

In order to reply to the first anonymous reviewer, we scrupulously annotated the supplement that he uploaded. The reviewer contribution is in black color and the author replies in red color. However, we want sincerely thank the reviewer for his very constructive comments, that will help us to improve the scientific content of the final version of our paper.

The main changes that we will make concern:

- The change of the title for “Snow cover and river flow regime in the Pamirs (Central Asia)”
 - The introduction of a statistical analysis of the trends using the Mann-Kendall criteria.
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Text

This paper addresses the trends of snow cover and runoff in the Pamirs. Unfortunately there are quite a few parts of the article to change to achieve this goal. In my opinion there are too many points of uncertainty of the conclusions drawn and not really new or substantial results. The existing results are not sufficient to support the interpretations and conclusions.

The title “trends for snow cover and river flows in the Pamirs” suggests that there are some in-depth trend analyses. But on the one side the subject of trend analyses is only slightly touched, on the other hand the methods used for this purpose are statistically not clean.

Author reply: That is right! Even if we have underlined in the conclusion that the approach was essentially “naturalist”, it’s true that the use of statistical expressions in the title and in few key parts in the text could misinform the reader on the real content of the paper. The words “trend for” will be removed from the title replaced by “Snow cover and river flow regime in the Pamirs (Central Asia)”. Moreover, we will make a scrupulous review of the statistical expressions, eliminating them, especially the word “trend”, if they could be misinterpreted.

In addition I think it is difficult to combine the results of different periods of 30+ years of T and Q in the past with a period of 2 years of Snow Cover, Glacier Cover, T and Q that is long after the other periods.

Author reply: We agree. But that was the conditions of data availability for the study. This restriction will be better explained in a short new paragraph introducing the section 3 on the available data.

On the positive side, the manuscript is well structured and provides an understandable and short summary of the full analysis. Furthermore the authors give proper credit to related work. And I understand it is difficult to do research in areas like the Pamirs where data availability poses a serious problem and only few studies have ever been carried out.

Author reply: Thanks for this comment, recognizing the difficulty of a rigorous scientific analysis in such context.

But all together there are substantial points that have to be changed. For me a paper with the title “trends for snow cover and river flows” should definitely go deeper into statistical analyses. Putting a trend line into some time series is not meeting scientific standard for trend research. And from two years of snow cover data I think it is not possible to deduce trends. Moreover, the explanation of some of the methods used (e.g. trend lines) is missing.

Author reply: As written above, we will take a strong attention to be more careful in the revised text with every sentence concerning statistics, eliminating those, which could appear as not rigorous, and detailing better the used methods.

Finally it seems that the Authors emphasize their finding that snow cover dynamics and temperature increase play the main role on Q-change in the research area, but

1. Firm evidence is not obvious.
2. In my opinion, if P is stable, the temperature conditions are the driving force for snow cover extend anyway. Following the authors findings above, this means that T plays a key role on Q-change in mountainous areas ☒ this is nothing new.

Author reply: We agree; we will be more careful in the final version with the statements concerning the main drivers of the runoff generation.

Going into detail, there are quite a couple of points that should be improved or where I do not agree:
Title: Trends OF snow cover and not “for”

Author reply: The title is changed in “Snow cover and river flow regime in the Pamirs (Central Asia)”.

Abstract line 18:

- “finally” misleading

Author reply: The word “finally” will be removed.

p.32, line 22:

- sentence structure
- “analyze trends of snow cover extent” with data of two years time? Maybe something more modest would be appropriate
- Same for Q: the “trend of the river flow regime “ is done just for two years only (Fig. 9). I think this is not enough for doing trend analyses. In Fig. 11, where you do research of longer periods of Q-data, there is no analysis of the trend of the flow regime as such conducted. But I think this analysis could reveal some interesting developments.

