Review: Pfau et al., Cast shadows reveal changes in glacier thickness (tc-2022-194)

The authors of the manuscript present a promising new way of extracting glacier elevation change from the changing extent of shadows cast from direct sunlight. The manuscript clearly portrays the potential of this approach, and important discussion is made on the potential of scaling this approach to regional or global scope. I find the validation analyses slightly unconvincing, however; a general agreement between this approach and one "reference" classical DEM comparison of questionable quality is found at Grosser Aletschgletscher, which is used as the main argument for the use of this approach. From a critical point of view, one general agreement is not enough to prove the potential of this method. If the authors either had another convincing case-study, or put less emphasis on this one agreement in the text, it would not be as large of an issue. I also do not consider absolute proof necessary for this study to be a novel contribution and an interesting read. I have comments on the text and some details of the analysis, but I generally find it well written and not in need of significant revision. I therefore suggest minor revisions of this work, and I look forward to be able to share this with my colleagues when published in its revised state!

General comments

In the end of the discussion, you mention the potential to automate and therefore efficiently scale the analysis. This is what I think is the biggest take-home message, and I suggest that it should be reflected in the abstract. Doing this manually on single glaciers is only so impactful, but the fact that this approach utilises globally available data, with relatively small automation challenges, speaks to the massive potential of this approach!

I was initially worried about the use of terrain-corrected Landsat images, but with some handkerchief mathematics, I could calm myself down. I suggest modifying the reasoning that I present here and putting it in writing, as it strenghens your choice of data. My worry was that the images will be skewed depending on what DEM is used by USGS to correct them. Optimally, one should run the correction with the same DEM as the one you use for shadow simulation, which would of course add significant computational overhead to the approach. If that is not done, there will be an error that is dependent on the DEM difference and the incidence angle (the relative angle between the pixel coordinate and the satellite at the time of acquisition). Landsat 8 orbits at an altitude of 705 km and its OLI instrument has a swath width of 185 km according to Wikipedia, meaning that the incidence angle varies between 0 and about 7.5 degrees. This means that the DEM bias that is required to shift the image by one pixel is around 230 m, which is extremely unlikely. Therefore, it should not matter too much, meaning that a potentially incorrect terrain correction is fine. If this is brought up in the text, it would save others having to go down the same rabbit hole as me!

References are sorted inconsistently in the text. Sometimes they are sorted by year and sometimes alphabetically. This should be made consistent in accordance to the journal standard (which I believe is by year).

This comment requires no changes to the manuscript; it is simply a suggestion for the future. Filtering of the geodetic lines as shown in Figure 3e could easily be done programmatically. If all lines are given an index before cutting them, then the cut line that is furthest away from the sun for each index would be the line to keep, whereas all others are erroneous.

I am not a Bayesian wizard, and therefore needed help from my colleagues to understand the statistics that are perfomed in this manuscript. I was told that this is a good approach as it is simple and understandable for anyone that is into the field. For the others (like me), however, could you keep that in mind for Section 3.2? For example, it is not argued for why this is preferable over a simpler optimization method (e.g. least squares). I now understand that this approach simply keeps the outgoing uncertainty high when provided with few data, whereas a regular regression would result in one misleading line that may be completely wrong. An easy argument for your approach is that you have few data points, and therefore cannot use regular optimization tools. But this was not clear to me before I had spent a few hours figuring out what is going on. Also, in your equations, I see no factor for elevation. Elevation change is highly correlated with elevation (i.e hypsometric gradient), so different points at different elevation are expected to have different values. Can you argue in the text for why this is hopefully not the main reason for the spread you present later in the Results chapter? Because if it is, I would suggest you update the model accordingly.

Currently, there is no consideration of elevation change uncertainty between the DEM comparisons in Section 3.3. The scanned topographic map and the SwissALTI3D DEM of Grosser Aletschgletscher for example may have substantial offsets, which are usually quantified from neighbouring stable terrain. Since the North America DEMs have gone through peer-review, they must have an associated uncertainty in the publication. For Grosser Aletschgletscher, I suggest validating that differences on stable terrain are close enough to zero and putting it in writing, or in a new figure. Without considering this source of error, I would not trust the validation from Switzerland, and therefore cannot trust the statement of similar trends on L230.

Please validate the date of the ArcticDEM product you used over Gulkana Glacier. The ArcticDEM explorer only shows strips from 2011–2016 in that location, while you claim that your DEM is from 2009.

Specific comments

- L18: Mention that the thinning pertains to the points of measurement, not the entire glacier. Otherwise, people might cite this number incorrectly.
- L36: Is 141,000 km^3 for mountain glaciers or total land ice?
- L37: Change "cover" to "covers"; magnitudes are in singular.
- L40: Change "elevations" to "elevation"; magnitudes are in singular.
- L41: Change "(ICESat)" to "(e.g. ICESat)".
- L44: There are more large-scale long-term studies (e.g. Belart et al., 2020; Geyman et al., 2022; Mannerfelt et al., 2022).
- L48: All Landsat programme products do not have a resolution of 30 m. Only most bands of the TM, ETM+ and OLI instruments (see the Landsat program wikipedia article).

