asl only.

12. Page 594 (Lines 19 – 20) – Delete – It is a nonsense sentence (gauge stations are not based on "distinct hydrologic regimes and magnitude of runoff contributions" they are carefully placed to gauge river flows of all major tributaries and main stem of the Upper Indus).

    It will be deleted.

13. Page 594 (Lines 21 -22) and Table 3. Shigar gauging station does not have continuous data from 1985 – 2011. The continuous data are only from 1985 – 1998 and then there are data for one year that is 2011. Get your facts straights.

    It is to clarify that on Page 594, lines 21-22 authors are talking about the availability of sub-basin gauges, and not the data availability from these gauges. However, thanks for pointing out this overlooked piece of information, which will be explicitly stated in the table 3.

14. Page 595 (Line 12). "limited skill" – another strange use.

    Authors don't see any problem with this expression. A few ready references are Liu et al., (2015), Maurer and Hidalgo, (2008), Jiang et al., (2009), and elsewhere, many more …

15. Page 595 (Line 25). Another wordy sentence with little weight.

    The sentence indicates reasons to justify why the relative homogeneity was performed instead of using a reference time series. It will be shortened.

16. Page 596 (Line 20). This period of record (1995 – 2012) is too short to detect any meaningful trend.

    Since this comments is repeated, kindly see the response to major comment # 5.

17. Page 598 (Line 2). Should be S NOT Z.

    Why not *Z.* It can particularly be S when n <= 10 and directly compared to probabilities table without calculating its variance and standardized normal variable, Z.

18. P 598 (Line 10). Say white noise, not “noise process”.

    No. It is not necessarily the white noise only but can additionally be an autoregressive process, indicating sequential dependence of the time series. Kindly see response to specific comment # 3 and the relevant literature cited in the article.

19. Page 599 (Line 6, Eq 8). The yt in this equation is not the same yt in Equation 6. Change symbol. Also, add t in this equation.

    In fact equation 6 showing a linear trend approximation can directly be referred here. So, the equation 8 will be removed. The ε_t refers to the white noise and it is shown in Eqn. 9.

20. Page 599 (Lines 10 – 25) and Page 600 (Lines 1 – 9). This procedure is valid for a long time series. For such a short time series (1995 – 2012) this is an overkill and the results are doubtful.

    No. This procedure is particularly required for shorter time series and not necessarily needed for n >= 50 (Bayazit and Önöz, 2007; Yue and Wang, 2002), as the effect of short memory diminishes or becomes negligible for longer time series. Since this comment is repeated, kindly see detailed response to major comment # 5.

21. Page 600 (Lines 11 – 13). Rewrite this sentence with correct grammar.

    The sentence will be corrected.

22. Page 600 (Line 15). You cannot divide UIB into smaller units based on hydrological regime. Obviously you don’t now what is meant by “hydrological regime” and are using the term completely ignorantly. There are two hydrological regimes throughout UIB. One is the high flow regime (May to September) and the other is low flow regime (October of a year to April of the following year). What you mean here is actually predominance of different genetic sources of river water (e.g. snowmelt dominant over glacial melt and vice-versa). Read Mukhopadhyay and Khan (2014a) for a better understanding of the distinction between hydrologic regimes and genetic sources of river flows. You have fallen as a victim of the misconception introduced by Archer in his 2003 Journal of Hydrology paper.

    Since the comment is repeated, kindly see the detailed response to specific comment # 5, where definitions of the hydrological regimes are clarified and relevant literature is referred.

23. Page 600 (Line 24). Same problem as noted above.

    Kindly see the detailed response to specific comment # 5, as stated above.

24. Page 601 (Line 8). Wrong information as noted above. Shigar gauging station does not have continuous data from 1985 – 2011. The continuous data are only from 1985 – 1998 and then there are data for one year that is 2011. Get your facts straights.

    It is to clarify that nowhere in the manuscript it is suggested that the Shigar gauge has continuous data for 1985-2011. May be the referee means 1985-2001 period instead of 1985-2011 period. Any case, here purpose is to state that the Shigar gauge went non-operational after 2001. The continuous data availability for the 1985-

1998 period and then for the year 2001 will be stated in the Table 3, as mentioned in the response to specific comment # 13.

