Comments on Smith et al. – Glacier Mapping in the Tien Shan

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General Comments:

The revised manuscript by Smith et al. shows very significant improvements than the old version. The scientific theme is now very clear and explicit. The sections and contents are also well organized. On the scientific aspect, several issues still can be addressed from my view, and need further improvements:

1. I still argue that the lack of meaningful control dataset is the critical shortcoming of this study, which significantly lowers down the scientific quality of this paper. Although current control dataset has been improved in Google Earth, its validity to act as a control dataset still need to be proved. The comparison between the algorithm outputs and current control dataset (also act as the verification of the algorithm introduced in the paper) is yet inconvincible to me. A control dataset from existing near simultaneous high resolution satellite images, which was mentioned in my previous referee comments, is again recommended here.
2. In the results, currently it focuses more on statistical aspects of the comparison between the algorithm outputs and manual control dataset, which cannot clearly illustrate the performance of the algorithm. I recommend the author to provide more glaciologically meaningful comparisons besides current pure geometrical comparisons, such as the differences between glacier with different size (e.g., in different area ranks) and different type (e.g., hanging glacier, cirque glacier, valley glacier, etc.), as well as between clean-ice and debris-covered glaciers. They will provide more detailed information on the suitable application case of this algorithm.

   Furthermore, because the algorithm presented in this paper is mostly focusing on debris-covered glacier delineation, it is necessary to provide some comparisons with results of previous algorithms, such as Taschner and Ranzi (2002), Paul et al. (2004), Bolch et al. (2007), and Shukla et al. (2010), to better illustration the advancements of this algorithm achieved.
3. I agree with the author's opinion (Line 343-345) on the limitations of this algorithm that it can only be used at the scale of watersheds, satellite image footprints, or mountain ranges, and to provide "baseline set of glacier outlines which can be corrected manually" (Line 356-357), rather than for compilation of regional glacier inventory.

   Besides, from my view, the complexity of this algorithm and the involvement of many manual work during its performance (at least seven steps may need human interventions, includes rectification of Landsat images, extraction of velocity fields, lake detections, lowest elevation definitions, velocity and distance threshold determinations, seed lake points definitions, etc.) may largely limit its wide application even within such large scale studies. Many improvements are needed to further promote its automating ability, includes the automatic

optimization of the involved thresholds, and selection of the glacier delineation and post-processing methods. These aspects should also be considered by the author.

Currently it is hard to say that how much improvements this algorithm can achieve on the accuracy and efficiency of glacier delineation than the widely used glacier delineation methods, i.e., automatic delineation of clean-ice area with manual improvements, and manual digitization of debris-covered area.

4. The text in the last part of Discussion (Line 360-370) truly illustrated the other real limitations of the algorithm introduced in the paper, which put more limitations on its applicability. To make the scientific contribution of this paper clear, I suggest the author to rewrite the **Abstract** and **Conclusion** section, to better illustrate the advancements and limitations of the algorithm presented in the paper.

Specific Comments:

Abstract: See 4 in **General Comments**.

Line 3-5: I don't see many necessities to mention glacier volume change studies here. Furthermore, several literatures should better not be cited here:

1) Aizen et al. (2007): The paper actually didn't use any of the new remote sensing techniques. It just depends most on the empirical formulas, and on some ground penetrating radar measurements and SRTM elevation data.

2) Bolch et al. (2012): It just provides summaries of some recent studies focusing on Himalayan glaciers rather than on new remote sensing techniques of glacier volume changes

Line 26: "where the debris is sourced from" is inappropriate. Most glacier surface debris comes from accumulation area and subglacial moraine, rather than the hill slopes around glacier tongue.

Line 44: "… and ideal test area" should be "… an ideal test area"?

Line 65-68: Which version of the SRTM data was used should be clarified because large difference exists among different versions.

Line 80: should the "and any …" be "and many …"?

Line 76-98: A carefully designed illustration should be better to present the contents of this section. Current organization somehow conflicts with following sections and is hard to follow. The sequential numbers also conflict with the main sections.

Line 112: "as high-quality georeferencing is already included in the L1T product", is this conclusion comes from your validations? If not, please provide some references to prove it!

Line 114: What is the meaning of "hydrologically corrected DEM"? Please give some illustration.