Author reply: The sentence will be modified as: “In such context, the present paper aims to analyze the dynamics of the snow cover extent and its links with the river flow regimes.” Concerning the Fig. 11, see the reply below.

p.33, line 24:

- emphasize measurement errors of solid precipitation (probably real precipitation values are a lot higher)☒ especially if the values are given for one station it would be interesting to know the height of the station (☒ Fig. 2,3) as at eastern stations (like Murghab at around 4000m a.s.l.) the percentage of solid precipitation is a lot higher than at the stations e.g. along the Pyandj River in the west

Author reply: You are totally right. We do not insist sufficiently on the issue of the solid precipitation measurement, especially at high altitude. We will add one or two sentences with references (e.g. Sevruck, Tahir et al.) explaining this point and highlighting that the amount of precipitation is very likely significantly underestimated.

Sevruck, B. 1989: Reliability of precipitation measurements. In Sevruck, B., editor, *WMO/IAHS/ETH Workshop on precipitation measurement - Swiss Federal Institute of Technology, St. Moritz, 3-7 December 1989*, 13-19.

Tahir, A. A., Chevallier, P., Arnaud, Y. and Ahmad, B. 2011. Snow cover dynamics and hydrological regime of the Hunza River basin, Karakoram Range, Northern Pakistan. *Hydrol. Earth Syst. Sci.* 15, 2275-2290.

p. 37, line 2 et sqq.: Why only two years of NDSI satellite data? Are there no other SCA (snow covered area) satellite products of longer period? What about the period from 2002 up to now?

Author reply: The paper is the result of a study executed in the framework of an EU FP6 project constrained in time and funds. It was not possible within it to treat more satellite data, which is highly time-consuming. We will add a sentence for clarification. We agree that it is a real weakness. However, in the final paper we will highlight more clearly the descriptive side of the approach, insisting on the statistical point of view only in the case of long duration data series

p.38, line 15: snow accumulation through avalanches not negligible in watersheds of that size?

Author reply: It is impossible to assess the concerned volumes and to verify if they are negligible or not. The main consequence of the avalanche is the accumulation of snow in the valley bottom at the lowest altitude, with a high density and a high thickness, with delay considerably the melting process, resulting in the observation that we made in late July 2007. We will explain better this point.

p.38, line 16 et sqq.: representative years? ☒ Comparison to in-situ data

Author reply: We will complete the sentence: "(...) for the six basins and for the period 2000-2002, the percentage of snow cover (...)". In situ data are not available. A sentence will be added in the section 3.3, justifying the use of satellite imagery.

p. 41, line 10: shift to the left (Comment 5th Jan. 2012: ERRATUM: right)? Or downward?

Author reply: Of course the shift is to the right, but you are right: the word "upwards" is more appropriate.

p. 41, line 20:

- "our results underline" (without -s); line 21: the snow cover (not "this");

- "it can be observed that the quality of the relationship between flow discharges and temperature depends not only on the percentage of the glacierized area, but also on the median altitude of the basin" ☒ I think this is not surprising: The higher the area (in one region), the more glacierised it is usually.

- Why and how does Fig. 10 underline the major role of the snow cover? Not obvious in the previous sentences and from the graph. ☒ from Fig. 10, I would deduce that there is a stronger relationship between R^2 (of T and Q) and glacier cover than between R^2 and median altitude. The higher the temperature, the more glacial melt water will be produced all over the summer month ☒ the higher will be R^2 .

Author reply: Many thanks for the comment.

(1) We will correct the grammatical errors.

(2) However, we partly disagree with your statement "The higher the area (in one region), the more glacierised it is usually". It strongly depends of the regional climate conditions. In our case the cases of the Obhingou and Kudara basins are significantly different.

(3) We agree that the explanations concerning the Figure 10 are too short and must be more developed and we will take into consideration your very consistent deduction.

p. 41, line 29: "In the Andes, the relation is much stronger, but surprisingly there is no snow cover"...?

☒ Please check the meaning of that sentence. I think you want to say something different.

