The Cryosphere Discuss., 6, C2368–C2374, 2012
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The Cryosphere Discussions

Interactive comment on "Regional estimates of glacier mass change from MODIS-derived equilibrium line altitudes" *by* J. M. Shea et al.

Anonymous Referee #2

Received and published: 6 December 2012

General Comments:

In this study, the authors attempt to bridge the divide between high-temporal frequency remote sensing measurements and mass balance estimates for large icefields. They use the 250 m MOD02QKM product to map snow- and ice-covered pixels of index glaciers and icefields in northwestern North America. They attempt to extrapolate mass balance changes for entire icefields based on average mass balance gradients calculated from the nearest index glacier. The MODIS-derived mass balance estimates are then compared with estimates using a geodetic approach that is described in another study.

Initially the authors describe how the late ablation season snow-line elevation can be used as a proxy for ELA. However, they instead use the 20th percentile of the elevation

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of snow-covered pixels as a proxy for the ELA of an index glacier or icefield. They state that the ZS(20) metric is better correlated to surface mass balance for "most" index glaciers. It's still unclear to me why the average snow-line elevation was not used for each location. The finding that ZS(20) "yields the best correlation to observed surface mass balance" is interesting and deserves more detail in the results section. I agree with the first reviewer's point that "ELA" is often misused. ZS(20) is not the same as the ELA. The terminology needs to be changed in the Data and methods section and then made to be consistent throughout the paper. The terminology "regional ELA proxy" from the supplement reply to Dr. Pelto is more appropriate.

The Methods section of the paper is the major strength and presents a few novel ideas. However, there are portions of the Methods section that need to be described in more detail in order for the study to be repeated for other locations. Additional detail regarding the mapping of the snow and ice facies needs to be added to this section. This is a key component of the paper and the k-means clustering method for mapping snow and ice facies appears to be a novel method since the authors do not cite any other studies. I have made a few specific comments about this section.

I understand the data limitations associated with this study, but, like Dr. Pelto, I am concerned about using mass balance data from single index glaciers to estimate mass balance for entire icefields, especially since two of the three index glaciers are over 75 km away from the icefields which they are supposed to represent. The authors mention in the results section (P. 3764, L.12-14) that regional ELAs of an index glacier (Place) are not correlated with ELAs from an icefield that is 75 km away (Lillooet). Compared to their icefield counterparts, have these index glaciers shown similar patterns of area loss during the satellite record? Is there anything else that could be done to show that these glaciers are indeed representative of the icefields?

I also agree with the other reviewers that more results need to be shown in Tables and Figures. I think it would be particularly helpful to include Tables or Figures which compare the ELA proxy and the actual ELA for the index glacier sites.

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Please see my specific comments below. I realize there may be some overlap with the other reviews. The authors should make a brief note if a comment has already been addressed.

I feel that this paper should be published, provided that the comments made in this review are fully addressed. I am primarily concerned with (1) the method used to classify different glacier facies and (2) the representativeness of index glaciers for calculating the mass balance of large icefields and the associated error.

Specific Comments:

1. Title

The title should be changed since true ELAs are not actually determined

2. Abstract

P. 3758, L.4: The authors should be careful about using "MODIS-derived ELAs" to describe ZS(20). Also, the authors need to develop a consistent terminology throughout the rest of the paper.

P. 3758, L.9: The worst estimate of mass change (+32%) is not within 30% of traditional geodetic approaches.

P. 3758, L.9-10: Is this study really "revealing" continued mass change? I think another word, such as "corroborates", might be more appropriate.

3. Introduction

P. 3758, L.17: Instead of using "substantial", please provide a range from the literature.

P. 3758, L.23-24: Be more specific about how "glacier mass change affects surface runoff in glacierized basins."

P. 3758, L.25: Use a number instead of the word "handful."

P. 3759, L.18-19: ". . . closely mirrors the equilibrium line altitude (ELA)." Please provide

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a few references to support this claim. Some examples: (Klein and Isacks, 1999; Williams et al., 1991; Winther et al., 1999)

P. 3759, L.23-24: What was the availability of Landsat imagery? Approximately how many scenes were available during the ablation period of each year?

4. Data Methods

P. 3761, L.15: define HEG acronym

P. 3761, L.18: Mention GLIMS here.

P. 3761, L.18-20: This portion of the methodology requires more detail. If this method is to be applied at other locations then the reader will need more detail to carry out the same procedure. What program was used to perform the cluster analysis? A brief discussion of k-means cluster analysis would also be helpful. Did topographic shading or atmospheric variability affect clustering?

P. 3761, L.22: If possible, please include a Table showing the total number of scenes that met these criteria for each site.

