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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 #1

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General comments:

This paper presents a method to compute glacier mass balance using a proxy of the equilibrium line computed from MODIS images and the mass balance gradient in both accumulation and ablation zones calculated from available fiel data. This method is developped for alpine type glaciers in the western North America.

The idea is good and the paper would deserve to be published, but I have a number of remarks and questions that have to be clarified before the paper can be accepted for publication.

1. The use of "regional snowline", "regional MODIS-derived ELAs", "regional ELAs", etc., and even the use of "snowline" appear to be abusive. The authors use the method

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they present based on MODIS images to extract not even the snowline, but the 20th percentile of elevation of snow-covered pixels (ZS(20)), and they claim that this ZS(20) metric yield to the best correlation with the surface mass balance for the glaciers where field data are available. In other words, they consider that this ZS(20) metric is representative of the snowline/equilibrium line. The studied glaciers are located in the mid-latitudes and must be temperate, so that we can assume that the end-of-summer snowline can be representative of the equilibrium line altitude, but what about the ZS(20) metric? However, because the authors have of all the necessary data, they can easily demonstrate this point.

For the eight glaciers used for validation, I would be interested to see:

- the scatter-plot between the snowline altitude computed from MODIS for each year and the ELA calculated from field data for the corresponding year.

- the scatter-plot between the ZS(20) metric computed from MODIS for each year and the ELA calculated from field data for the corresponding year.

Furthermore, I would be interested to understand why the authors use this ZS(20) metric instead of the ice/snow limit. Is it to avoid any confusion between the snowline and the firn line? In any case, further explanations have to be given.

Also, using the ice/snow limit would avoid the problems of glacier delineation errors, bare rock pixels in the accumulation zone, cloud-covered pixels and so on.

2. The authors mention that their methodology is an extension of Rabatel et al. (2005) approach, but it has almost nothing to see. Here, the authors compute the mass balance using a proxy of the ELA (the ZS(20) metric) and mass balance gradients for the accumulation and ablation zones. However, Rabatel et al. (2005, and see also, Rabatel et al., 2008, J. Glaciol. 54, 185, 307-314) compute the annual mass balance using the geoditic balance over a study period and the difference between the snowline altitude for each year of the study period with the altitude of the ELA0 (representative

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of a steady state of the glacier over the same period), this difference being multyplied by the mass balance gradient at the level of the ELA0. One question is: because the authors have the geodetic mass balance for many glaciers, computed by the difference between SRTM and SPOT5 DEMs, why don't them simply apply Rabatel et al. (2005, 2008) method? They can also apply their equation 1 in parallel and compare the results of the two approaches. And finally compare both approaches with annual mass balance computed from field data.

3. The authors mention time-series of "ZS(20) metric used as a proxy of ELA" and time-series of mass balance. They even comment the observed trends for some of the studied glaciers, but where are the data?? A table with the time-series and a graph presenting the temporal evolution have to be presented.

4. About the geodetic method to compute volume changes by differencing DEMs, what is the impact of the different resolution between SRTM and SPOT5? Did you consider the penetration of radar signal which may biased the results? See see Gardelle et al., 2012, J. Glaciol., 58, 419-422.

5. In a general way, more details have to be given about - the satellite data (dates used to finally compute the "ZS(20) metric used as a proxy of ELA" for the different glaciers. This can be given in a table). - the field data: a table giving for each glacier the monitoring period, number of ablation and accumulation measurements, ... - the error analysis for the computation of the "ZS(20) metric used as a proxy of ELA". With a pixel size of the images of 250 m, a DEM resolution of 90 m and considering the slope of the glacier at the level of the ELA, what is the error? - the accuracy of the method for the smallest glaciers for which the wide must be contained within two pixels of a MODIS image?

Specific comments:

Title:

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The title is not appropriate because the authors do not provide a “regional estimate of glacier mass change”. They present a method to compute the mass balance for single glaciers/ice caps, and validate the method. Thus, it has to be changed.

Abstract:

As mentionned in the general comments, the use of “regional snowline”, “regional MODIS-derived ELAs” is abusive and these terminologies have to be removed. This is the case for the whole paper.

P. 3758, L. 1. The method described does not allow to automatically extract the ELA but a proxy of the ELA.

1. Introduction

P. 3758, L. 25. How much represents “a handful of mass balance record”? A reference has to be cited here.

P. 3760, L. 12. Why do you use, here and everywhere in the draft the terminology: “index glacier site” or “index glacier mass balance sites”?? Also, you mention here seven glaciers, but then eight are presented (for example on the same page L. 25, in Fig. 6, ...)

Some bibliographical references are missing in this section:

- Demuth and Pietroniro, 1999. Geogr. Ann., 81A(4), 521-540.

- Jiskoot et al., 2009. Ann. Glaciol., 50, 133-143.

- Pelto, 2011, The Cryosphere, 5, 1127-1133.

2. Data and methods

P. 3761, L. 5-10. Are these data used to compute the mass balance gradient? Why don’t you use more rencent values?

P. 3761, L. 9, L. 15. BC, HEG have to be defined.

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P. 3761, L. 12-23. Finally, how many images have been used? In total, for each year?

P. 3761, L. 25. What is the “seasonnal ELA for a given glacierized region”?

P. 3762, L. 3. GTED, SRTM have to be defined.

P. 3762, L. 20. In equation 1,

- b0 is finally the “ZS(20) metric used as a proxy of ELA” and so could be mention as “ZS(20)”.

- b1 and b2 are respectively the mass balance gradient for the ablation and accumulation zone and could be mentionned as db/dzabl, and db/dzacc

- Zj < b0 represents the ablation zone and, Zj > b0 represents the accumulation zone, why don’t you just say that?

3. Results

P. 3764, L. 4-8. As mentionned in the “General comments”, a Table with the time series and a graph would be welcomed.

P. 3764, L. 8. The word “exist” should be removed.

P. 3764, L. 9. Table 3 should be Table 2 and conversely, Table 2 becomes Table 3 because the current Table 3 is cited before Table 2.

P. 3764, L. 16-17. Why do you consider the values for Emmons Glacier and Place Glacier, because you say on the same page L. 2-3 and you show it on Fig. 6, that for these two glaciers, the “ZS(20) metric used as a proxy of ELA” is not correlated with the mass balance?

4. Discussion

P. 3766, L. 11-13. About SRTM data, see Gardelle et al., 2012, J. Glaciol., 58, 419-422, about the impact of resolution and radar penetration on glacier elevation changes computed from DEM differencing. Indeed, due to radar penetration, the SRTM DEM

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may map a surface which is below the real surface, especially in accumulation areas, leading to biased estimate of glacier elevation changes.

Table 1 = For Lillooet site, mention the SPOT5 DEM of the 20 of August before the one of the 29.

Table 2, which should be Table 3 = Use the same units than in the text P. 3765, for coherency.

Table 3 should be Table 2

Figure 6 = Include the r^2 for each regression line.

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