

Comments on “Improving semi-automated glacial mapping with a multi-method approach: areal changes in Central Asia”, by T. Smith et al, submitted to *The Cryosphere Discussion*

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General comments

The manuscript submitted by Smith et al. described a set of new methods mainly focusing on the improvements on the automatic delineation of debris covered glacier, and gives some results of glacier area change, taken Tien Shan Mountain of Central Asia as their study area. My general comments are as follows

1. An overall remark is this manuscript was not well written, includes the writing style, the organization of sentences, paragraphs, and section, and also the English expression. Some words, like “decipher” and frequently used “glacial” in Abstract and subsequent contents, were not very popular in glaciological research paper. Detailed comments can be seen from specific comments on each section.

2. The second remark is about the focuses of this manuscript. From my understanding, I think this manuscript mainly want to describe a new method to delineate debris covered glacier. But it also focuses on the glacier change, and even on the atmospheric setting. From my view, too many focuses sometimes equals to no focus. It is better if the authors can give up other focuses, return to the main one, and make everything is clear about it, includes give more details.

3. Regarding to the methods developed in this manuscript, many problems can be addressed according to my personal experiences.

3.1. The clean ice area will be significantly underestimated by these methods due to following reasons:

A. The unique band ratio threshold (2.0) during the delineation of clean ice glacier area via band ratio method needs to be argued. It cannot always fulfill the demands of precise clean ice delineation from any Landsat image. From my experience, it is too larger in many cases, especially from images acquired during ablation season.

B. The additional 250 of TM1 DN value is too higher because the clean ice area of glacier below equilibrium line during ablation season is fairly dark due to the exists of melted water. This will eliminate many ablated clean ice area.

C. Actually glacier tends to be survived hiding behinds the hill shadow in North Hemisphere. Therefore the exclusion of area in hill shadow is also problematic, and will exclude many glacier areas, which can be clearly seen from Figure 3 and 4.

3.2. Even among the new methods to delineate debris covered glacier area, there are also many uncertainties introduced.

A. The topographical filtering step seems too subjective to exclude debris covered area with relatively flat topography. The coarse resolution SRTM also cannot support for this even if the glacier surfaces in this region are fairly rugged.

B. Although the idea is very innovative, the glacier velocity filtering step will also bring very larger uncertainties, because the unknown accuracy of the glacier velocity extracted.

C. The spatial filtering step uses the HydroSHEDS 15s river network to remove “potential debris areas”.

However, from my check, the river network was so sparsely distributed, and the author didn't give the weight of two different distances, I suspect that it will also eliminate many real debris covered glacier area.

D. In the final mask step, from my experience, even a single 3×3 median filtering will bring large changes on the shape of final glacier outline. However, according to the author's description, totally 4 kinds and 6 steps of filter, include 3×3 median filter (applied twice), 5×5 median filter, image opening (twice in total), and image bridging, were applied. After so many filter steps, I wonder whether the resulted glacier outline can represent the true situation of glaciers on the Landsat image.

4. The most important problem of this manuscript is the lack of a convincing referential dataset to evaluate the accuracy of the results from all these methods it illustrated. The authors use manual digitized outlines for ~750 glaciers as the control datasets. But as the authors stated in the paper, the results of manual work has very larger uncertainties, especially from coarser resolution like Landsat images and for the debris covered glacier area. I have checked the Google Map, and found there are many higher resolution images in the regions this manuscript involved. I suggest the authors do similar works as Paul et al (2013) to validate their results by referencing the results from Google Map image, both for clean ice and debris covered glacier area.

5. From my view, this manuscript actually gives no real results in the "Results" section, especially if respect to the title of the manuscript "areal changes in Central Asia". Even in the "Discussion" section, the glacier changes presented are not persuasive to me because of larger and unverified uncertainties introduced by the multi-step method illustrated. Besides this, theoretically multi-step method has its own shortcomings because each step will introduce some errors. The error will unavoidably be cumulated together and made the uncertainty of final results fairly larger and unevaluable. That's why simple methods can often give the best results.