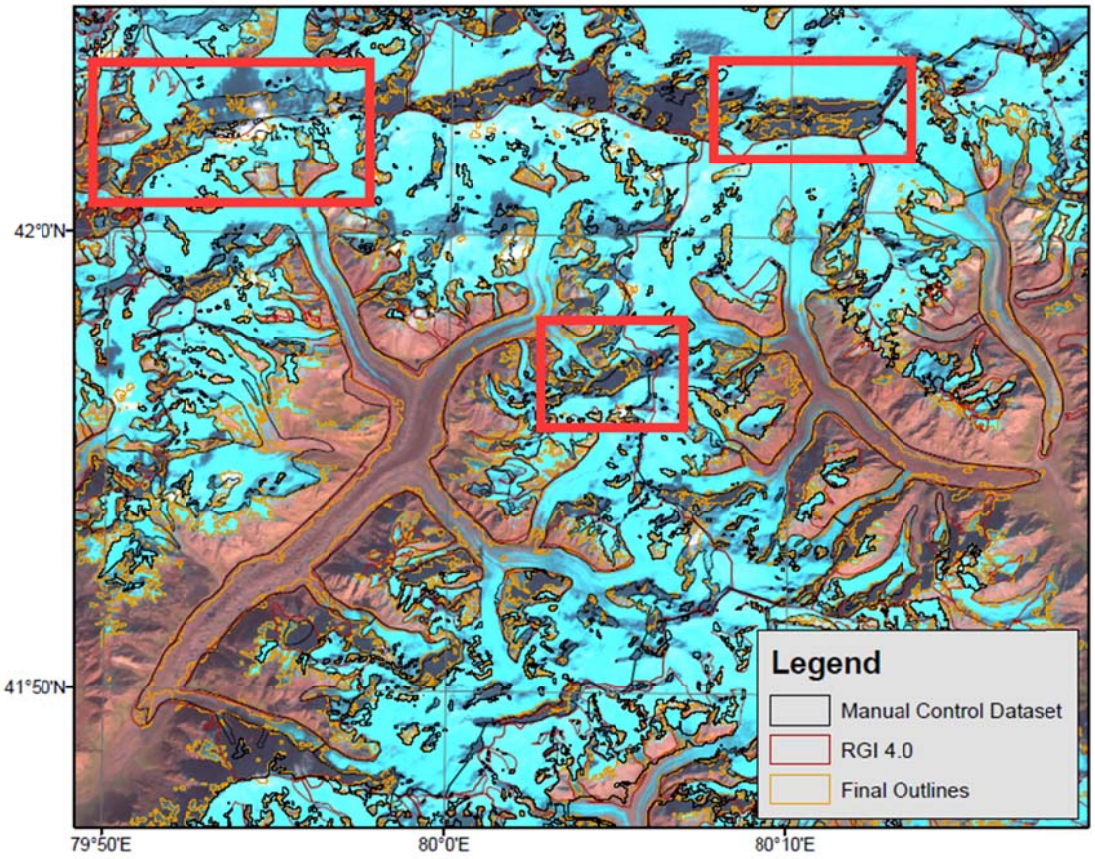
Line 141-142: Again same issue as my early comments (see general comments 3.1A in tcd-8-C2680-2014-supplement.pdf) arise here. Your response in tcd-8-C2882-2015-supplement.pdf mentioned you have changed the threshold of TM3/TM5 to 1.5. Is it typing error? The results shown under the correspondent response

in tcd-8-C2882-2015-supplement.pdf give overestimation of clean ice in accumulation area, but it seems coming from the extensive seasonal snow cover in the image you used, rather than smaller TM3/TM5 threshold.

Line 167-170: The image correlation methods required that the glacier surface has similar structures or spatial patterns. I noticed from Table 1 that 7/8 of the Landsat scenes use image pairs with time interval longer than 10 years. Glacier surface may change dramatically from year to year, no matter the clean-ice or debris-covered area. Ten years must be too long to perform image correlation. So I suspect about the validities of the extracted velocity field to filter the glacier surface pixels. The ideal time interval between the selected image pair no should longer than several years. I suggest the author to reselect the image pairs with short time interval, e.g., 2-3 years.

Line 176-177: "cloud-free and snow-free images" should better to be "cloud- and snow-free images".

Line 256-263: The manual control dataset seems containing many bias itself, especially in the regions under hill shadow, which occupy very large area and can be identified from Figure 13 (see red rectangles marked on its screenshot below). The bias of algorithm results towards high elevation areas thus should partly attributes to such misclassifications. Although manual digitization of glacier outlines in accumulation area is more tedious than other region, it is worth to be carefully performed to provide better comparison.



