

Interactive comment on “Supraglacial debris cover assessment in the Caucasus Mountains, 1986-2000-2014” by Levan G. Tielidze et al.

Anonymous Referee #2

Received and published: 12 December 2017

This paper focuses on assessing multi-temporal trends in area supra glacial debris cover (SDC) for the years 1986, 2000 and 2014 using a variety of imagery (Landsat series, SPOT). They also track the change in the number of SDC glaciers, and attempt to give explanations for the differences in behavior from the northern to southern slopes. Given that changes in the debris cover in this area are not very well documented, this is a noteworthy effort. However in spite of the efforts undertaken to process the Landsat scenes and to try to explain the topographic controls, the paper does not feel very focused; the information could be selected in order to answer scientific questions and perhaps remove information that does not feel relevant.

While the focus of the paper seems to be on the SDC changes, much of the paper discusses differences between various techniques of mapping SDC, which seems to

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be a different topic altogether and distracts from the main objective. I suggest that instead of presenting the various approaches, which do not bring much innovation, the authors just choose their approach and conduct an uncertainty analysis and then focus on the area change.

The main concerns are: - Objectives need to be clarified, and the methods and results adjusted accordingly - The excessive increase in SDC seems questionable, particularly with respect to changes in the internal rock area and overall the methodology used - Regional trends reported here so not yield detail about glacier-by-glacier area changes which might be subject to SDC mapping errors - GPR section on ice thickness seems irrelevant here and is outside the scope of the paper, it does not fit with the rest of the analysis - A comparison with other studies focused on area changes is missing, and it is not clear if this study is in line with other studies regarding the increase in area changes

Specific comments are below:

Abstract

The abstract is quite clear but then the paper diverges a bit from it. The authors may specify why they chose to focus on different parts of the ranges, to compare and contrast. The number of debris-covered glaciers is really not important, as there are many processes that could be involved ie. glaciers might disintegrate rather than form and grow within the period of a few decades.

Introduction

The introduction lacks focus, despite bringing in some background information about the importance of glaciers. For ex. the issue of debris thickness is mentioned but the objectives of the paper do not target debris cover thickness. The introduction would need to be revised/focused. Most of the statements need to be expanded since they do not fully explain the gaps in knowledge, and remain vague, for example p 2 lines 31

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to 37:

“The Greater Caucasus SDC is an important control for ice ablation, as it is similarly in many other glaciated areas (Lambrecht et al., 2011) and has been identified as a key player in glacier mass balance (Popovnin and Rozova, 2002).”

This would need to be expanded and the concepts explained.

Similarly: “In addition, in some cases SDC and proglacial lakes are directly related to glacial hazards” This remains vague and issues are mentioned but not developed.

Study area and previous studies

The different parts of the range are mentioned but it is not clear how these were defined. The 1st paragraph on p.4 is indented to be a literature review but it mixes glacier area change with debris cover mapping, these are not clearly addressed. The authors could list the studies that determined area changes, the extent of the areas and the remaining gaps; then the debris cover mapping issues could be addressed separately.

P4 l.12- 15 “This research further aims to compare glacier mapping using manual and semi-automated methods to assess the SDC change across a larger area than previous studies according to the western, central and eastern Greater Caucasus over the last 30 years.”

This paragraph sounds like it belongs to introduction, not study area/previous studies.

Also, here the objectives do not come out clearly; while in the abstract and introduction the author mention the need to document glacier area changes, here this points to another objective, which is to compare various methods to delineate debris cover.

Overall I suggest that the authors sharpen their objective and focus on glacier area changes in the various parts of the ranges, perhaps splitting the glaciers into clean and debris cover glaciers. To achieve this objective, the authors could just choose their best method for delineating debris cover and provide an uncertainty analysis. As

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written currently, this almost contains two different studies and it prevents this from developing either.

3. Data and methods

Sections 3.2 to 3.4 summarize various methods for delineating debris cover, and it seems to be a literature review. This is fine for a paper that would focus on comparing various methods, but here the objective seems to be different and this much detail might not be needed. Sections 3.2 to 3.4 could be combined and shortened, and the authors could just present the approach they took in a section on glacier clean ice and debris cover delineation, for example.

Furthermore: the method tested is presented for the 2013 image, and it's not clear if the same method was applied also to the other images. These sections can be clarified.

