Review The Cryosphere – Cast shadows reveal changes in glacier thickness

Pfau et al.

General comments:

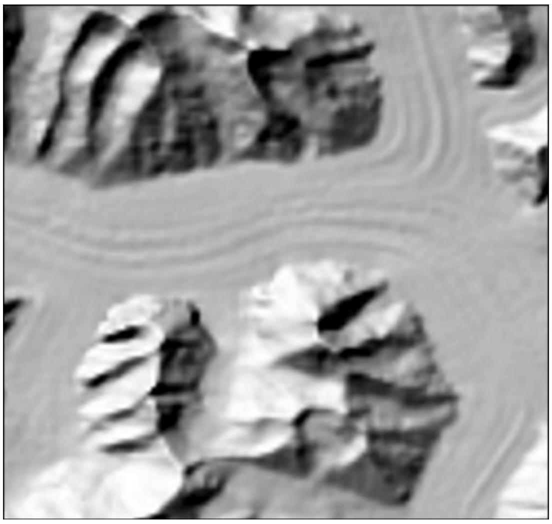
This research proposes to estimate glacier surface elevation change by using the length of shadows cast by surrounding topography. Specifically, it relies on a reference DEM from which shadows are modelled at times corresponding to several Landsat acquisitions over the 1990-2020 period. From the imagery, a binary thresholding on the green band is used to map the actual shaded area. The proposed method then derives the change in glacier surface elevation along the boundary of cast shadows. This is done using the difference in length between the modelled and mapped shadows in the direction of illumination, and under the assumption of unchanged topographic gradient in that direction.

The method is tested on 5 glaciers that exhibit a prominent surrounding topography casting extensive shadows over parts of the glaciers. The SRTM 1’’ DEM (acquisition Feb 2000) is used as reference DEM for 3 glaciers (Sperry, Aletsch, South Cascade); a variation of SRTM 3’’ potentially mixed with other unknown data source (Viewfinder Panoramas DEM, VFP) is used for Baltoro, and ArticDEM for Gulkana. For each glacier and each landsat image, differences in shadow length are converted to height variations and analysed statistically with a Beyesian multi-level linear regression model to estimate linear trends of thickness change for each glacier. This suggests significant downwasting trends for Aletsch, Cascade and Sperry, while the author conclude thickening for Baltoro and no significant trend for Gulkana. A comparison of results with repeated DEMs is completed on all but Baltoro glacier. The effect of DEM source and resolution is assessed on Aletsch Glacier.

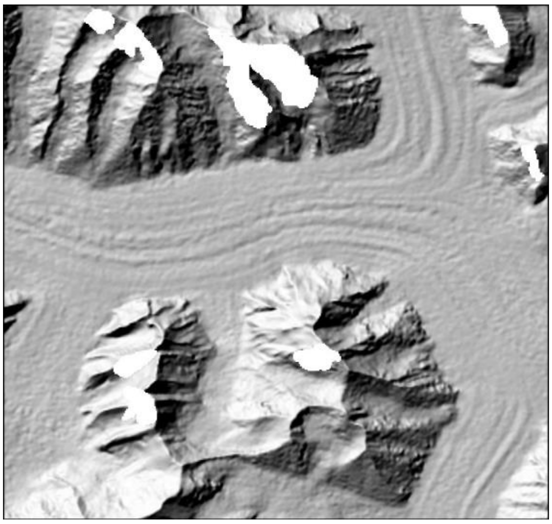
Overall, I find this contribution original and interesting but not overly convincing. It is clear and well written. The methodology is well and sufficiently explained, and the results can be reproduced. However, although limitations of the approach appear correctly identified, I find several shortcomings that require attention and significant revisions before this work can be considered for publication.

Specific comments:

