

Interactive comment on “Trends for snow cover and river flows in the Pamirs (Central Asia)” by P. Chevallier et al.

Anonymous Referee #2

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This paper attempts to analyze the hydrological situation in six basins in the Pamir Alay mountain range in Central Asia. The analysis is based on a long-term dataset between 1940 and 2000 and a short-term one covering the 2-year period between May-2000 and May-2002. The hydroclimatological parameters that were considered in this study include surface temperature, precipitation, snow cover, glacier extent and river discharge. The paper confirms the IPCC's fourth assessment report in that this region is a sensitive one and that temperatures are increasing in this mountainous area. It reports some differences in the snow dynamics between the basins, and claims that snow cover dynamics and increasing temperatures play the major part in the changes in the river flow regimes.

The subject of the paper is important and suitable for the publication in the HESSD

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journal, however, the way it is presented is quite confusing. There are many inconsistencies throughout the paper regarding the subject, objectives, data length and quality and the analyses. Take, for instance, the subject/objective of the paper. The title of the paper is "Trends for snow cover and river flows in the Pamirs (Central Asia)"; the subject of the paper is expressed as "to analyze the hydrological situation in six benchmark basins" in the abstract; the aim of paper is given as "to analyze the trends of the snow cover extent and of the river flow regimes" in the introduction part; the subject is stated as "to analyze the current hydrological situation and to reply to the objective of the research, six benchmark basins were chosen in the Pamir Alay range" in section 3.2 entitled "Hydroclimatology"; and finally, the topic of the paper is referred as to study "the state of the cryosphere in the Pamir Alay mountain range" in the conclusion section. These highly inconsistent statements are reflected to the outline of the paper as well. The paper starts with an analysis of a relatively long period, and suddenly, switches to an analysis of a comparatively very short period, and then, back again to the long period of interest. The results associated with the latter, which are based on the figures 10 and 11, are only given in Discussion section. The lengths of the data are different, and they have so many gaps, but they are treated as are in the analyses. Some analyses include all basins, some others include a few of them. All these inconsistencies make it hard to justify the results and conclusions of this study. I think that the results and conclusions are not robust enough as argued in the Conclusion section; I think they are not adequately justified by the data.

I suggest the paper should address the following related questions clearly: What is the "punch line" of this study? What are you really trying to show us by this study? And, what is new in this study?

I would also suggest you to describe the methodology thoroughly. I understand that the data are scarce for this region, but please do not go beyond (in your conclusions) what the available data could really justify.

Regarding the inclusion of the analyses of longterm vs shortterm data, I think you

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should give one of them more weight in the paper and treat the other as minor contribution. The similar weights as in the present version of the paper are counterproductive, in my opinion.

Some other points:

1. The title does not properly reflect the content of the paper.
2. Page 31, Line 5: "In this region, the cryosphere, glaciers, and snow cover significantly..." Isn't "cryosphere" a general word including the glaciers and snow cover?
3. Page 31, Line 18: "Finally the expected changes in the flow river regime..." What do you mean by "the expected" in this sentence? Also, "flow river regime" should be "river flow regime".
4. Page 31, Line 21: "4rd" should be "4th".
5. Page 33, Lines 20-: Most of the precipitation stations are outside of the basins of interest. Few of them are located in these basins. Given the fact that the region is mountainous, do they give us adequate and reliable information about the spatial distribution of precipitation in these basins (not the outside)?
6. Page 35, Line 13: What does METI stand for?
7. Page 36, Line 6: The example should be given before the preceding sentence?
8. Page 36, Line 17: Since you have daily discharge data, why don't you study whether there are temporal changes in the timings of the peak flows, which may be an indication of climate change in this region?
9. Page 37, Line 20: It is not clear how you took average of the data since their lengths are different. It is also not clear how you calculated the trend line and the gradients.
10. Page 38, Line 11: "...Kudara basin (2000)..." Is "2000" indicating the year?
11. Page 38, Lines 13-15: If that is not entirely true, then what might be the problem?

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Could it be resolution? And also, Figure 8 indicates that Obighingou behaves differently from Kudara Basin!

12. Page 38, Line 26: "(same as the left side of..." It should be right side.
13. Page 39, Lines 3-21: What is the source of precipitation amounts? Figure 2 does not give that much detail.
14. Page 40, Lines 2-4: Why did you use precipitable water data from NCEP/NCAR? It should have precipitation rate data. Have you checked other datasets including CRU?
15. Page 41, Line 23: "Except on the Kyzylsu basin,..." Does this mean that you didn't take Kyzylsu into account while calculating the regression line? What happens if you take out the MUK data? Also, no information for the line is given in the caption.
16. Page 42, Lines 5-12: There are too many missing data periods in these series. How did you calculate the trend lines? They seem to cover the missing data periods.
17. Page 42, Lines 24-25: How do we know that the precipitation regime in volume remains almost stable?
18. Page 42, Line 25: How can you make such an assumption?
19. Page 43, Line 17: What do you mean by "naturalist" approach? And, why do you chose this approach?

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