P. 3762, L.3: Define acronyms GTED and SRTM

P. 3762, L.4: What program was used for resampling?

P. 3762, L.6: If possible use a number instead of "most."

P. 3762, L.6-9: The metric ZS(20) should represent the ELA and not "the elevation of the local transient snow-line." This point should be moved to the results section. I would be interested to see how ZS(10), ZS(20), and ZS(30) correlated to observed surface mass balance at each site. How does the actual, averaged snow-line correlate with surface mass balance? I'd like to see a comparison for one or two years showing ZS(20) versus the actual, averaged elevation of the snow-line extracted from the MODIS classification.

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P.3763, L.1: I realize that the methods are described elsewhere, but I think a brief description of DEM co-registration is important.

P. 3763, L.6: GLIMS acronym has not been defined yet.

P. 3763, L.6-9: Which standard error value is used? Is it the standard error associated with the MODIS-derived ELA or the average standard error calculated for the entire lowess curve? Also, please explain the choice of "an assumed error . . . of 10%."

5.Results

P. 3763, L.14: How favorably do the glacier surface types derived from MODIS compare with Landsat-derived surface types? In order to make a quantitative evaluation, I would recommend hand-digitizing the snow and ice facies in a few Landsat scenes and then resampling the resulting classes to 250 m. In this way, the "accuracy" of the MODIS classification could be determined using some simple quantitative statistics.

P. 3764, L.3: There is no mention of the trend associated with South Cascade glacier. The linear regression coefficients and statistics could be shown in a table.

P. 3764, L.4-8: This paragraph requires more discussion. Presumably, most of the other index glaciers that do not show significant ELA trends lost area from 2000-2011. So what does this mean if the ELA proxy isn't changing, but glacier area is declining? Are changes undetectable because of the relatively small time interval and moderate spatial resolution? How did the actual ELAs change during the same time? Instead of Figure 3 from the supplement reply to Dr. Pelto, I'd like to see individual linear regressions (actual ELA vs. ZS(20)) for each index glacier. This will be telling for whether ZS(20) is actually a good proxy for ELA.

P. 3764, L.9: Table 3 and Table 2 need to be switched based on the order that they are mentioned.

P.3765, L.1-2: How were estimates of volume change in mm w.e. converted to estimates of volume change in m i.e.? I did not see any mention of this in the Methods

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section.

P. 3765, L.6: I believe “Andrei” should be switched to Sittakanay.

P. 3765, L.9: It would be helpful to put the mass loss in context by comparing estimates from this study with those from other regions of the world.

6. Discussion

P. 3765, L.11-18: This paragraph should be moved to the Conclusions section.

P. 3765, L.17: “within 30%” is not correct since the highest difference was +32%.

P. 3765, L.24: Are there temporal trends associated with the mass balance gradients from other sites?

P. 3766, L.7-9: The actual ELA and ELA proxy may not match exactly, but the trend in ELA should be about the same.

P. 3766, L.24: How prevalent is debris-cover for these sites? Also, where should this methodology be applied next?

7. Conclusions

P. 3767, L.8: The only “marked improvements” are shown visually in two figures. While I agree that the improvements do appear to be significant, the authors need to quantify this improvement in order to make this claim.

P. 3767, L.11: Again, fix 30%.

8. Tables

Include a Table summarizing the GLIMS glacier area, elevation range, and number of MODIS pixels corresponding to each index glacier and icefield.

Table 2 and 3 should be switched.

9. Figures

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Figure 1: I would suggest changing the color of the symbols associated with the ice-fields. Also, there are no units on the scale bar. Some additional detail, such as an underlying DEM, would improve this map as well.

Figures 2 and 3: These figures are well done. To be consistent with Figure 1, please make sure the units of the map labels are in degrees. Also, indicate the band combination (it looks like 5,4,3). For Figure 3, indicate the location of the Bridge glacier.

In Figure 2a, it appears that either the Columbia Icefield includes a few debris-covered glaciers or that the GLIMS outlines are not correct for a few glaciers. Does the presence of debris or exposed bedrock in the lower portion of the outline affect clustering?

Figure 5: Show r2 values for each portion of the piecewise linear fit. The y-axis range should be the same for each subplot.

Figure 6: Show the r2 for each regression line.

10. References

Klein, A. G., and Isacks, B. L., 1999, Spectral mixture analysis of Landsat thematic mapper images applied to the detection of the transient snowline on tropical Andean glaciers: Global and Planetary Change, v. 22, no. 1-4, p. 139-154.

Williams, R. S., Hall, D. K., and Benson, C. S., 1991, Analysis of glacier facies using satellite techniques: Journal of Glaciology, v. 37, no. 125, p. 120-128.

Winther, J. G., Gerland, S., Ørbæk, J. B., Ivanov, B., Blanco, A., and Boike, J., 1999, Spectral reflectance of melting snow in a high Arctic watershed on Svalbard: some implications for optical satellite remote sensing studies: Hydrological Processes, v. 13, no. 12âĂŘ13, p. 2033-2049.

Interactive comment on The Cryosphere Discuss., 6, 3757, 2012.