Author reply: We agree; this sentence is ambiguous. We will change it for: "A similar behavior is observed in the Andes (references), with a still stronger correlation, which depends essentially from the glacier melting, regarding the limited contribution of the snow cover in the not glacierized areas".

p. 42, line 26: Why do you assume the impact of CC on Pamir snow cover as "limited"? ☒ You said earlier that temperature increases in this region "appear in the upper part of the global warming trend (p.38). In my opinion, temperature rise in mountainous regions is a lot stronger than in flatland regions BECAUSE of the strong decrease of snow cover due to the positive feedback processes of albedo and surface temperature.

Author reply: It is correct. This sentence is not convenient at this place of the text. We will remove it entirely.

p. 43, line 7,8: "it can be advanced that the peak discharge value is possibly backwards." ☒ for me this sentence is not possible to understand

Author reply: The sentence will be modified: "(...) the glacierized area is already quite small and the glacier mass unbalanced, which could accelerate the decreasing of the glacierized areas".

p.43,line 24: what are the consequences?

Author reply: The sentence will be modified: "It confirms the high vulnerability of this region to the global warming as identified by the IPCC's 4AR."

p.44,line 1: Check sentence structure and wording

Author reply: This sentence will be removed.

Figures:

Fig. 1: Borders of southern and western Tajikistan are missing and 1/3 of the upper part of the map is not necessary (Kazakhstan)

Author reply: The initial idea was to show the complete extension of the Aral Sea basin. But we will modify the figure, limiting it at approx. 47°N and showing the borders hidden by the rivers

Fig. 2 and 3 and p.36, line 15: data: means for 30 years ☒ which 30 years? Indication of the year 2005 in the caption is misleading

Author reply: The range 1960-1990 will be added and also the complete reference to the NSIDC-NOAA source.

Fig. 6: - Method of trend estimation? regression? ☒ Only linear and global (!) relations ☒ trend magnitude depends strongly on the interval considered! (Fig. 11 as well)
- Significance of trend?
- Suggestion: Modified Mann-Kendall trend test for autocorrelated data and Sens Slope for trend magnitude (e.g. Khaled et al. 1998)

Author reply: Yes, the trend line is a linear and global regression aiming to underline the display, not to represent an accurate trend. We agree that it should be more statistically based. The Mann-Kendall test will be applied to all data series separately and a table with the results for each series will be added and discussed. Thanks for the suggestion of reference. We will quote also: Kendall, M.G. and Gibbons, J.D., 1990. Rank Correlation Methods, 5th ed. Edward Arnold, London.

Fig. 7: One should be critical with the informative value of two years of data (esp. snow cover ☒ highly variable). Maybe it is necessary to emphasize that and keep that in mind while doing the interpretations

Author reply: We agree and one or two sentences of comments will be added within the text.

Fig. 8: potential for better formatting of the diagram: date and description of y-axis in the graphics

Author reply: This figure is quite difficult to present in a synthetic and easy-to-read manner. We have tried several layouts before this one. But we will try to improve it still more.

Fig. 9:

- The direct comparison of grid-data (P,T at 600 hPa) and local station data poses some serious uncertainties which should be pointed out
- P explanation missing

Author reply: We agree. A sentence will be added in the text focusing the necessary precaution when comparing gridded and observed field data

Fig. 10:

- "grey and black"? I can see colors.
- Best fit line is a bit shaky

Author reply: The words "grey and black" remained from a previous version in grey levels. They should be replaced by "light colored and dark colored". The best fit lines will be removed and a comment will be added in the text.

Fig. 11:

- same problem like in fig.6; completely different intervals \boxtimes comparability and informative value of trend lines is equal to zero
- descriptions of the axes are way too small

Author reply: The figure will be completed by the same approach with the Mann-Kendall criteria as described for figure 6. The readability of the axis will be improved.

References:

Khaled H. Hamed, A. Ramachandra Rao, A modified Mann-Kendall trend test for autocorrelated data, Journal of Hydrology, Volume 204, Issues 1-4, 30 January 1998, Pages 182-196