Earth Syst. Dynam. Discuss., 6, C420–C428, 2015
www.earth-syst-dynam-discuss.net/6/C420/2015/

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 #1

Received and published: 14 July 2015

Review of the manuscript titled: Prevailing climatic trends and runoff response from Hindukush – Karakoram – Himalaya, upper Indus Basin By S. Hasson, J. Böhner, and V. Lucarini Submitted to: Earth System Dynamics Discussion Manuscript No. ESD-2015-12

General comments and recommendations The main contribution of this work is a set of trend analysis of monthly average maximum (Tx), average minimum (Tn), average (Tavg) temperatures, average diurnal temperature range (DTR = Tx – Tn), and total precipitation from six low-altitude (valley-based) meteorological stations covering the period 1961 – 2012 and twelve relatively high-altitude stations covering the period 1995 – 2012. In addition, the authors also present trend analyses of discharge data

from ten gauging stations within Upper Indus Basin (UIB) as supplemental to the trend analyses of the climatic data. The only original idea presented in this work is the determination of field significance of the local climatic trends and this is the best part of the paper in spite of the limitation of its applicability in case of a network with sparse observation stations. While such a set of analyses can be considered new contribution to the advancement of knowledge on the hydro-climatology of UIB, the paper suffers from several major drawbacks that must be removed before it can be further considered for publication. For this reason, my recommendation is bound for rejection with an invitation to resubmit the work as a new submission that will undergo another round of fresh reviews. In the new and revised submission the authors are advised to scale down the writing to remain focused on the new materials presented here and remove already known information by referring to the relevant previous published papers and by eliminating the redundancy syndrome pervasively present in the current version of the paper. The major shortcomings of the paper are summarized below followed by specific 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. 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]. 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. 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. 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. 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 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. 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. 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. 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. 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. Specific Comments 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. 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. Page 584 (Line 4). The stochastic component of a time series is called "white noise" NOT "red noise". Do not use wrong terms. 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). 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). 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. 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 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]. 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. 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]. 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). 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. 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. 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). 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. Page 595 (Line 12). "limited skill" – another strange use. Page 595 (Line 25). Another wordy sentence with little weight. Page 596 (Line 20). This period of record (1995 – 2012) is too short to detect any meaningful trend. Page 598 (Line 2). Should be S NOT Z. P 598 (Line 10). Say white noise, not "noise process". Page 599 (Line 6, Eq 8). The yt in this equation is not the same yt in Equation 6. Change symbol. Also, add ïĄět in this equation. 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. Page 600 (Lines 11 – 13). Rewrite this sentence with correct grammar. Page 600 (Line 15). You cannot divide UIB into smaller units based on hydrological regime. Obviously you don't know 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. Page 600 (Line 24). Same problem as noted above. 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. 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). 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 ($\approx$) instead of equal sign in the equations. 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. 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. 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). Page 622 (Lines 26 – 26) – Your calculation of Shigar flows is in error due to the reason explained above. 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 that can be taken from this study.

Please also note the supplement to this comment:
http://www.earth-syst-dynam-discuss.net/6/C420/2015/esdd-6-C420-2015-supplement.pdf

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