- L49: Change “mapped in Landsat” to “mapped from Landsat”
- L51: Change “help reveal” to “be used to estimate” or similar. Outlines themselves cannot reveal elevation change. The first reference uses area for a rough estimation of volume change, and the second uses them to crop their DEM differencing.
- L64: The DEM abbreviation is already introduced in the abstract, and the DEM abbreviation is used in L49.
- L81: Change “long(decadal)” to “decadal”
- L86: The line starting with “High and steep” is very short, and could be rephrased or combined with another.
- L87: Specify that “Concordia” is the name of a mountain.
- L89: Change “Gulkana” to “Gulkana glacier” to be consistent with L90 and to help the reader. I know that the word “glacier” occurs in the beginning of the sentence, but the wording is still not obvious to me.
- L105: Did you validate the georeferencing? Sometimes they can be off if not enough GCPs exist. Please just mention that this was (hopefully) not a problem.
- L109: Which ArcticDEM product was used? The mosaic or an individual strip? If an individual strip, which id?
- L112: Do you mean the swissALT3D DEM 2019 version? The acquisition year over Grosser Aletschgletscher is 2017–2018 as is correctly mentioned later.
- L122: It would be nice to know a bit more about why the green band was chosen. For example, did it have the highest difference between shaded and unshaded locations? It is mentioned that “shadows appear dark” in the green band, but not “darker” than the other bands.
- L125: Does the algorithm in SAGA account for the Earth’s curvature? The effect is in the order of 1 m in elevation per 3 km of horizontal distance, so this would matter somewhat at these scales. Please just mention if it is already accounted for, or mention that future improvements can be made by accounting for it.
- L128: Specify that it’s RGI version 6.
- L187: Are you entirely sure that the maps represent these exact years? Swiss maps are very often asynchronously updated over time, and can have large (tens of metres) differences to what one would expect (c.f. Fig. 10 in Mannerfelt et al., 2022).
- L207: The swissALTI3D DEM is a mix of lidar at low elevation and aerial photogrammetry at high elevation (see the technical information on swisstopo’s website). At Grosser Aletschgletscher, it is therefore most likely based on photogrammetry.
- L211: Repeat what the line spacing is. 45 lines is not informative unless the spacing is mentioned.

- L231: The topographic maps may have a high resolution, but it is far from guaranteed to have a high accurancy and precision. I would rephrase this from "high-resolution DEMs" to "reference DEM comparison".
- L244: Is the substantial variance found before or after considering the elevation dependent elevation change that is observed on almost every glacier in the world? If they are at relatively similar elevation, then say that: "... in spite of their similar elevation ...".
- L252: Where is the date of the GLO-90 DEM information from? On the associated OpenTopography website it says 2011–2015
- L277: Right now, you only show a difference in the horizontal variability that is associated with different DEMs, not the effect on calculated elevation change. My Figure 7 suggestion below would solve this problem.
- L294: The effects of SAR penetration would lead to a potential negative bias (longer shadows), no? But since this bias will be consistent between years, one would just have to subtract the shadow-derived elevation change at the year of acquisition of the DEM. So this is arguably a very simple fix. Could this be put in words, assuming you agree with me?
- L302: "Precision" (spread) AND "accuracy" (bias). There seems to be both consistent and inconsistent errors in the shadow-maps derived from poor DEMs.
- L309: Please elaborate on why you think winter months are better suited for this method. Lower solar angles, I presume? What about the contrast between shaded and unshaded terrain?
- L330: Steep topography is arguably not a requirement; just stable topography. Indeed, steep slopes increase the potential time of day at which shadows can be created, so it works better with steeper topography. But the approach still works in shallow topography; just at fewer hours of the day! On e.g. ice caps, there is no stable topography to cast shadows, so the approach fails. So steep topography and high latitudes are preferable, but stable topography is the only theoretical requirement.
- Figure 1: I just want to add that I think this is a great figure that explains the concept simply and artfully! Great job!
- Figure 2: The Randolph Glacier Inventory is mentioned without abbreviating it, contrasting it to Figure 3.
- Figure 3: The RGI abbreviation is introduced multiple times. Only RGI is necessary here.
- Figure 5: It is unclear where the grey bubbles come from. Are these from other satellite images than Landsat? Please clarify this in the text.
- Figure 7: I recommend to add another axis label on top where you show calculated height change assuming the solar parameters you decided. This would solve my issue in L277 that you have actually not shown the variable DEM quality effect on elevation change.

References

Belart JMC, Magnússon E, Berthier E, Gunnlaugsson AT, Pálsson F, Aðalgeirsdóttir G, Jóhannesson T, Thorsteinsson T, Björnsson H. 2020. Mass Balance of 14 Icelandic Glaciers, 1945–2017: Spatial Variations and Links With Climate. Frontiers in Earth Science 8:163. doi: 10.3389/feart.2020.00163.

Geyman EC, van Pelt WJJ, Maloof AC, Faste Aas H, Kohler J. 2022. Historical glacier change on Svalbard predicts doubling of mass loss by 2100. Nature 601:374–395. doi:10.1038/s41586-021-04314-4.

Mannerfelt ES, Dehecq A, Hugonnet R, Hodel E, Huss M, Bauder A, Farinotti D. 2022. Halving of Swiss glacier volume since 1931 observed from terrestrial image photogrammetry. The Cryosphere Discussions :1–32doi:10.5194/tc-2022-14.