

Interactive comment on “Trends in timing and magnitude of flow in the Upper Indus Basin” by M. Sharif et al.

Anonymous Referee #1

Received and published: 5 October 2012

The manuscript “Trends in timing and magnitude of flow in the Upper Indus Basin” by Sharif et al. analyses stream flow data in the study region for the presence of significant changes over an observation period of ~30 years in the second part of the 20th century. The manuscript is in general well written and structured. The methods used are standard statistical methods which are applied in a sound and well documented way. Although the manuscript does not involve a high degree of sophistication in the methods nor presents any innovation or new hydrological theories as pointed out by the editor earlier, I feel that it can be an interesting contribution to literature. This is in particular as it complements a range studies on stream flow changes in the US which appear to be consistently pointing towards reduction in stream flow. In contrast to these studies, the presented manuscript comes to different conclusions, as no consistent or

C4559

no trends at all could be detected. I see the importance of this manuscript thus in raising awareness that a generalization of results from other parts of the world to at least the UIB is not easily possible as it will potentially involve serious misrepresentations of local conditions. Further, the manuscript highlights the importance of differences in the hydrological regime on stream flow trends in the study region, which again aids in getting a better understanding of what is actually driving these trends. On balance, I would be happy to see this manuscript eventually published as it (1) is a potentially good reference for further research in the UIB and similar regions and (2) emphasizes the importance of local/regional regimes in assessing the impact of (climate) change.

A few minor comments:

- (1) p.9933, l.3-6: please add some references here (e.g. Stahl et al., 2006)
- (2) p.9933, l.10 and elsewhere: although the general term “magnitude” might be suitable in places, it would nevertheless be good to be a bit more specific where possible. Which magnitude? Annual volumes? Peak flows?
- (3) p.9934, l.11-12: although it becomes clear later in the manuscript, please be more specific here, i.e. what exactly do you mean by “the percentage of annual runoff”?
- (4) p.9935, l.18: “attribution” sounds a bit awkward here. But then again, I am not a native English speaker. . .
- (5) p.9935, l.24-25: Please clearly state the objectives of the paper at the end of the introduction. What are the research hypotheses you want to test?
- (6) p.9936, l.18: not entirely sure what “mean winter half year temperatures” are. Please try to rephrase are clarify.
- (7) p.9936, l.13 – p.9937, l.5 and p.9938, l.3-8: in addition to these paragraphs of explanation it would be fantastic if you could add a map indicating the location of different predominant regimes. Alternatively you could incorporate this information in Figure 1, replacing the elevation zones. It would considerably help the reader to understand

spatial context of these different regimes.

(8) p.9937, l.8: "annual peak daily flow" sounds awkward. Maybe this could be rephrased.

(9) p.9937, l.25: over which period does this significant increase happen?

(10) p.9938, l.14-19, P.9939, l.4-6 and Table 1: one formal thing that needs to be clarified is WHY are no flow records available after the late 1990s? Did the local authorities stop taking measurements? Did the authorities not grant access to the data? Are there other reasons? Please make this clear as trend analysis in the light of (climate) change is obviously a politically and scientifically highly sensitive and much discussed topic and some readers might become suspicious if you do not clearly state WHY there were no more flow records used in this study.

(11) p.9943, l.12-13 and Table 3: Although mentioned below the table, make it clear in the text, as well, that your chosen significance level $\alpha=0.1$. I encourage you to do so as it is a relatively elevated and somewhat unusual level. By clearly stating it in the text you will avoid misinterpretations and potential accusations of misleading the reader.

(12) p.9944, l.13-p.9945, l.3 and Figure 4: Please also show in Figure 4 which regions belong to which regime. This will help the reader to follow and better understand the spatial context.

(13) p.9946, l.18: I do not fully agree with this statement. In the following sentence you acknowledge that it cannot be true, which is in fact obvious: while the proportions merely describe the seasonality, the actual volumes can be shifted up or down. Thus you can get higher seasonality with both increased and reduced total flow volumes and vice versa. Please somehow rephrase this paragraph and point out the fundamental difference between the two variables.

(14) p.9947, l.26-28 and elsewhere: The comparison with the North American studies

C4561

is really one of the strong points of the manuscript. However, in order to increase the relevance of this comparison I think it is absolutely necessary to at least state over which time period the North American studies have been carried out. If they have been carried out over significantly different time periods, please adjust your interpretation accordingly!

(15) Table 2: please give units for each variable. Please specify if variables 7-11 are average flows over the time period or the total volume of flow

(16) Tables 6 and 7: can maybe combined into one table

(17) Figure 3: are these mean monthly temperatures? Please clarify.

(18) Figures: Please include a figure with the runoff time series for at least a characteristic choice of stations. The reader only has the values of the correlation analysis and has no means of checking their plausibility if you do not provide at least some time series.

REFERENCES

Stahl, K., Moore, R.D., Floyer, J.A., Asplin, M.G., McKendry, I.G., Comparison of approaches for spatial interpolation of daily air temperature in a large region with complex topography and highly variable station density, *Agricultural and Forest Meteorology* 139:224-236, 2006.

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 9, 9931, 2012.

C4562