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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Early 21st century climatology of snow cover for the western river basins of the Indus River System" *by* S. Hasson et al.

Anonymous Referee #1

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This paper presents an analysis of 11 years of MODIS snow cover data for several basins of the Indus River System. The authors describe a new methodology for filtering out cloud coverage and interpret snow cover products with respect to temporal trends, climatic regimes in the sub-basins, glacier coverage and regional drivers of snow accumulation changes. Over the last decade, a decrease in the elevation of the snowlines in the Upper Indus Basin is found having an impact on the water resources. Correlations of trends in snow cover with the North Atlantic Oscillation (NAO) are analyzed and the authors speculate about a possibility to forecast runoff at seasonal time scales. Overall the paper is well written, the methods are clearly described and the re-

sults are interesting. Before the paper can be considered for acceptance, the authors however need to address a few substantive issues related to the interpretation of the results. These points have – in my opinion – considerable impact on the conclusions:

(1) Link snow cover (2D) – snow amount (3D): MODIS images allow extracting detailed information on snow-covered area. This information is however purely two-dimensional, i.e. a MODIS pixel provides exactly the same information if the snow thickness is 0.1 m or 2 m. In terms of water resources this is a huge difference! As much of the motivation of this paper is related to water resource management and hydrological forecasts, this is a crucial issue. Throughout the paper it is implicitly assumed that a high percentage of snow coverage corresponds to a high snow volume. This link is however not given a priori: For example, enhanced precipitation combined with higher air temperature might lead to an overall smaller snow coverage (due to melt at lower elevations), but larger snow volumes due to thicker snow at high elevations. Especially in winter (high snow coverage), the information content of MODIS images in terms of the snow amount is very limited. The authors should try to establish a link between the percentage of snow-covered areas and the snow amount if they want to relate their study to water resources management and discharge. The respective interpretations in the paper (see also detailed comments below) should be re-considered correspondingly.

(2) Snow cover – runoff: There is a lack in clarity regarding the authors' interpretations regarding the link of snow cover trends and stream-flow runoff. A trend in snowlines towards lower elevations is related to decreasing runoff volumes. The reasons for this should be discussed at a process-based level. And do decreasing SLAs really mean a decrease in water resources as implied by the authors? In fact, lower snowlines indicate positive glacier mass balances and thus enhanced water storage at high elevation which might represent an important future supply to water resources in a warming climate.

(3) Validation of snow cover products: I did not completely understand how the snow

cover products were validated. What is the independent reference for judging the performance and the validity of MODIS snow cover and the applied cloud filtering methodology. A better explanation would be helpful.

(4) Relation to glacier mass balance: End-of-summer snowlines are related to glacier outlines. If the authors intend to link observed snowlines with glaciers (which is important and interesting), a more detailed analysis and discussion is required. Instead of the median glacier elevation, the percentage of the glaciers covered with snow at the end of the melting season should be evaluated, corresponding to the so-called Accumulation Area Ration (AAR). For glaciers in High Mountain Asia often having particular hypsometries and considerable debris-coverage, the median elevation might be a bad indicator for the elevation of the equilibrium line.

(5) Correlations to NAO: The correlation of snow cover to NAO is speculative and needs to be improved. Why did the authors choose to use the NAO (and only the NAO!) for their analysis? The causal link between air pressure differences in the North Atlantic and the Indus Basin should be described in detail. Otherwise anything (!) could be correlated to the snow cover products. It is also unclear how the authors chose the different periods with varying lengths to perform the correlation analysis. Have there been any systematic statistical evaluations which months should be correlated with which ones, or is this based on physical relations in the climate system? More justification on the choices has to be provided and the lack in correlation for some basins needs to be better discussed. My major points (1) and (2) are most critical in this respect as the NAO is intended to be used to forecast runoff (i.e. snow melt QUANTITY), although winter snow percentage contains almost no information on snow depth.

(6) Figures: I had troubles to judge the Figures of the paper. In my printed version ALL axis labels consisted of symbols I was not able to read (probably Hindi. . .).

Detailed comments:

- page 13154, line 22: How was the size of the filer determined? Was there some kind

of optimization performed?

- page 13156, line 22: “results have been improved significantly”. This needs to be reformulated: first, “significantly” means that a statistical test has been applied indicating the enhanced performance. But against what has the quality of the snow cover product been evaluated? I agree that the filtered snow-cover product looks better. But without validation against independent data statements about the quality need to be put into perspective.

- page 13158, line 8: The presence of glaciers does not have any impact on snow cover variability. It is rather the particular topographical characteristics of ice-covered (high-elevation) basins that reduce the variability.

- page 13159, line 13: “systematic underestimation”. Why underestimation? . . ..

- page 13160, line 1: positive / negative trends – provide a clarification for the reader what positive / negative trends imply (less / more snow-covered area; rising / decreasing snow lines altitudes). A figure showing these main results would be helpful.

- page 13161, line 1-13: More details are needed here. Average correlation coefficients should be also stated in the text. Reasons for choosing the considered periods should be provided. Furthermore, according to Fig. 8 there is very small year-to-year variability in snow line elevation for most basins. It is difficult to understand.

- page 13162, line 14: See substantive comment above. A snowline below the median glacier elevation does not directly indicate positive glacier mass balance. This might be a good occasion for citing Gardelle et al (2012, 2013).

- page 13162, line 17: “mass release of accumulated snow” – what does this mean?

- page 13165, line 1-13: here in particular (and elsewhere): At several instances the authors use the terms “underestimation” which is unclear and wrong here. Why underestimation? This does imply that a bias is present. But relative to what?

- page 13167, line 5-12: This paragraph is poorly connected to the rest of the paper and its statement are not very clear. It could be omitted.

- page 13167, line 15: “increased water storage capacity is needed”. I think this statement is not correct. If snow line altitudes are decreasing this actually indicates that water storage in the Karakoram is increasing!

- Table 2: referenced much too early - before Table 1 and before introducing the variables and results that are shown. What is the unit of the trend slope?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13145, 2013.