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. 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.

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.

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.

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.

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"

Abstract line 18:

- "finally" misleading

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.

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

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?

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

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

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

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.

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.

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.

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

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

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

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)

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

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)

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

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

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

Fig. 10:

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

Fig. 11:

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

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