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The Cryosphere Discussions

Interactive comment on "Investigating spatiotemporal patterns of snowline altitude at the end of melting season in High Mountain Asia, using cloud-free MODIS snow cover product, 2001–2016" *by* Zhiguang Tang et al.

Anonymous Referee #1

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The manuscript presents a remote sensing study, aiming to detect large-scale patterns of snowline altitude at the end of melting seasons for High Mountain Asia and the period 2001 to 2016. The proposed method is based on MODIS fractional snow cover data that are processed using a previously published routine to remove cloud cover. This method is extended by an approach to estimate perennial snow by empirically matching MODIS-derived snow-covered days to glacier mass balances and Landsat-based snow cover estimates. Strongly generalized results are then analyzed towards intrinsic trends and correlations with meteorological station data by applying basic statistic

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Discussion paper

tools, i.e. linear regression and correlation coefficients. As such, the overall topic of the manuscript is relevant and fitting TC's scope but the methods and data hardly satisfy basic standards. The actual result data is not included with the submission, so that an assessment of their quality is not possible. I find this particularly problematic since the manuscript is widely lacking critical reflections on the method, even though its weaknesses are becoming obvious in the results despite their high grade of generalization. English language of the manuscript is subject to abundant basic errors, some of which make it impossible to understand what sentences means. In summary, I find that the manuscript cannot be considered for publication owing to substantial issues in methods and over-interpretation of questionable results. I consider the work required to thoroughly tackle these issues to be way beyond the time frame for revisions, so that I unfortunately have to recommend rejection despite the interesting topic and general potential.

In the following are my comments on some of the major issues of this manuscript that might help the authors to improve the study for a future submission.

The methodological evaluation is not robust and a critical evaluation of the actual data quality is lacking: - MODIS data is presented as the 'most recent and advanced remote sensing snow product' (L76) and treated as this throughout the manuscript. The fact that there are much more recent missions providing more detailed data is ignored, the adverse effects of the extremely coarse resolution of ∼500 m is hardly considered, and studies clearly indicating that the quality of MODIS snow cover is less accurate for HMA (Rittger et al., 2013) is not mentioned. - MODIS FSC data treated with the 'cloud removal method' by Tang et al. (2016) are presented as having a 'high snow-classification accuracy' (L94). However, a considerable part of the methods focuses on finding an empirical threshold to replace the number of days per year for perennial snow cover –which should clearly be 365– with a number that fits observations. - It is generally stated that there is a 'significant linear relation' of the MODIS-derived SLA-EMS results with WGMS glacier mass balance measurements (L209f), but the presented data clearly shows this is not the case. Only for six out of twelve glaciers 95% critical values of sample correlation are reached. Conversely, data for Leviy Aktru is basically uncorrelated and R values for Chorabari as well as Pokalde are far off the thresholds. Also, RMSE and R^2 values are not considered at all. - Using Landsat data to evaluate the MODIS snow cover results is a good idea. However, instead of simply checking whether the overall areas are equal I would consider it mandatory to investigate in how far the classifications match.

SLAs are substantially influenced by local topography, particularly slope aspect. The 500 km input data is hardly capable of detecting these, the 30 km grid resolution pixels completely ignores such effects. Therefore, the relevance of the findings regarding (individual) glaciers is highly questionable.

It does not become clear how data from the meteorological stations is treated, i.e. how averages are calculated over space and time, and how these are related to the results. As visible in Fig. 1, meteorological stations are extremely scarce in glaciated regions. There are no data at all for the Karakoram and the W Kunlun and most of the other stations are far away from glaciers, typically in completely different climate regimes down in the valleys – this is obviously known to to the author's according to L410ff. So why not use more appropriate data, such as the freely available ERA5?

Honestly, I don't understand why the manuscript focuses on the end-melting season. By this approach the greatest advantage of MODIS data, its high temporal resolution, is lost whereas the challenges regarding the quality of specific measurements are fully affecting the results.

Large parts of the discussion do not present content supposed to be there. While interpretations of the results and evaluations of their implications against the literature are short and remain superficial, many paragraphs basically repeat statements regarding the relevance of such remote sensing studies as well as the general, subjective praise of the method.

Recommended literature: Rittger, K., Painter, T. H. and Dozier, J.: Assessment of methods for mapping snow cover from MODIS, Advances in Water Resources, 51, 367–380, doi:10.1016/j.advwatres.2012.03.002, 2013.

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