25. Page 601 (Lines 10 – 24). The method used here for the calculation of derived flows at Shigar is wrong. It is because the reach lengths between the upstream gauges and a downstream gauge are significantly long. Throughout those long reaches flows from numerous other tributaries join the main stem and contribute to a downstream gauge. So subtraction of the sum of two upstream gauge flows from a downstream gauge flow gives substantial overestimation of the derived flows at a third upstream gauge. For example, excepting Shigar gauge, the only other two gauges upstream of Kachura are at Kharmong and at Yogo. So if you subtract sum of Kharmong and Yogo flows from Kachura flows to derive flows at Shigar then you are completely ignoring other flows that originate and contribute to Kachura from the points of gauging at Kharmong and Yogo and are assuming that only flows from Kharmong, Yogo, and Shigar contribute to Kachura. This process gives wrong flows at Shigar. In other words, the additive (subtractive) method of flow derivation is not a valid method. On the other hand the method of using flow ratios (as implemented in Mukhopadhyay and Khan, 2014b) is much more robust even if time-averaged ratios of flows at upstream and downstream gauges are used since the ratio of flows at two points is independent of contributions of other flows between these two points (assuming if there is any increase or decrease in flows then it affects all contributing streams in the same way).

It is to clarify that no attempt has been made to derive the flows right at the Shigar gauging site. The expression given in the Table 1, serial no.11 and explanation given in the text on page 601 lines 19-24 clearly suggest that flows are derived for the region comprising the Shigar sub-basin itself and all the extraneous area not represented by two upstream gauges of Kharmong and Yogo (shown without color in the manuscript Figure 2). Such area is already named as derived-Shigar in Table 1, serial no.11.

To avoid confusion, first the equations 11-13 will be removed and only Table 1 will be referred. Second, the region will be renamed as Shigar-region in the Table 1 and lines 19-24 will be revised as following:

"On the other hand, instead of estimating post-1998 discharge at the Shigar gauge, we have derived the discharge for the Shigar-region, comprising Shigar sub-basin itself plus the adjacent region shown blank in the Figure 2. This was achieved by subtracting the mean discharge rates of all gauges upstream Shigar gauge from its immediate downstream Kachura gauge at each time step of every time scale analyzed."

The reason for estimating the Shigar-region discharge is well explained on Page 601, lines 15-20 that coefficients identified from the pre-1998 period cannot be assumed time-invariant for the post-1998 period, in view of large drainage area upstream and also due to the distinct discharge trends present for the upstream gauges. This reason is further supported by Mukhopadyay and Khan, (2014b) themselves, who stated that since the correlation between the Shigar and Kachura gauges during the pre-1998 period was not constant in time, the generated post-1998 flows for the Shigar gauge have greater uncertainties than its pre-1998 flows. The variable snow and glacier melt contributions as stated by the referee in the specific comment # 1 also reinforce this fact. Given that the found relationship between two time series is variable in time over the known period, what guarantees that it will be time-invariant for the unknown period, and particularly when upstream flow series are non-stationary? Against this background, no attempt has been made to generate the missing flow records for any gauge. Instead, flows from the Shigar-Region and from the other ungauged regions are derived from the upstream-downstream gauges. For this, the additive approach is applied at each and every time step of the considered time scale (monthly to annual), which ensures application of time-variant relationship/factor. It is to clarify that both the additive or multiplicative approaches in the context of time-variant relationships for each time step, yield exactly the same results.

The time-variant relationships between the Shigar and Kachura gauges as found by Mukhopadyay and Khan, (2014b) are mainly due to the active memory processes that occur at various temporal scales. Thus, the derived flow series obtained through either additive (expressions given in Table 1) or multiplicative approach are only an approximation of the measured flow series. In Table 1, 'Expression of Derived discharge' will be replaced by 'Expression for deriving approximated discharge'

26. Page 601 (Lines 24 – 29) – Page 602 (Lines 1 – 6). Strictly speaking, Equations (11) – (13) are not correct because they do not obey the fundamental principle of flow balance of hydrology. However, this limitation can be partially removed by using an approximation sign (≈) instead of equal sign in the equations.

    The equations 11-13 will be removed as stated above. However, in Table 1, 'Expression of Derived discharge' will be replaced by 'Expression for deriving approximated discharge' as stated in above.

27. Pages 602 (Lines 7 – 24) to Page 604 (Line 10). This is the only original contribution of this work. This part is relatively well written. However, based on the mathematics presented to illustrate the method of "field significance", it appears to me that this method is most reliable when there are several local stations in a region. In the sub-regions of UIB, defined in this work, there are two to three local stations and the areal extents of these sub-regions are too large (e.g. UIB East). I am not sure how good this analysis is, in spite of the fact this is the first time someone has attempted this (in sharp contrast to Archer and Fowler or Fowler and Archer who made big conclusions about climate change in the entire UIB based on a few local observations at valley floors). This is the part of your paper I like most.

