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.

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The following review was sent to me by e-mail. AK

Recommendation: may be suitable for publication after major revisions

This paper analyses trends in precipitation, temperature and runoff in the Upper Indus Basin (UIB). There have been a number of previous studies focusing on trends in this context, but the main novelty of this paper is in calculating trends using high elevation automatic weather station (AWS) data. However, given that data for these stations are only available for 1995-2012, the trend analysis is conducted for a relatively short pe-

riod (although this is compared with longer-term trends from lower elevation stations). My main concern is whether trend analysis is meaningful and justified for these short record period data, even if the focus is stated as "prevailing climatic conditions" rather than longer-term trends. This is a critical issue for the paper, as all of the results are dependent on the robustness of the trend analysis.

The methods employed for trend analysis are standard (non-parametric Mann-Kendall test, Sen's slope and pre-whitening), but the practical significance of the results may be limited by the time series length. In addition, the authors divide the UIB into sub-regions for testing the field significance of calculated trends. While this may be a potentially new approach in the UIB context, one of the difficulties with it is the relatively small number of stations (18) with which to estimate statistical field significance in such a complex setting (even with a bootstrapping method). This is particularly so given that some of the sub-regions contain very few stations (minimum 2?). Plotting the stations on Figure 2 or tabulating the number of stations in each sub-region would make this more transparent.

Description of the methods could probably be clearer and more carefully written. For example, it might be useful to explain briefly the bootstrap resampling approach rather than just provide a reference. Not all of the symbols used in the equations seem to be defined in the text (e.g. theta in Equation 2, t in Equation 6 – all should be checked). Equation 12 might also be clearer if split in two.

While a range of plausible explanations for the estimated trends are presented, the discussion and interpretation of results could be a little more carefully presented. Some trends may be consistent with mechanisms and processes that have been put forward in the literature, but the manuscript reads a bit too definitively in parts (with quite a lot of assertion). The level of interpretation does not feel consistently justified by the results. Explaining recent historical changes in terms of climate model projections for the future also seems ambitious. The discussion section could therefore benefit from adjusting its emphasis and tone to be less conclusive. Along the same lines, the conclusions on

trends reached in the paper should be more clearly stated in the conclusions section, with less emphasis on interpretation in terms of processes here.

The overall presentation and structure is clear, but the manuscript still seems long and might benefit from transferring some of the detail to the supplementary material. For example, there are long descriptions of delineation of the UIB catchment boundary and data sources where some of the detail could be moved out of the main text. The introduction and results section could be shorter and more focused. The standard of English in the manuscript should be improved further (it is reasonable overall but not fluent in all parts).

Some improvements to the tables are needed. Latitude and longitude seem to be the wrong way around in Table 2. The latitude and longitude of gauging stations should be quoted to a lower number of decimal places in Table 3. Tables 4 to 7 are very large. It may be better to move the full results to supplementary material and synthesise the key findings in the main text. Also, the signs of the numbers do not always seem to agree with the colour coding as described in the captions (e.g. Table 4 caption says that blue means an increasing temperature trend, but the numbers coloured blue are negative). If gradational colour scales are to be used with the tables, I think more care and consistency is required (e.g. consistency between tables and more explanation of what is being shown).

The station names are difficult to read on Figure 1, and Figure 2 might benefit from showing (unlabelled) station locations to clarify how many stations are being used to determine field significance. Figure 8 requires a key to explain the size and colour of the symbols (and ideally some spatial reference, e.g. UIB sub-regions or rivers).

Overall, I am concerned that trend analysis and field significance tests are inappropriate given the record periods and number of stations available. The analysis and interpretation may be beyond what is justifiable for the dataset.

Minor comments:

line 353: <typo> "DTR - Tx - Tn" should read "DTR = Tx - Tn"

lines 785-791 & 842-852: While the increase in (late) summer precipitation reported by the authors is not disputed, its attribution to monsoonal weatherly systems rather than westerly disturbances, other than aligning with theoretical future circulation changes, seems to be conjecture rather than substantiated. In effect, the additional summer precipitation at high elevation/latitude stations could be a result of greater (than previous historical period) penetration of westerly systems due to weakening/southerly position of the monsoon which structurally is more generally a lower altitude system. Furthermore the teleconnections cited, particularly NAO, have been principally associated with variability of westerly disturbances rather than monsoonal circulation.