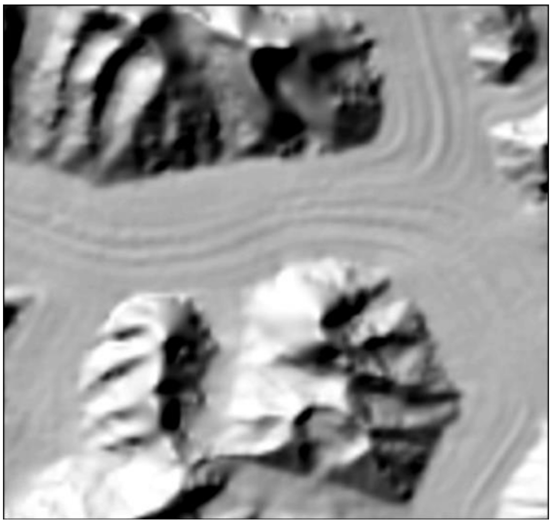
I find the use of Viewfinder Panoramas DEM for Baltoro Glacier arguable. This DEM is of uncertain quality. The authors themselves state that “date of the map basis of VFP is not known”. It also appears incorrectly referenced as 30m resolution in Table A2 although it is specified that VFP DEM in Asia is only at 3’’ (http://viewfinderpanoramas.org/dem3.html). Figure 1 in this review compares the VFP DEM with the SRTM 1’’ (30m) and CGIARSRTM v4.1 (90m) over Baltoro Glacier. It confirms the 3’’ resolution of VFP. Figure 1(b) also shows that SRTM 1’’ exhibits no hole that would compromise the shadow algorithm. Figure 1(d), however, demonstrates how different VFP is from SRTM 1’’, in particular over areas of significance to render proper shadows. In view of this, I don’t understand the choice made by the authors to mix VFP and SRTM. I believe the analysis of Baltoro should be redone on the basis of SRTM 1’’ alone.



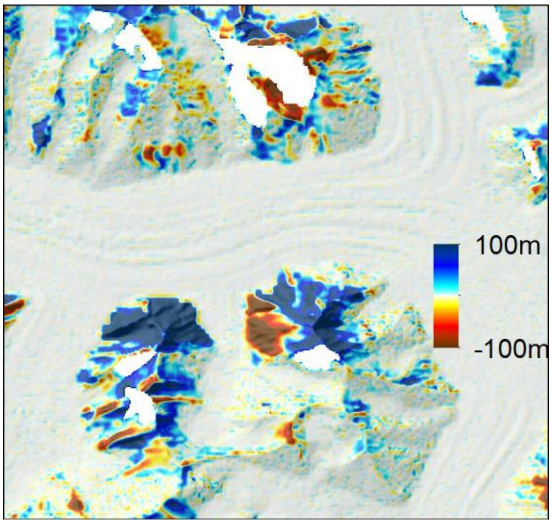
(a) VFP (N35E076.hgt, from http://www.viewfinderpanoramas.org/dem3/I43.zip, last retrieved 05/12/2022



(b) SRTM 1 Arc-Second Global (DOI:10.5066/F7PR7TFT) (tile n35_e076_1arc_v3.tif from USGS)



(c) CGIAR SRTM v4.1 (https://srtm.csi.cgiar.org/wp-content/uploads/files/srtm_5x5/TIFF/srtm_52_05.zip)

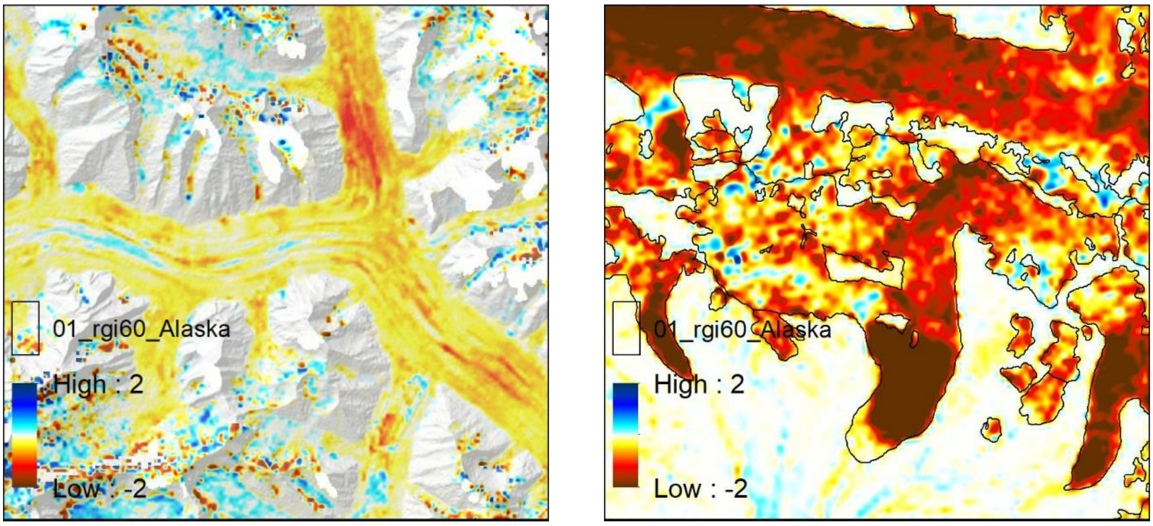


(d) VFP minus SRTM 1''

Figure 1 Various DEM of Baltoro glacier and difference between VFP and SRTM 1''

For Baltoro Glacier, the authors also state in P9L192 that no data are available for comparison. I would recommend that the authors give more consideration to Hugonnet et al. (2021) as data are readily available from https://www.theia-land.fr/en/monitoring-700000-km%C2%B2-of-the-worlds-glaciers/.