Specific Comments

Abstract

The abstract was not well summarized. From my point of view, I suggest the authors give some emphasis on the major improvements of the methods to delineated debris covered glacier area. But in current version it is too generalized.

P5435:

Paragraph 2: this paragraph is also not well organized. The previous two sentences are illustrate the importance of glacier length change, but the left parts were more focusing on glacier outline delineation

L9: "focused on" is suggested rather "focuses on"; **besides, from the entire manuscript, I cannot found many contents focusing on glacier length change. Is it a true focus of this manuscript?**

L20: 15-30m only corresponds to half to one pixel of most bands of ETM+. If this is the real meaning, it should be clarified by state the panchromatic band of ETM+.

P5436:

L14: Maybe "based on" is more suitable to represent the meaning of the author rather than "Building on"?

P5441:

L6: The residual error of Landsat images compare to their master image in this step should better be given in proper form, as it is important to determine the accuracy of glacier velocity and glacier change in the following contents.

L25: From my experience, the clean ice of glacier always has similar NDWI value with water bodies, how the authors solve this contradiction should be clarified, along with the method to differentiate the frozen lake with glacier surface, because many Landsat images used in the study were acquired during winter

(can be read in Table 1).

P5442:

Section 3.3: In my opinion, currently this section is somehow confusing. The author should better add a general introduction in front of all sub-sections for all methods used to delineate glacier. My understanding is only the 3.3.1 is the delineation of clean ice surface, while all others are focused on debris covered glacier area. In current version, it is little confusing if one did not look into those sub-sections in detail. It is also suggested to change the title of 3.3.1 into “clean ice delineation”, and organize all other section into 3.3.2 like “delineation of debris covered glacier area”.

L9: “sensors on Landsat 8” duplicates with previous “Landsat 8 OLI”.

L11: From my experience, the optimal band ratio threshold to differentiate glacier and non-glacier is not unique, and many glacier surfaces has TM3/TM5 ratio of less than 1.5. Can you make certain that the value of ≥ 2 is the best to delineate clean ice glacier? Will it underestimate the area of clean ice?

L12: The value of > 250 is confusing. Do you mean it is the DN value of OLI2 of Landsat 8? If it really is the DN value of TM1, I believe most of glacier surface will be eliminated as non-glacier or debris covers because the brightest DN value is only 255, especially if the image was acquired during strong ablation season.

L15-19: If you simply exclude all areas in cast shadow, how to appropriately detect the area of glaciers hided by shadow (improper exclusion can be seen from Figure 3 and 4)? In the other hands, is the 90m SRTM DEM has the sufficient resolution to accurately simulate the topography by this method? So I think there are enough reasons to doubt about the precision of the final clean ice glacier outline.

Section 3.3.2: This section is also not well organized, and the section header is totally not coincident with its contents. The first paragraph can be called as real “topographic filtering”. However, the second and third paragraphs are talking about the glacier surface velocity rather than glacier topography, and should better to be called as “kinetic filtering” or “dynamic filtering”; the fourth paragraph is again talking about “frequential filtering” rather than topography. Besides this, since the experiments on frequential filtering were not successful, I suggest to put it into Discussion section?

L22: The glacier surface topography has very larger variances for different glaciers and under changing every day, and nobody can make sure their surface is flatter or rougher that other surface. As the authors have stated, the glacier surface only “tends to be rougher than the surrounding areas”, but the reality is this assumption is not valid for all glacier surfaces. Therefore, I think this first step to delineate debris covered glacier is ambiguous, and may lead to some exclusion of real debris covered glacier area in certain cases.