    Authors are thankful to the referee for the appreciation that leads towards encouragement. As indicated by the referee, the problem of uneven distribution for the method is briefly discussed on Page 625, lines 3-10. Also, this is one of the main reasons that the field significance is further qualitatively compared with the discharge trends from the corresponding regions.

28. Page 614 – 616. Section 6. This whole section should be abridged. Everything stated here is superfluous. If your objective is to have an interested reader to read your paper then you need to capture his/her attention by making things short and succinct. Develop respect for a reader's time.

    The Section 6 will be shortened.

29. Page 622 (Line 25). Mukhopadhyay et al. (2014) is not in the reference list. Discussion should also include the trends for Yogo (eastern Karakoram) and Hunza (west Karakoram) as given in Mukhopadhyay et al. (2014; Hydrological Sciences Journal, http://dx.doi.org/10.1080/02626667.2014.947291).

    The trends for Yogo and Hunza from Mukhopadyay et al. (2014) will be discussed in the discussion section. The reference list will be corrected.

30. Page 622 (Lines 26 – 26) – Your calculation of Shigar flows is in error due to the reason explained above.

Since this comment is repeated, kindly see response to the specific comment # 25.

31. In general from Page 605 – 629 – Shorten the discussion. Discuss to the point otherwise it is hard to remember the key points (trends) in the maze of lengthy and verbose discussions. Your main contribution has been establishing field significance of the trends whereby you can draw some generalization for a region from point observations. So focus on that aspect and then your paper will receive the derived attention of a reader. Currently, the way materials have been presented and discussed, no one will have the time to go through all these details and then get lost to figure out the key points than be taken from this study.

The discussion will be shortened, and will focus on the field significance results. Kindly see response to major comment # 1.

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Table 1.Hydroclimatic trends (1995-2012)