P5 l 13- 16 : I do not see the use of presenting the Alifu et al 2015 methodology here, if it is shown later (in results) that their method did not work for this study. I suggest removing the comparison with the Alifu paper in this section as well as in results (p.9 l .16-19). Again, this distracts the reader from the main goal of the paper which gets buried into the details of SDC mapping.

Section 3.5 on comparison with manual digitization can also be presented as part of the glacier mapping section.

Fig.3 seems to belong to the results section, or can be presented as part of the uncertainty analysis.

The SPOT imagery used to check the SDC tongues is from 2016, but the Landsat imagery was from 2014- are the authors certain that there was no change in the glacier extents? Area changes might be small but I am not sure this can be used as a check for the classification if the images are not from the same year..

4. Results and discussion

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section 4.1 Supraglacial debris cover (SDC) assessment using Thermal/Near-IR/Mid-IR band ratio methodology

Again the focus here is not on assessing the accuracy of the delineation methodology but on the changes in debris cover, it seems- in this case it is unnecessary to mention the method used in the section title.

p. 9 | 16- 25: see my comments above about Alifu et al paper. I suggest removing this comparison and just focusing on the method presented, if the authors are confident in it.

Fig. 4 in the manual outlines, it is not clear if the NW part of the glacier tongue delineated as debris cover is supraglacial debris or just moraine. Have the authors checked this area with the high-resolution imagery? I suggest that in fig 4 it would be more helpful to present the high-resolution imagery rather than the Alifu et al outlines.

P10 | 4-5 “Therefore, the Alifu et al. methodology to identify debris covered ice in the Greater Caucasus cannot be considered robust for OLI imagery, and unsuitable for extended time series”. Again the evaluation of Alifu et al method is beyond the scope of the paper as I understand it, and the authors would benefit from focusing on their own method here.

P10 section 4.2 and 4.3 similar to my comments about the methodology, the paper wonders about here with the comparison of the different methods. I think one method should be chosen over the other and the results of that classification method should be chosen.

Table 2: - total glacier number should be “total number of glaciers” - glacier number should be “number of glaciers” since the authors do not refer to glacier IDs - “debris cover should be “debris covered ice”

Figure 6 does not tell much in its present form, since there is almost no change. This could be just mentioned in the text, with the

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Results on SDC p. 11-12 are hard to follow, and in some parts are descriptive, some ways of improving include: - Separating the glaciers only by range rather than range and glacier area. For ex, in figures 7 and 8 it is hard to distinguish any difference among the ranges since different colors are used for the same class (DC) - Clarifying what “percent” the authors are referring to- sometimes this relates to - It would help if instead of average slope, the authors did a correlation between slope of each glacier and its - The large changes (+49

Fig 11 a: The multi-temporal glacier mapping extents shown here are questionable, looking at the area covered by the nunatak. It seems that a) either a lot of the nunatak was exposed in 2016 by surface lowering, or b) that the 1986 image has a lot of snow which covered some of the area around the nunatak. In this case, the area changes are affected by the way that the nunataks were mapped.

For consistency, a number of authors chose to consider the area of the nunataks constant and only estimate the area changes due to glacier retreat. This issue should be carefully assessed since it can be introduce large errors in the change estimates.

Fig 12: the glacier outlines overlaid on the SPOT imagery do not seem to much; it is hard to see but it seems like the SDC area is over-estimated (unless there was a large area change since 2014 which is unlikely).

Also I am not sure how the GPR analysis helps here, it seems to have been added to the paper since the measurements were taken; however the purpose of the paper is not to provide ice thickness measurements; The GPR section seems to be out of the purpose of the paper and can be removed/saved for a future paper on glacier thickness.

Fig 13: looking at the areas of increase in DC, it seems that some of the “apparent” increase is indeed due to difference on mapping. For ex fig 13b shows an increase in DC area in the glacier accumulation area, which seems to be rather due to the way that the rock outcrops were mapped. Similarly there is a large increase where two glacier tongues merge- this can be a similar case of mapping differences.

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Conclusions :

yr +1 should be yr -1

Given the arguments above, I question the main conclusion about the significant increase in SDC in the Caucasus- I think an uncertainty analysis is pertinent before conclusions, and a careful assessment of mapping consistency should be addressed. Furthermore: have any other studies reported an increase in SDC in this area? This should be included in the discussion of the paper to assess whether this is a trend noticed in other studies as well.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2017-96>, 2017.