P5443:

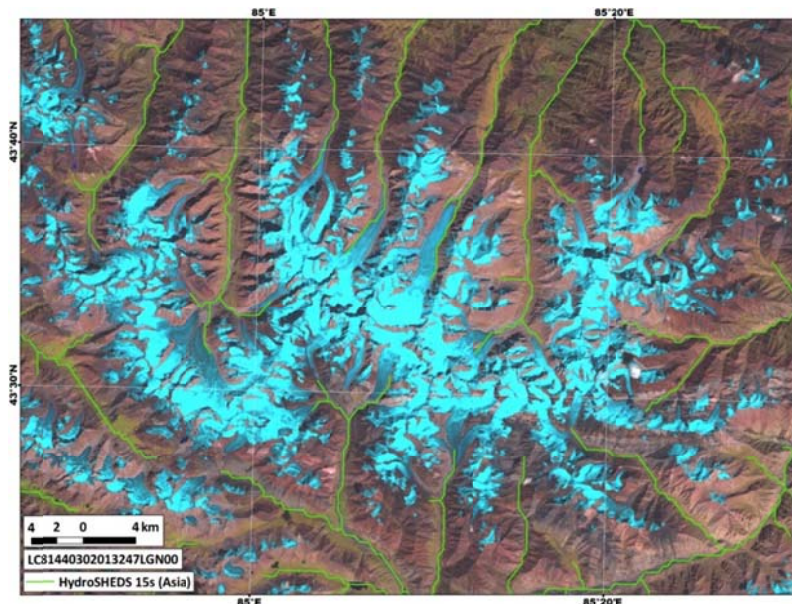
L12: I noticed that acquisition date of Landsat 8 OLI images used in the study are fairly different, and the glacier velocity is known has seasonal differences. So how the measured glacier surface velocities were standardized into $m\ yr^{-1}$ should be clarified here. Furthermore, as Käab (2002) didn't gave the accuracy of the measured velocity, and the original thesis that describe the CIAS software are not in English, the authors should better give some illustration on the accuracy achieved by CIAS, because of the importance of glacier velocity in determining debris covered glacier area.

L26: “Two additional topographic indices ...”: This paragraph is actually talking about some experiments in frequential domain rather than topography, so “frequential indices” is more appropriate here. Furthermore, as mentioned above, because the experiments illustrated in this and the following paragraphs were not successful, it is suggested to put these two paragraphs into Discussion section.

P5444:

L27: I checked the HydroSHEDS 15s of Asia, and found that this data is very coarse in depict river networks,

and can only illustrate larger trunk rivers (see picture below). So I suspect that if using such river networks to determine the rationality of debris covered glacier area, it will eliminate many debris covered areas of small glaciers in this step, especially those cirque glaciers or small valley glaciers. Even by using 200m buffer, this situation cannot be well overcome. If it is solved by add manual seeds in second step, the manual workloads should be very heavy.



P5445:

L24-27: As the authors have stated, the determination of proper thresholds of all parameters must be difficult to accurately delineate glaciers because so many methods have been introduced, and most parameters must be site dependent. So I believe that the portability or the universality of these methods must be very limited.

P5446:

L4-10: Although the spatial filter is very effective in removing the isolated pixels and bridge gaps and fill holes with glacier, it will largely change the shape of accurately delineated glacier outline, especially if using more than once. This will no doubt bring larger uncertainties in the final results.

L18-23: The manually created glacier outlines must have very limited accuracy, especially for the debris covered glacier area, and should not be regarded as control datasets to validate the results of the methods used in the study. As has been suggested by Paul et al (2013), the higher resolution images in Google Map can provide excellent referential source. So I suggest the authors choose this better way to validate the results of this study.

P5447:

Section Results: In my opinion, the results section actually gives no results. Meanwhile, the bulk elevation distribution of different results (Figure 5, 6, and 7) can not give a good description of the accuracy achieved, as well as the situation of Figure 8 and 9. Many actual differences will be covered by this way. In this sense, the area differences for different glacier area classes between the three datasets can provide useful information on their comparison, especially between the results of authors' methods and the manually digitized dataset.