Variable	Stations	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	DJF	MAM	JJA	SON	Ann.
Tavg	Khunrab	0.13	0.09	0.13	0.05	0.19	0.00	-0.06	0.06	-0.13	0.05	***0.17***	0.10	***0.15***	0.09	-0.03	0.06	0.06
	Deosai	0.06	0.01	**0.15**	0.00	0.07	0.01	-0.07	0.03	-0.05	0.02	0.08	0.01	***0.10***	0.06	0.03	0.04	**0.07**
	Shendure	-0.05	-0.05	0.05	0.02	0.02	-0.05	-0.10	-0.05	**-0.15**	-0.04	0.06	-0.03	0.01	-0.04	-0.05	-0.02	0.01
	Yasin	0.02	0.01	**0.13**	0.01	0.06	0.04	-0.19	-0.07	***-0.27***	0.11	0.01	-0.08	0.04	0.13	-0.05	0.02	0.06
	Rama	-0.12	0.02	0.05	-0.06	0.07	0.01	-0.03	-0.03	**-0.19**	-0.09	0.05	0.02	0.02	0.00	0.00	-0.01	-0.04
	Hushe	-0.03	0.05	0.06	0.02	0.14	-0.05	-0.07	0.02	**-0.13**	-0.07	0.03	0.04	0.01	0.06	-0.01	0.00	-0.01
	Ushkore	-0.07	0.00	0.08	0.05	0.21	0.00	-0.03	-0.03	***-0.17***	-0.09	0.06	0.01	0.04	0.09	-0.01	-0.02	0.01
	Ziarat	0.04	***0.11***	0.10	0.00	0.09	0.06	-0.09	-0.03	***-0.15***	-0.03	0.09	0.03	**0.08**	**0.07**	-0.02	0.00	0.05
	Naltar	-0.03	0.01	0.08	-0.05	-0.11	-0.07	**-0.12**	-0.06	***-0.17***	0.00	-0.03	0.01	-0.13	0.07	-0.04	-0.04	0.01
	Rattu	-0.11	-0.01	-0.05	-0.04	0.09	0.10	-0.04	0.00	-0.18	-0.07	0.04	-0.10	-0.06	0.03	0.00	-0.05	-0.05
	Shigar	0.05	-0.02	0.00	-0.06	-0.30	-0.13	-0.13	0.04	0.04	-0.14	0.07	0.03	0.01	-0.04	-0.07	-0.01	0.00
	Skardu	0.02	0.11	0.07	0.01	0.02	-0.10	***-0.15***	0.04	***-0.17***	***-0.11***	**-0.06**	-0.07	-0.11	0.06	-0.12	***-0.12***	**-0.07**
	Astore	0.10	0.03	**0.12**	0.01	0.13	0.03	-0.05	0.00	***-0.14***	-0.09	0.03	-0.01	0.05	**0.13**	-0.02	-0.03	0.01
	Gupis	-0.08	-0.06	***0.22***	0.09	0.13	0.00	-0.05	-0.05	-0.08	0.06	0.04	-0.07	0.02	**0.14**	0.02	-0.01	0.03
	Dainyor	-0.06	-0.02	***0.22***	-0.01	0.18	-0.08	***-0.15***	0.02	-0.11	-0.04	0.04	-0.09	-0.05	0.11	-0.04	-0.04	0.00
	Gilgit	0.02	0.01	0.11	0.03	0.06	0.04	-0.06	0.05	-0.09	0.00	***0.08***	0.05	0.03	0.08	-0.02	0.00	0.03
	Bunji	0.06	-0.02	0.06	0.02	0.05	0.02	0.00	0.09	-0.07	0.03	0.06	-0.06	0.03	0.08	0.06	0.00	0.01
	Chilas	-0.02	-0.14	0.06	-0.02	0.16	-0.03	**-0.12**	-0.07	**-0.19**	-0.07	0.01	-0.06	***-0.09***	0.03	**-0.06**	**-0.08**	-0.07
P	Khunrab	***3.64***	2.59	-2.21	-1.55	-1.47	0.10	0.35	0.80	1.82	-1.04	0.93	2.34	***8.86***	-9.09	-1.74	1.65	6.14
	Deosai	0.07	1.28	**-1.42**	-0.66	-1.27	-0.89	-0.40	-1.00	-0.77	-0.42	-0.81	-0.32	1.40	-4.50	0.00	-1.99	-7.87
	Shendure	***1.54***	***2.75***	1.35	2.13	0.60	***2.12***	***1.83***	***1.38***	***1.45***	1.24	1.40	***1.20***	***5.71***	***4.50***	***4.82***	**3.58**	***29.53***
	Yasin	1.33	***1.86***	0.59	0.25	1.22	-0.50	***1.45***	0.02	0.92	-0.21	0.06	**2.74**	***6.09***	0.60	1.32	0.26	11.70
	Rama	0.77	0.00	-6.50	***-8.55***	***-4.52***	***-2.16***	***-2.35***	***-1.89***	***-1.44***	***-2.05***	-3.74	-2.03	7.00	***-25.44***	***-8.41***	***-14.60***	***-43.92***
	Hushe	0.65	0.24	-1.23	-0.30	-1.97	-1.21	-1.71	-0.60	0.73	-0.64	0.11	0.72	***3.47***	-4.51	-4.28	0.70	-5.54
	Ushkore	0.56	-0.59	***-2.33***	-1.02	-1.97	-0.93	0.00	-0.09	1.01	***-0.61***	-0.48	0.09	-0.13	-4.57	-1.54	-0.42	-3.83
	Ziarat	-0.91	-0.56	***-4.18***	**-5.28**	-1.83	0.25	-0.67	-0.18	**1.20**	-0.58	-0.43	-0.61	-3.59	**-9.10**	-1.71	-0.21	**-16.32**
