

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 #2

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The authors present a study of snow cover observed by the MODIS over the Indus River System. They report on an improved method to derive snow cover from the MODIS product available at 500 m spatial resolution. The resulting snow cover products for the period from 2001-2012 are subsequently investigated for its relationship with large-scale atmospheric circulation. The authors spent a considerable amount of effort on describing the connections of snow cover with climate and the hydrological cycle. On the other hand, a rather small portion of the manuscript is devoted to a detailed understanding of the snow processes at the ground and observed by satellite sensors such as MODIS. My feeling is that the paper is telling two stories: one about

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improving a remote sensing products and other about the role of snow cover in climate and the hydrological cycle. Both are very interesting topics but the mixing of the two does, in my opinion, not contribute to in-depth analysis and quality of the manuscript. I recommend, therefore, a thorough revision of the manuscript before publication.

Major comments:

1) It is very optimistic to speak about climatology or trend at all in this particular study. The dataset investigate is only 11 years long and, as the authors, write in text this period subject to several extreme events; the episodes 1999-2003 and 2006-2009 were very dry. It is impossible to derive a trend from such a highly variable and short dataset.

2) From the manuscript it appears as if snow cover is the only variable of a snowpack whereas snow albedo, density and depth are very important states that in essence define the snow processes and the resultant snow cover. In fact, there is a snow albedo product and perhaps also a few other snow-related products available from the MODIS. This background information is missing in the manuscript and that is important for the motivation of the study. Many of the modelling studies the authors refer to adopt simplified model structures that ignore these processes.

3) The procedure for developing the improved snow cover product is based on reducing the number of cloud cover pixels via spatial and temporal filtering technique. This seems a bit awkward because by spatial/temporal filtering you lose information and snow cover varies spatially and temporally. Temporally because of snow melt that is defined by the available energy (incoming and albedo) and spatially also because of the wind-redistribution of snow. As such I am not convinced that the developed snow cover product is an improvement of the existing.

4) The remote sensing problem of snow cover is not very well introduced. What are the state-of-the-art methods? Is cloud cover really the only issue with estimating snow cover?

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5) Validation: The new snow cover products are validated at all. A validation should be included and the results should be compared against a standard before it can be accepted as a superior product.

6) Spatial scale: Wind blow and patchiness of snow are important issues. This inevitably has an impact for the estimation of snow cover with 500 m spatial resolution satellite observations. The magnitude of its impact also depends on the type of snow that is dealt with; alpine, tundra, and prairie. These issues should be addressed in the manuscript.

7) Teleconnections are interesting, but I wonder if a 500 m snow cover product is needed to prove these relationships. I think it is more effective to test to hypotheses against data from local stations in support with the snow cover product because now it is not clear if a connection is observed or it is anomaly in the RS product.

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