By curiosity, I plotted the 2000-2019 rate of surface elevation change from Hugonnet et al. (2021) for Baltoro Glacier (Figure 2a). The spatial variability in surface elevation change illustrates one major limitation of the proposed approach. It reveals how trends along a path that is limited to cast shadow can fail to resolve significant signal and trends for the rest of the glacier. The unambiguous negative trend visible from Hugonnet et al. (2021) also potentially contradicts results from this study (e.g., figure 4 and statement P10L218 "*Baltoro Glacier shows slight gains in glacier thickness*") which cast concerns over the methodology and/or statistical testing. It may suggest that the inference derived from the statistical model are ill-informed or that the selective coverage of cast shadow is deceiving as it conceals the overall behaviour to the extent of drawing wrong conclusions. It appears necessary to revisit findings and conclusions with this in mind. Again, I am curious to see what would come from using the SRTM 1'' data as it may exemplify further the sensitivity of the method to the DEM.



(a) Baltoro, 2000-2019 (m/year) (b) Gulkana, 2010-2019 (m/year)

Figure 2 Surface elevation change form Hugonnet et al. 2021 (https://www.theia-land.fr/en/product/rate-of-glacier-elevation-changes-from-2000-to-2019/)

Another useful comparison can be made for Glkana Glacier. Hugonnet et al. (2021) map rates of change over the 2010-2019 period that are directly comparable with the trends and conclusion inferred by the authors from cast shadows. Figure 2(b) shows the contrasts in trends from the accumulation area with shadow cast by Ogive Mountain and those cast by Icefall Peak.

In this context, the authors state P10L217 that "*Annual rates of glacier elevation change at Gulkana Glacier are not credibly different from zero*", and strengthen their conclusion P11L237 by stating "*At Gulkana, both our method and high-resolution DEM suggest the highest uncertainties in the estimated trends, leaving little room for a credible trend in glacier elevation change*".

While I could conceive that the author's method finds not trend from shadow cast by Ogive Mountain as it would correspond to marginal rate of change in Figure 2(b), it would be expected that shadows cast by Icefall Peak yield a significantly negative signal. While revisiting the results in view of these data, it would be useful to separate signals from each mountain and compare critically with the rates assessed by Hugonnet et al. (2021). The conclusion that annual rate is not credibly different from zero must be reassessed as it either echoes again a significant limitation of the method, or it compromises findings from Hugonnet et al. (2021). At this stage and with the evidence provided by the authors, I believe the former remains more credible.

By contrast, rate inferred for Sperry, South Cascade, and Aletsch seem to compare better with Hugonnet et al. (2021) although the trends derived over the 1990-2020 period may subdue that assessed by Hugonnet et al. (2021) over the 2000-2020 period. Such detail assessment with a consistent dataset would be desirable and will provide more perspective on the validity and limitations of the proposed approach, while also shedding light on the contrasted and generally unconvincing agreements found by the authors with trends derived from repeated DEMs and historical maps.

Finally, the authors assess the variability of shadow predicted over Aletsch Glacier from various DEMs. This is a useful and well-thought comparison that does inform about uncertainties associated with resolving shadows. Nonetheless, I find the assessment falls short of considering the effect of using these different DEMs on determining a trend of elevation change. It would be necessary that the authors repeat the full analysis on Aletsch with each DEMs to fully determine how DEM propagate uncertainties into the linear model.

Technical corrections

P6L127 : “manually mapped shadow” should better be called “shadow derived from Landsat images” as it is not mapped manually but rather derived via thresholding on the Green band.

P6L129: A *geodetic line* is defined as the shortest distance between two points on the surface of the ellipsoid. I don’t think this is a relevant name for what is used here, namely a set of regularly spaced line in the direction of the sun at the time of image acquisition.

P19&20: Notes in both Tables A3 and A4 should read “at a lower” instead of “at an lower”.

P19TableA2: ViewFinder Panoramas DEM of Baltoro is ~90m (3’’ for ASIA, see http://viewfinderpanoramas.org/dem3.html).

P19TableA2: There is no SRTM 1’’ for Gulkana glacier