	Naltar	3.75	8.41	-4.49	-0.36	-2.75	**-2.17**	0.43	-2.33	1.32	-0.36	-0.70	1.35	**19.43**	-8.39	-0.99	2.42	-0.28
	Rattu	***1.36***	**2.13**	0.08	0.36	0.26	0.53	***0.91***	0.75	***0.95***	***0.84***	***0.69***	***1.53***	***4.43***	1.23	1.81	***2.36***	10.64
	Shigar	-0.24	-0.89	-1.07	-2.62	-2.05	-0.33	1.75	0.80	***2.40***	1.13	0.18	1.49	-1.67	**-8.36**	0.78	**3.08**	-7.04
	Skardu	-0.64	1.62	0.60	0.19	-0.74	-0.47	-0.07	-0.44	0.46	0.00	0.00	0.20	0.41	0.89	-1.26	0.49	1.29
	Astore	0.00	0.41	0.12	**-1.41**	-0.48	-0.16	-0.08	-0.29	0.57	0.00	0.00	0.29	1.50	-1.36	-1.63	0.34	-0.16
	Gupis	0.65	**0.97**	**0.81**	0.38	-0.06	**-1.33**	**-1.07**	-0.49	0.06	0.35	0.26	***0.89***	2.81	0.29	***-3.49***	0.43	4.46
	Dainyor	-0.21	0.42	***0.51***	**0.55**	***0.67***	***1.24***	0.91	-0.71	-0.39	0.00	0.00	0.00	***1.68***	**1.81**	3.09	-0.34	6.69
	Gilgit	0.98	0.45	**-1.94**	-1.34	-1.57	-0.73	0.29	***-3.99***	0.32	0.00	0.00	0.30	0.00	***-9.39***	***-9.60***	-0.92	**-20.31**
	Bunji	0.01	-0.10	***-1.06***	***-2.34***	0.17	0.20	-0.34	-0.22	0.56	-0.01	0.00	0.11	-0.47	-2.68	-0.51	0.06	0.09
	Chilas	0.00	0.13	-0.14	-1.56	0.16	0.29	-0.51	0.13	***1.37***	-0.10	0.00	0.07	0.22	-0.81	-0.80	1.86	0.53
Q	UIB-East	***-0.80***	0.00	0.04	0.11	-4.19	2.00	-1.65	6.70	-4.74	-5.45	***-2.46***	**-1.37**	***-0.75***	-2.64	-2.62	-0.86	-1.73
	Eastern-Karakoram	0.06	0.08	-0.10	0.00	1.96	0.96	**-22.97**	0.92	-8.84	-1.06	0.50	-0.09	0.29	0.67	0.30	-4.41	-0.95
	Central-Karakoram	0.96	1.28	1.56	-0.84	3.74	-8.94	***-37.93***	-9.08	-5.98	0.71	2.50	***2.76***	1.13	1.13	-21.61	1.10	-1.56
	Kachura	0.33	1.39	**1.06**	-0.33	-2.08	-22.50	**-50.04**	-16.74	-4.25	-2.18	0.59	2.64	0.46	-0.81	-18.90	-2.63	-4.97
	UIB-Central	**2.19**	1.81	2.02	-0.84	6.89	-18.08	***-43.79***	-20.20	-4.88	1.05	***4.38***	***2.34***	**2.00**	1.79	-18.34	2.01	-2.47
	Western-Karakoram	1.20	1.00	1.50	***2.00***	0.59	12.09	-4.53	-4.09	**6.40**	***3.50***	***3.82***	***2.03***	***1.88***	1.00	-1.64	***5.43***	2.50
	Karakoram	1.88	2.00	1.33	1.00	-5.82	-7.80	**-64.97**	-37.17	-9.48	0.60	***8.97***	***5.97***	***1.65***	0.11	-24.43	5.64	-3.90
	Hindukush	0.87	0.26	0.15	1.27	2.05	3.49	-6.61	**14.02**	**7.03**	2.17	1.82	1.06	0.75	1.00	3.94	4.44	4.00
	UIB-WU	1.24	**1.02**	1.39	**2.38**	***16.85***	12.38	-25.48	-15.50	-1.28	0.69	0.98	0.52	0.55	***7.76***	-3.68	0.45	-1.25
	Astore	0.05	0.00	0.22	0.50	7.65	4.26	-3.01	5.00	-1.00	-1.11	-0.67	0.00	0.00	2.20	1.97	-0.89	2.16
	Partab_Bridge	1.00	-0.13	3.60	8.80	63.22	-34.86	-39.86	-67.33	29.65	0.69	8.89	15.12	8.40	36.29	-67.00	9.81	-12.40
	UIB-WL	1.88	0.41	6.39	-0.52	41.58	**59.50**	28.19	***81.58***	**30.99**	16.18	5.17	2.33	1.92	19.90	***65.53***	***16.02***	***25.44***
	UIB-WL-Partab	-3.00	0.80	-4.38	-0.82	***87.89***	51.53	9.00	17.67	2.71	-12.24	1.40	-6.00	-3.74	28.32	47.93	-3.00	18.94
	UIB_West	2.45	1.37	5.43	2.42	**61.35**	54.89	0.21	42.93	***28.24***	**13.68**	***5.87***	1.38	2.00	**23.43**	44.18	***17.71***	***22.17***
	Himalaya	0.30	-0.32	4.10	0.91	43.99	62.23	12.43	83.33	22.43	9.97	2.32	0.23	1.17	26.64	**57.88**	7.75	***24.66***
	UIB	1.82	***5.09***	5.37	-2.50	11.35	14.67	-46.60	41.71	35.22	10.17	5.29	0.75	1.91	15.72	-1.40	19.35	4.25