Besides, the purpose of most study focused on whole glacier area rather than the clean ice alone, even if the manual digitization of debris-covered

glacier area were needed. In this sense, the comparison between spectral dataset and other two datasets seems unnecessary. The bias caused by inclusion of debris-covered glacier area is therefore unmeaningful.

Line 264-277: Most reasons of mismatch between the algorithm and manual control datasets described here are reasonable, but I suppose that if the quality of the control dataset can be further improved, the distribution of over- and under-classified glacier area along elevation should be slightly different with current one.

Line 264: Maybe “intrinsic” or “inherent” is better than “persistent” here to better represent the meaning of natural shortcomings of the algorithm.

Line 279-283: Maybe the distances between the vertices of algorithm results and their nearest vertices or lines in manual control dataset are more meaningful here, because generally the manual digitized glacier outlines contains far less vertices and fewer shape variations than the automatically delineated results.

Line 283: What’s the meaning of “Normalized Distance” in Figure 11? Please give some illustrations around here.

Line 290-298: This section seems duplicates with 4.1. It should better to be merged into 4.1, or be totally removed.

Line 300-318: From my view, this section is unnecessary in current paper, because they only described the failed method experiments that give no improvements on the glacier classification. It should better to be totally removed.

Line 325-328: This sentence is hard to follow. What are you actually wanted to say here? To substitute the thresholding method of clean ice? Please rewrite it.

Line 335-338: From my view, the comparison between the algorithm and manual control datasets and RGI in this region is unnecessary. The lower quality of current RGI in this region is well known, therefore should not to be regarded as a reference.

Line 350-352: I didn’t think the velocity data extracted from different image pairs can be “static in time”, because large differences may exist among different images, although the topographic data can be considered as “static”. “same areas are captured …” may mostly due to the “static” topographies.

Line 357-359: How the “processing time” can be “decreased when a large set of Landsat scenes are considered”? “as generating the input velocity surfaces can take longer than processing glacier outlines from dozens of Landsat scenes”? Is it a typing error?

Line 370-379: These findings are very meaningful. However, they seem not tightly connected to the contents of this paper. They were suggested to be shorten.

Line 381-397: See 4 of **General Comments**

Table 1: “Bold dates indicate use for Velocity profiles” should better to be “Bold dates indicate images used for Velocity profiles”; see Line 167-170 for further comment.

Figure 4: The higher velocities in the regions along the river and on the hill slopes aside glacier tongues make me further doubt about the validity of velocity fields extracted in this study; also see comment on Line 167-170.

Figure 9: See comment on Line 256-263.
Figure 10: See comment on Line 264-277.
Figure 11: See comments on Line 279-283 and Line 283.
Figure 12: See comment on Line 290-298.
Figure 13: See comment on Line 256-263.
References: (NOTE: The references were only checked in a selective way)
DOI is needed for the literatures.
Line 433: Delete the "." after "1999-2011".
Line 435: Delete the "." after "Peru".

References mentioned:

Aizen, V. B., E. M. Aizen and V. A. Kuzmichonok (2007). "Glaciers and hydrological changes in the Tien Shan: simulation and prediction." Environmental Research Letters **2**(4).

Bolch, T., M. F. Buchroithner and A. Kunert (2007). Automated delineation of debris-covered glaciers based on ASTER data. 27th EARSel Symposium "Geoinformation in Europe", Bozen, Italy.

Bolch, T., A. Kulkarni, A. Kaab, C. Huggel, F. Paul, J. G. Cogley, H. Frey, J. S. Kargel, K. Fujita, M. Scheel, S. Bajracharya and M. Stoffel (2012). "The State and Fate of Himalayan Glaciers." Science **336**(6079): 310-314.

Paul, F., C. Huggel and A. Kaab (2004). "Combining satellite multispectral image data and a digital elevation model for mapping debris-covered glaciers." Remote Sensing of Environment **89**(4): 510-518.

Shukla, A., M. K. Arora and R. P. Gupta (2010). "Synergistic approach for mapping debris-covered glaciers using optical-thermal remote sensing data with inputs from geomorphometric parameters." Remote Sensing of Environment **114**(7): 1378-1387.

Taschner, S. and R. Ranzi (2002). Comparing the opportunities of Landsat-TM and Aster data for monitoring a debris covered glacier in the Italian Alps within the GLIMS project. Geoscience and Remote Sensing Symposium, 2002. IGARSS '02. 2002 IEEE International.